SCIENTIFIC MEMOIRS,

SELECTED FROM

THE TRANSACTIONS OF

FOREIGN ACADEMIES OF SCIENCE AND LEARNED SOCIETIES,

AND FROM

FOREIGN JOURNALS.

EDITED BY

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VOL. III.

LONDON:

PRINTED BY RICHARD AND JOHN E. TAYLOR,
RED LION COURT, FLEET STREET.

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1843.

"Every translator ought to regard himself as a broker in the great intellectual traffic of the world, and to consider it his business to promote the barter of the produce of mind. For, whatever people may say of the inadequacy of translation, it is, and must ever be, one of the most important and meritorious occupations in the great commerce of the human race."—Goethe, *Kunst und Alterthum.*

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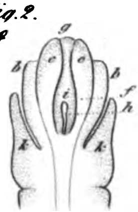

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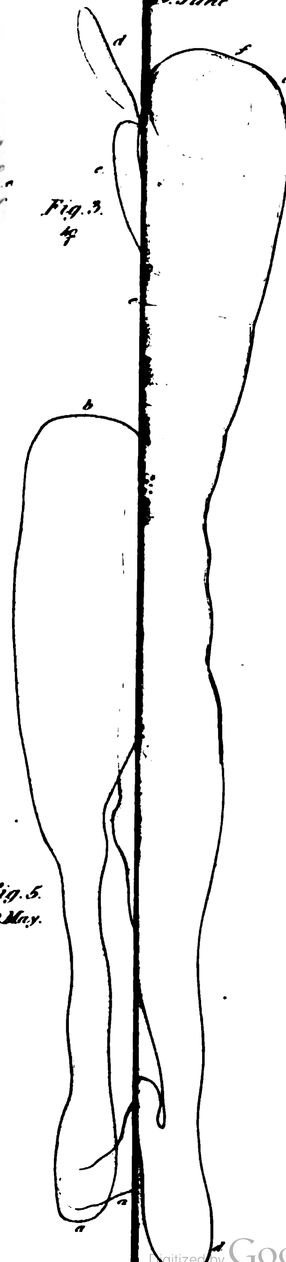

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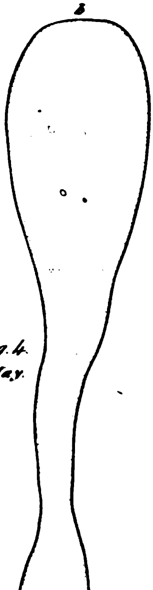

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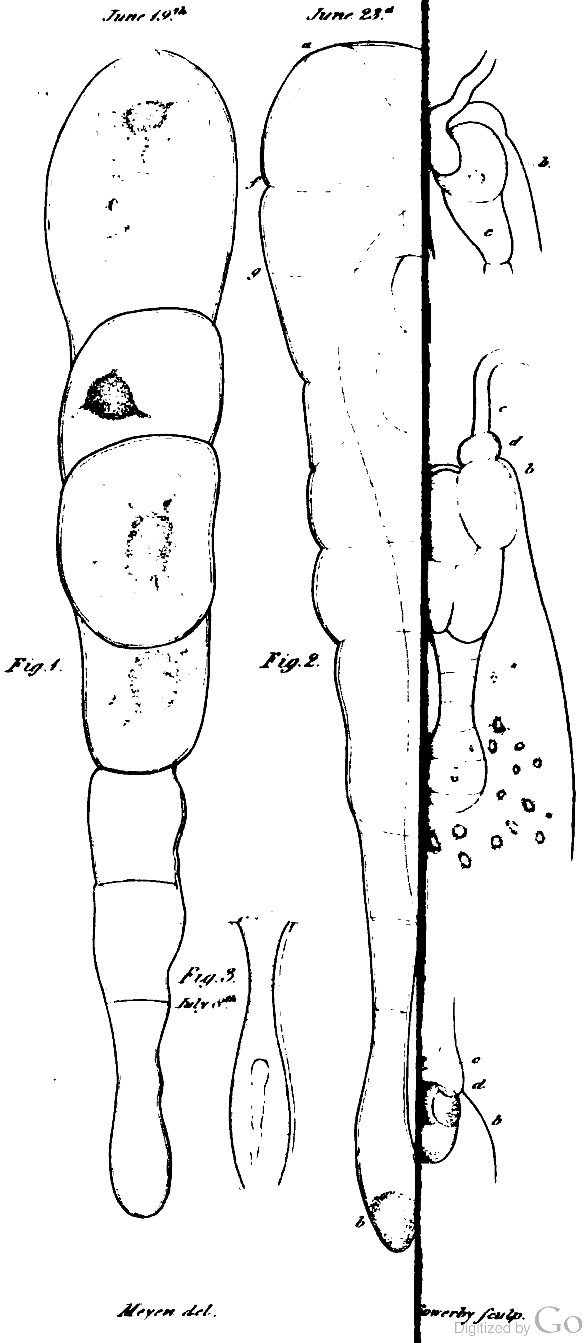

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SCIENTIFIC MEMOIRS.

VOL. III.—PART IX.

ARTICLE I.

A few words on the act of Impregnation and on Polyembryony in the higher Plants. By the late Dr. F. J. F. MEYEN, *Professor of Botany in the University of Berlin*.*

[Published as a separate Work. Berlin, 1840.]

PREFACE.

IT is but too well known that new views generally meet with a more or less warm reception, and frequently prevail for a length of time, even although the greatest difficulties are opposed to their establishment; this is so well known, that we need not be astonished that new hypotheses, which have lately been advanced on the formation of the embryo in plants, and which aim at no less than the overthrow of our long-established and well-grounded views respecting the sexes of plants, have been adopted in so many quarters with such great applause; I mean the hypotheses published by MM. Schleiden† and Endlicher‡ for the purpose of correcting the views hitherto entertained respecting the sexes and generation of plants.

Although these hypotheses are not entirely new, and the principal view has already been refuted in several older works, yet they did not cause such great sensation in former times; although even then their publication excited a controversy which was carried on with great vehemence.

I have already in another work§ treated at length of the new

* Translated and communicated by Mr. William Francis, A.L.S.

† Of which a translation was published in the Philosophical Magazine, vol. xii. p. 172.

‡ Sketch of a New Theory of the Production of Plants. Vienna, 1838.

§ New System of Vegetable Physiology. Berlin, 1839.

hypothesis on the sexes of plants, and have refuted it, at least in my opinion, by some decided proofs; but since I have learnt that some persons have misconceived my observations on the act of impregnation in plants, I take up the subject once more, and, bringing together the main results of my former inquiries, I shall combine them with my recent observations, and at the same time avail myself of the latest researches of other botanists.

J. MEYEN.

Berlin, October, 1839.

IT has been seen that the doctrine of the difference of sex in plants was so generally and firmly established by the production of hybrid plants, that a long period has elapsed before even a doubt has been ventured against the adoption of this difference of sexes similar to that of animals. The writings of Schelver and Henschel were, each in its peculiar way, to have laid open our error on this subject; but already a short period has pronounced judgement upon them, for they themselves are now but of historical interest, and precisely through the means of these writings a more accurate confirmation of the doctrine of the separate sexes of plants was brought about. From this period greater care was employed in observing the plastic process which occurs in the impregnation of plants, and we arrived, by the united discoveries of Amici, Brongniart, Robert Brown and many others, at the result that the fructifying substance in phænogamous plants is carried from the interior of the pollen-grain through a peculiar tube, the pollen-tube, into the cavity in which the production of the embryo, or the substratum for the future plant, takes place. An accurate historical exposition of the progress of these discoveries will be found in the last volume of my Vegetable Physiology, to which therefore I may refer.

In the year 1837, however, Professor Schleiden brought forward a new view on the subject of the sexes of plants; he worked under the special guidance of his uncle, our honoured and celebrated Professor Horkel, and how much of this new view belongs to the latter is, it is true, not known to me; but of this I am aware, that Professor Horkel was acquainted with the plastic processes in the impregnation of plants long before the appearance of the celebrated memoir of Brongniart, and, indeed, frequently more accurately than they are represented in that memoir. MM. Horkel and Schleiden believe that the passage of the pollen-tubes from the stigma to the ovulum, is the general process in the fructification in Phanerogamia; that one, seldom several, of these tubes creep through the intercellular passages of the nucleus, and that the tube which reaches the embryo-sac, presses it forward, drives this before it, indents it, and then appears like a cylindrical bag, forming the commencement of the embryo, which, therefore, is stated to be nothing more than a cell of the leaf-parenchyma ingrafted upon the summit of the axis. The embryo, according to this, therefore, is formed by the pollen-tube, and the indented embryo-sac; and in plants whose ovula contain several embryos, there would be just as many pollen-tubes present as embryos; whence there is derived the important conclusion that the two sexes of plants have been named altogether erroneously, each pollen-grain being the germ of a new individual; that the embryo-sac, on the contrary, should merely be considered as the male principle which only determines dynamically the organization of the material substratum.

There are still, it is true, at the present time, many cases which might be mentioned, where, notwithstanding all care employed, no pollen-tubes have hitherto been detected during the act of impregnation; but since, in by far the greater number of plants, the pollen-tubes are very easy to be observed on their way to impregnation, we may undoubtedly, with some justice, presuppose this process of the fructification as being generally valid as regards the Phanerogamia. But, on the other hand, those observations of M. Schleiden are not correct according to which the embryo-sac is said to be common to all Phanerogamia. I have become acquainted with a large series of cases in which the embryo-sacs are wholly wanting; in the Monocotyledons this seems to be very generally the case, but there is no want of examples even among the Dicotyledons; and recently MM. de Mirbel and Spach have proved the absence of the embryo-sac in a number of Grasses*. In those cases, therefore, in which no embryo-sac exists, the formation of the embryo must there *de seipso* occur in another manner than according to that described in the new doctrine; although, as we shall subsequently see, it is precisely these cases which apparently speak irrefutably in favour of M. Schleiden's view, that the germ for the new individual is always contained in the pollen-grain, or in the pollen-tube.

* *Notes p. s. à l'histoire de l'Embryogénie Végétale. Par MM. Mirbel et Spach*:—*Compt. Rend.* 1839, 18 *Mars.*

We will first make ourselves acquainted with the formation of the young embryo, or what takes place in the true process of impregnation in those cases where an actual embryo-sac is present, and where the union of the pollen-tube with the summit of the embryo-sac may be more or less easily traced.

In most cases where separate embryo-sacs occur, the formation of their first substratum takes place in the apex of the nucleus, and descends from thence towards its base or chalaza-end; in other cases, on the contrary, the embryo-sac is formed in the base of the nucleus, and then ascends upwards, as for instance in *Viscum album*; or, what far more frequently happens, the embryo-sac is formed out of the apex of the nucleus, and grows upwards to meet the entering pollen-tube. This growing to meet (towards) the penetrating pollen-tube of the embryo-sac, I regard as a phænomenon worthy of most especial attention; I observed it first in *Phaseolus*, where the embryo-sac originates in the apex of the nucleus, but is developed on the outside of it, in the interior of the cavity of the second tunic, and grows with its apex directly into the endostomium*; nay, I even saw in this plant the remarkable phænomenon, that the apex of the embryo-sac grew a couple of lines in length out of the apertures of the tunics, because the formation of the pollen-tubes, and their entrance to the ovula in forced plants, had been retarded by continuous cold weather. I have for three years observed in *Alsine media* another phænomenon connected with this, to which I would likewise draw attention. In the first months of spring, immediately on the disappearance of the snow, this little plant usually flowers more or less frequently; however, it seldom exhibits embryos before the end of April, as the formation of pollen-tubes is prevented by the low temperature, and thus the impregnation remains unaccomplished. Now, at this time it is very frequently seen that the apex of the nucleus grows far beyond the apertures of the tunics, and forms itself into a large somewhat cup-shaped body, which becomes nearly as big as the entire circumference of the ovule, but subsequently falls off as far as the endostomium. In the month of May, on the contrary, I no longer found the above excrescence of the apex of the nucleus. We have likewise learnt from the memoir of Mr. Griffith† that in *Santalum al-*

* See Meyen's Vegetable Physiology, vol. iii. pl. xv. figs. 9*, 10, 12.
† Transactions of the Linn. Soc. vol. xviii. pl. i. p. 71, &c.

bum a naked nucleus occurs, out of whose apex the embryo-sac grows forth quite freely, and advances to meet the pollen-tube; here, then, that happens quite regularly which I have observed merely exceptionally in *Phaseolus*, namely, that the apex of the embryo-sac serves immediately as a micropyle for the reception of the polien-tube.

But this union of the pollen-tube with the apex of the embryo-sac is the very act of impregnation; and I have observed it under the following perceptible phænomena. There in some cases originates after the union of the pollen-tube with the apex of the embryo-sac, probably by the reciprocal dynamical action, a small protuberance at the place of the union, which grows larger and larger, fills with a turbid, slimy substance, and separating itself from the pollen-tube, becomes a vesicle, which very soon expands in length, and grows deeply into the embryo-sac. I have called this vesicle the *germ-vesicle*; it is the first product resulting from the sexual action which the pollen-tube exerts on the apex of the embryo-sac, and we shall subsequently see how the substratum for the embryo originates from this germ-vesicle. The most usual case which we observe in the process of impregnation, is, however, that where the germ-vesicle originates in the interior of the embryo-sac; and this again takes place in two different kinds of union of the pollen-tube with the embryo-sac; either the pollen-tube coheres directly by the apex of its extremity with the apex of the embryo-sac, or, which indeed is the more rare case, the extremity of the pollen-tube applies itself laterally to the apex of the embryo-sac. In this latter case the origin of the germ-vesicle is usually very easy to observe, and, at the same time, these cases are the most decided proofs against the doctrine that the embryo-sac is indented by the entering pollen-tube in order to help in forming the embryo. In some species of the genus *Mesembryanthemum* I have found this lateral union of the pollen-tube with the embryo-sac quite constant, and have already published some drawings of it*; but recently I have observed this process very minutely in *Mesembryanthemum pomeridianum*, and have made from it the accompanying drawings (Plate II. figs. 7–14). In both cases of the union of the pollen-tube with the embryo-sac, a perfect cohesion first takes place, quite similar to the conjugation of some *Conferva*, and soon

* Meyen's Vegetable Physiology, vol. iii. pl. xiii. figs. 46 and 47.

after this the germ-vesicle originates in the interior of the apex of the embryo-sac, exactly at the place where its cohesion with the pollen-tube occurs. *The union or cohesion is here the act of impregnation,* and the origin of the germ-vesicle the first product of impregnation; the germ-vesicle is, indeed, not the result of a violent irruption of the pollen-tube into the embryo-sac, whose membrane would thereby be indented; but it is formed of the substance of the two cohering membranes, viz. that of the end of the pollen-tube, and of the apex of the embryo-sac. In the figures 15, 16, 18, Plate II. this part is represented in a lateral view of *Mesembryanthemum linguæforme* in *d d d* (the end of the pollen-tube [*c c c*] swells here, quite constantly, to a very enormous size, which, however, happens only after effected copulation), and in fig. 17 such a position of the embryo-sac is represented as that the obliterated circular place (*d*) may be seen, from whence the formation of the germ-vesicle resulted, in the direction of the cavity of the embryo-sac. The germ-vesicle (*e*) is here already very large; it is expanded downwards, and at its extremity the substratum [groundwork] for the embryo is already present. During the formation of the germ-vesicle this circular spot (*d*, fig. 17) is open, as the substance of the two membranes there obliterated expands, in consequence of the act of impregnation, to form the germ-vesicle, which in its growth is nourished by the substance in the interior of the apex of the pollen-tube, as well as by the substance in the interior of the embryo-sac; and from the union of these two substances, and their innate formative properties, results the new product, namely, the base for the future embryo. There is no ejaculation of the fructifying contents of the pollen-tube into the cavity of the embryo-sac, but the fructifying substance passes in very slight quantity only into the forming germ-vesicle, soon after which this free communication of the originated germ-vesicle with the pollen-tube ceases by a constriction resulting from the formation of a diagonal septum. Upon this, in most cases, the end of the pollen-tube shrivels, and very soon its former communication with the embryo-sac ceases entirely. In other cases, however, the end of the pollen-tube continues for some length of time adhering to the embryo-sac, and then even undergoes different variations in form; thus, for instance, it swells vesicularly, as in *Mesembryanthemum linguæforme* (*Hort. Bot. Berol.*), where this protuberance remains within the apertures of the nucleus and the tunics; or as in *Ceratophyllum*, where the vesicular swelling projects out of the apex of the ovule, and exhibits here and there other cylindrical excrescences, of which I have seen three, and even four. Although it is almost a rule in the genus *Mesembryanthemum*, that the end of the pollen-tube applies itself, in the process of impregnation, laterally to the apex of the embryo-sac, yet it even here sometimes occurs that the germinal vesicle is formed at the apex of the embryo-sac, and then grows into it, as, for instance, takes place regularly in *Alsine media*. Accordingly these differences appear not to be very essential; and if we see in the conjugation of the *Conferva conjugatæ*, some *Mucedines* [*Fadenpilze*], and other *Algæ*, an analogue to the act of impregnation in the higher plants, in which we most undoubtedly are justified; so in like manner do we find in these exactly the same differences, which, however, are here entirely unessential. In *Conferva bipunctata* the well-known mode of conjugation usually occurs, in consequence of which the spore is formed in the interior of one of the conjugated joints; at times, however, we meet with entire masses of this *Conferva*, in which the spore constantly arrives at development in the obliterated warts of fructification, quite as in *Syzygites*, and as we have observed it in *Closterium*, &c.

Now after the germ-vesicle has been formed in consequence of the act of impregnation, it proceeds in its development, which again varies in the different genera and species of plants. In general the germ-vesicle expands in length, growing into the depth of the embryo-sac, and usually represents a cylindrical tube [*Schlauch*], from whose end a simple globular cell then separates [*abschnürt*], which is the young embryo. The other remaining part of the cylindrical tube forms the funiculus of the embryo, and adopts in different plants very various forms and structure; frequently it remains like a simple string of rows of cells; frequently individual parts swell to a greater or less size in a vesicular form*, and often the funiculus of the embryo is transformed into one thick cellular cord. The various drawings which I have given in the work above cited afford sufficient explanation relative to the formation of the rudiment of the embryo and its separation from the funiculus; and,

* See the drawings of *Capsella Bursa-pastoris* and *Alsine media* in my Vegetable Physiology, vol. iii. pl. xiii.

moreover, in several of the figures of *Mesembryanthemum* represented in Plate II., the separation of the young embryo from the funiculus, which originated from the prolongation of the germ-vesicle, will be distinctly seen. In figs. 11 and 12 the end of the funiculus, which has been transformed into the embryo, is designated by *d d*; in fig. 17 the constriction of the embryo *f* is still more evident; and in figs. 15 and 16 the young embryo is already quite separated from the funiculus, but still connected with it. After the formation of the young embryo has once taken place, the function of the funiculus seems no longer to be of any considerable importance, for we almost always find it die off very soon on the further development of the embryo, and frequently disappear without leaving a trace, which in some plants occurs earlier, in others at a later period.

Thus, then, we have arrived at the interesting result, that the embryo, in all those cases where it is formed in the interior of the embryo-sac (which undoubtedly occurs most frequently), *does not proceed direct from the pollen-tube*, but is first formed at the extremity of an organ, subsequently serving as its funiculus, which originates from the prolongation and further development of the germ-vesicle. We have further arrived at the result, *that the embryo, on its first appearance, is nothing more than a simple globular cell, and thus presents the form and structure of the simplest plant**. This state of the vegetable embryo I term its first stage of development, of which I distinguish principally three. The simple globular cell is developed from within outwards, into a cellular mass, which at last adopts in most plants a regular globular form; and this is the state which represents the second stage of development; whilst the third commences with a lengthening of this globe, and the production of the cotyledons. It is especially interesting to find these various stages of development of the vegetable embryo repeated in the different large divisions of plants, corresponding to the degree of their development; thus I have indicated the embryo of the *Orchideæ* as one which persists at the second stage of development.

From what has hitherto been said on the act of impregnation in plants, it without doubt very clearly follows that we have met with no actual facts which justify us, much less compel us,

* See my Report on Physiological Botany for 1837, p. 151; or the English Translation by Mr. W. Francis. London, 1839: R. and J. E. Taylor, pp. 120–128.

to give up our old and so generally adopted ideas on the sexes of plants,—those, namely, according to which the fructifying principle resides in the contents of the pollen-grains, and the anthers are therefore to be compared to the male sexual organs of animals. With these views, and the facts above communicated on the first formation of the embryo, the origin of hybrids among plants will appear to us quite intelligible.

There is, moreover, another fact which perhaps speaks quite as decidedly against the view of M. Schleiden and his followers, in addition to the relation of hybrids, though I must confess that, unfortunately, I myself have never noticed this phænomenon; I mean here Mr. R. Brown's observation of ramified embryonal-funiculi, where the extremities of the branches afforded the rudiments of embryos, and to which I shall subsequently again revert.

Many years ago M. de Mirbel* advanced the position that impregnation in plants (at least in the higher orders) is nothing more than the grafting of the male cell upon the female. I believe I may conclude from this remarkable expression, that De Mirbel already at that time (1833) had observed the union of the pollen-tube with the embryo-sac. Now, the comparison of this union with a graft is not indeed quite correct, for that which is grafted, as is well known, continues its further growth without considerable change—in general without any change at all; but this is what De Mirbel by no means intended by this expression, for he and Mr. Robert Brown are both defenders of our old views on the sexes of plants.

We now come to the consideration of the formation of the embryo in those cases where it is formed in the interior of the cavity of the nucleus without a separate (special) embryo-sac; this occurs most frequently in Monocotyledons, and also not very rarely in Dicotyledons. M. Schleiden at the commencement mentioned the process of impregnation in the *Orchideæ* principally in proof of his new view, namely, that the germ is carried to the embryo by the pollen-tube into the ovule, where it then undergoes further development; however, although the process is somewhat different in this case, on account of the want of an embryo-sac, yet the impregnation agrees in the main with the act of fructification in the cases already cited with an embryo-

* *Complém. des Obs. sur la Marchantia polymorpha*, p. 51.

sac. There is no embryo-sac in the *Orchideæ*; but the nucleus is converted about the time of impregnation into a thin membrane, which occupies as it were the place of the embryo-sac, although but for a short time, as this membrane is very soon re-absorbed, and the embryo is then formed within the second tunic. As soon as the pollen-tube in the *Orchideæ* has penetrated into the summit of the nucleus-sac, and there comes into contact with the contained fluid, its summit swells to a more globular form, and out of this originates the germ-vesicle, which very soon separates by constriction from the cavity of the pollen-tube. This germ-vesicle expands, as in the other cases, in length, consequently descends deeper into the cavity of the nucleus, and out of the end of this much denser new cellular formation originates the young embryo, and not, as M. Schleiden has stated, immediately out of the summit of the pollen-tube*. Still more similar to the former cases is the formation of the embryo in *Capsella Bursa-pastoris*, in *Draba verna*, &c., where a funiculus of considerable length always makes its appearance. In the former plant I was not able, from the firmness of the tunics at their aperture, to lay open the germ-vesicle at its first formation, but only after it had become converted into a small cylindrical bag, which, however, was four and even five times as broad as the pollen-tube still adhering to it, but separated from it by a septum. This new cylindrical bag grows longer and longer, swells more and more to a globular form at its end connected with the pollen-tube, divides below this swelling into a series of cells, and at the other end, which projects free into the cavity of the nucleus, is formed the globular cell, which is the first substratum for the embryo. A long series of drawings, which represent the act of impregnation, and the formation of the embryo in *Capsella Bursa-pastoris*, I have given in plate xiii. of my Vegetable Physiology, to which work I may here refer. In *Draba verna* the funiculus is likewise very long, and the young embryo first originates deep in the nucleus-cavity: since the funiculus is here almost perfectly cylindrical, and occurs merely provided with some diagonal walls, it appears like an immediate continuation of the pollen-tube, which, however, at the time of impregnation,

* See my illustrations on this subject in the Vegetable Physiology, vol. iii. pl. xiii. figs. 34, 35 and 36, and pl. xv. fig. 23.

is scarcely half as broad as the funiculus which is formed subsequently. As, moreover, in *Draba verna* there is no embryo-sac, it is evident that the funiculus must appear like a direct continuation of the pollen-tube, since, as in many of the preceding cases, it persists long after impregnation.

Less clearly than in the cases hitherto mentioned, and apparently speaking strongly in favour of M. Schleiden's new theory, do the impregnation and the formation of the embryo take place in the *Liliaceæ* and similar families. In these plants there is also no embryo-sac; the pollen-tube penetrates the apertures of the two tunics, and passes through the loose cellular mass of the summit of the nucleus down into the cavity which has been formed in the interior of the nucleus, and occupies the place of the embryo-sac in other plants. It is the apex of the pollen-tube which has penetrated into the upper end of the cavity of the nucleus which here swells in a greater or less degree to a clavate or globular form, at times attains a very considerable size, then divides itself by septa into several large cells, constricts itself from the still adhering pollen-tube, and is finally transformed by the formation of new cells in its interior into the embryo. Here likewise there are traces of a funiculus, which, however, does not separate so decidedly from the actual embryo as has been seen in the previous cases. On plate xv. of my Physiology of Plants there is in figs. 1—8 a series of drawings of the phænomena of impregnation here mentioned, and more particularly of the Crown imperial, &c. In this case also it is not the apex of the pollen-tube, out of which the embryo is formed; but this apex serves merely as the substratum to the formation which represents the young embryo, and which results in the first instance from the secret action between the contents of the pollen-tube and the contents of the cavity of the nucleus. The apex of the pollen-tube will certainly not be transformed to an embryo outside of the cavity destined for the formation of the embryo; and the case of an extra-nucleal pregnancy, which is said to have been met with in the genus *Orchis*, appears to me to be founded on an error. In thus regarding it I am the more justified, as I have found that M. Schleiden, the author of that communication, has not quite correctly conceived the formation of the embryo in the above genus; the drawings which he has given in illustration are all of a much later period, and he was thus sometimes misled by those observations to consider the fully developed funiculus of the embryo as the remnant of the pollen-tube.

MM. de Mirbel and Spach* have also come forward with a very interesting memoir against the new doctrine of M. Schleiden on the sexes of plants; they have made their observations on the formation of the embryo in the Grasses, and particularly in Maize, and have discovered some hitherto unknown and very important phænomena, which serve still further to show what an exceedingly manifold diversity there may be in the impregnation and formation of the embryo in plants. They discovered that in the Maize a separate transparent bag is formed within the small cavity in the summit of the nucleus, which is transformed into the embryo; this bag or utricle is, according to Brongniart, the germ-vesicle, and was regarded by M. Schleiden as the end of the pollen-tube. But MM. de Mirbel and Spach found that this bag originates directly from the cambium, *i. e.* from the formative sap of the cavity of the nucleus, and indeed even previous to the action of the pollen, and called it *l'utricule primordiale.* How impregnation takes place in the Maize and in some other tropical Grasses, they have not been able to state; but they have endeavoured to demonstrate that the *primordial utricle* is neither the apex of the pollen-tube, nor can the name of embryo-sac be applied to it. At the inferior extremity of this utricle the above-mentioned observers found a small group of minute ovate cells, and they are inclined to believe that these small cells are nothing more than abortive *primordial utricles*; but they do not mention what further becomes of them. At the period of the formation of the embryo in the seeds of *Maïs* the primitive utricle first expands in length, and especially the extremity situated toward the micropyle expands into a long and slender bag. In the interior of the primitive utricle cellular tissue is formed, and from this originates the embryo, whilst its long-extended apex is converted into the funiculus, and does not disappear till after the perfect development of the scutellum [*Schildchen*]. Quite unexpected as these communications on the formation of the embryo in *Maïs*, &c. were to me, and undoubtedly to many other observers, yet from my recent researches relative to this subject on the

* Notes pour servir à l'Histoire de l'Embryogénie Végétale:—Compt. Rend. 18 Mars, 1839.

above-mentioned plants, I must confirm them; and I am at the same time so fortunate as to be able to add to them something new. I too have never been able to observe a connexion of the primordial utricle with the pollen-tube; I moreover found the apex of the primordial utricle, when it had been extricated and prepared without injury, always perfectly closed. The primordial utricle is converted into the embryo, and the cyme of ovate cellules which originates at the other extremity of the primitive utricle, increases more and more, and becomes the scutellum. I did, it is true, state formerly, trusting to the statements of MM. Horkel and Schleiden, that the scutellum of Grasses was the albuminous body which is situated immediately next the embryo; but I have now clearly observed that this albuminous body takes its commencement from the inferior end of the primordial utricle, grows in the form of a leaf contracting on both sides over the entire embryo, and forms the scutellum; out of the small inferior fissure of the scutellum the radicular end still projects for some time, and shows the half-withered but very large funiculus. I have, at least, not been able to detect in *Maïs* and *Sorghum* any separate embryo-sac, inclosing the embryo and its albuminous body; yet I have observed not very rarely in the former plant considerably large pieces of an exceedingly delicate and slightly firm membrane, which clothed the cavity of the nucleus more towards the micropyle end; usually, however, this wall is merely clothed with a homogeneous mucus. How impregnation takes place in *Maïs*, and in similar Grasses, has remained to me quite as unknown as to my predecessors; and the observation of this act will probably long remain undisclosed to us, from the peculiar structure of the ovarium; yet so much may here be adopted as certain, *that it is not the pollen-tube which is in this case transformed into the embryo*; for the tube or bag out of which the embryo is formed appears long before the action of the pollen can take place. MM. de Mirbel and Spach have enumerated several other Grasses where the formation of the embryo is said to be quite similar; and whether it is not of a similar kind in other plants, is as yet unknown.

It results, therefore, from these observations on the formation of the embryo in *Maïs* (although these are still imperfect, from the act of impregnation remaining unknown), that the new views on the sexes of plants cannot be the correct ones.

It was my intention in this little work only to bring forward facts, and to point to their results in such a manner that it might be perceived that we are not compelled to advance any new view on the functions of the sexual organs in plants, but that those prevailing are correct. For this reason I will here merely give some other indications, which will likewise, more or less decidedly, oblige us to adhere to our old views.

According to the new theory, each pollen-grain would be the germ of a new individual; but we likewise know at present that it is the pollen-tube, at least in most cases, from whose action the production of the embryo results; and we have also since come to know how various the number of apertures is through which the pollen-grains can discharge the fovilla, and that out of one and the same pollen-grain as many pollen-tubes may be formed as there are apertures. Nay, we even know that the pollen-tubes may themselves be ramified, and that even from one and the same pollen-grain several pollen-tubes may penetrate into the canal of the style; there is therefore nothing opposed to our concluding, that not only each pollen-tube of a pollen-grain, but that even each branch of a pollen-tube which finds its way to the ovule, is capable of causing the production of an embryo; and it follows, therefore, that the number of germs common to a pollen-grain must be quite undetermined; for in the first place this depends on the number of tubes which are formed, and secondly, on their perfectly accidental ramification. But of these distinct germs, which consequently must frequently be present in great number in one and the same pollen-grain, there is nothing to be seen; the fovilla which penetrates into the pollen-tube exhibits, as is well known, no trace of separate germs for the young embryos; and it is even dissolved, at the apex of the pollen-tube with which the impregnation is effected, into a homogeneous substance; so that no room exists even for imagining that the spermatozoa or spermatic corpuscles can represent the germs.

Mr. Robert Brown concludes, from the gradual disappearance of the fovilla in the pollen-tube, that it is employed in nourishing and forming the pollen-tube. Highly probable as this supposition is, yet it cannot thence be concluded that the fovilla exists merely for the nutrition and formation of the pollen-tube. We observe in many cases so very distinctly that the pollen-tube first occurs in great vigour, *i. e.* with a thicker and firmer wall, when it is either nourished by the mucus of the conducting cellular tissue, or when it merely touches the apertures of the tunics, and proceeds within these to the summit of the nucleus.

I find a very important proof of the correctness of our older views respecting the sexes of plants in the recent discoveries that the contents of the anthers in the lower plants contain a substance which exhibits a very great similarity to the spermatic fluid of animals; I mean the discoveries of the occurrence of spermatozoa in the anthers of Mosses and *Hepaticæ*, and in the *Charæ*. Whether these forms, to which I have applied the name of vegetable spermatozoa, be considered as actual animals, as intestinal worms, or anything else, it matters not. I merely wish them to be regarded as identical with what have been asserted to be the spermatozoa of animals. Now I believe that it is more reasonable to conclude, from the similar occurrence of these substances, that they possess like functions in plants and in animals, than that it is necessary to go into lengthened proofs. But if it be admitted that the fovilla in the Cryptogamia is really the fertilizing male substance—which indeed cannot be in the least doubted, from the relation which the anthers of those Cryptogamia bear to the female organs,— we shall find ourselves also compelled to regard the fovilla in the interior of the pollen-grain as the fertilizing male substance. The controversy which has been carried on respecting the nature of the self-active molecules in the interior of the fovilla is well known to the readers of this little work; but I consider it no longer necessary to reply to the statement of some writers who believe that I have mistaken amylum-granules for those self-active molecules which I compare in their signification to the spermatozoa of animals. But since these remarkably-formed spermatozoa have been detected in so many of the lower plants, similar even in the highest degree to those in the sperm of animals, the question may undoubtedly be started, how it can be explained that the spermatozoa in the higher and lower plants are so enormously different, that, with the exception of their independent motion, no other similarity can be detected between them. This independent movement of the spermatic molecules in the higher plants I have recently, again, been frequently observing, in order to investigate its cause with the magnifying powers at present at our disposal; but I have unfortunately not noticed any distinct organs of motion on these molecules. I have, however, nevertheless observed the motion of the spermatic molecules in many plants in such manner, that it appeared to me, and likewise to others who saw their motions, no longer so very improbable that these forms likewise are provided with some distinct organ of motion, which is however so delicate that it is still imperceptible with our present instruments. When, for instance, the spermatozoa of the Mosses are observed lying together in large masses, peculiar jumping motions of the extremities of their bodies are very often visible, which can only be produced by the unrolling of their proboscis; and just such frequent and remarkably quick movements I have not unfrequently noticed in the spermatic molecules of the higher plants, and most especially evident in the *Cucurbitaceæ*. It is not difficult to arrive at such a supposition; for when Schmidel discovered the spermatozoa of *Jungermannia pusilla*, and afterwards also when those of some species of *Sphagnum* were detected, their proboscis was completely overlooked, from the lowness of the magnifying power.

It might also be well to bear in mind that the pollen of many plants, in large masses, evinces that remarkable odour which is so peculiar to animal sperm; so that we should thence likewise be led to admit that the fovilla of plants corresponds to the sperm of animals.

We will now, lastly, consider some phænomena attending the production of hybrids, which certainly can only be satisfactorily explained according to the views that have hitherto been entertained upon the sexes of plants. It is well known, from the multiplying of plants by buds, that the nutritive substance which is allowed to pass to an ingrafted or transferred bud, does little, or in most cases not at all, alter its specific nature. But if we should conclude, according to the new view, that in the production of hybrids the act of impregnation is to be compared to a grafting, the germ of the embryo being merely transferred into the tunics of the blossoms of another plant, and requiring there only to be nourished,—the important results which C. F. Gærtner arrived at on the relation [*verhalten*] of hybrid plants cannot be brought into harmony with this view. Above all, the new theory is opposed by the observed fact that hybrids very usually exhibit a tendency gradually to pass over again into the female plant (*i. e.* that which, according to the old view, is called the mother plant!), a phænomenon which has long been known to attentive horticulturists and florists. M. DeCandolle has already advanced this fact*, in refutation of the views of those who believe that the embryo proceeds from the seminal germs of Gleichen (*i. e.* the seminal animals, or spermatozoa of the present day!), a view which it is well known was rather prevalent about the middle of the last century, and from which the new view of M. Schleiden differs, properly speaking, only in his allowing those seminal germs of Gleichen to be carried by the pollen-tubes into the tunics. If to this it be replied, that the production of hybrids can be explained as well according to the new view as according to the older theory of generation, the embryo-sac and its contents exercising the male fructifying action, —then this latter supposition must not only be repelled as one perfectly gratuitous, but must also be characterized as one which is destitute of every degree of probability. Is there the slightest analogy in favour of admitting that the young embryo penetrates into the interior of the male impregnating organ, and is there developed? And how then will M. Schleiden explain, according to this, the cases mentioned by him, where the embryo in an Orchideous plant is stated to have been formed on the outside of the cavity of the nucleus? For what is figured as such is not a pollen-tube with a swelled extremity, but it is the young embryo with its funiculus exserted, &c.

The greater part of that which I have here brought forward against the view of M. Schleiden, applies also to the new theory which M. Endlicher † advanced last year on the generation of plants. He likewise felt the insufficiency of Schleiden's explanation relative to the male and fertilizing principle, and endeavoured, therefore, to perfect that theory by a different assumption.

M. Endlicher, too, compares the anthers of the higher plants with the ovarium of animals; and finds the male fertilizing organ in the glands of the stigma, which must not be regarded merely as a conducting organ, but one whose peculiar secretion excites the pollen-grain to that activity which renders it capable of penetrating into the tissue of the pistil, and of arriving in the covering of the germ. But without any long consideration it will be clearly seen that this view, viz. that the stigmatic fluid is the fertilizing male substance, is purely arbitrary; and since

* *Physiologie Végétale*, ii. p. 546.

† *Grundzüge einer neuen Theorie der Pflanzenzeugung.* Wien, 1838.

M. Endlicher himself confesses that the anthers in Cryptogamia are the true male organs, I consider that it at once results from the similarity which the structure of the female organs in the Mosses, *Hepaticæ* and *Charæ*, evinces to the pistil in the Phanerogamia, that too high an importance has been here assigned to the stigmatic fluid. The pistil of the Mosses has, with respect to form, the most surprising similarity with the normal pistils of the Phanerogamia;—ovarium, style, and stigma may be distinguished on it. In the *Charæ* the female organ exhibits only the ovarium, in which a single spore is situated, and immediately upon this is situated the stigma, the folds of which at the time of impregnation bend somewhat from each other, and thus leave a canal through which the fertilizing substance arrives immediately at the apex of the spore. In Mosses the anthers are mostly situated very close to the aperture of the pistil; and when they open, a portion of the seminal fluid must necessarily find its way into the cavity of the pistil; and it is almost exactly the same in a number of *Hepaticæ*. Impregnation, however, can here only take place by the dynamic action of the seminal fluid on that organ, which, generally of the form of a small globe, is situated on the base of the ovarium, and is subsequently developed in the seed-vessel. The spores are only developed in these organs some months after impregnation has occurred, and a direct contact of the fovilla with the spores never takes place in this case; which, however, according to M. Endlicher's view, should take place, in order that the spores might become capable of further development.

But, properly speaking, M. Endlicher has advanced nothing further against the theory of generation hitherto entertained, than that the activity which the anthers exert in impregnation present not the least analogy with any one of the functions of the male sexual parts, in the various classes of the animal kingdom. I believe, however, that this objection is nevertheless not of such great importance. We find, for instance, in the various classes of animals, that the sexual organs are very differently circumstanced, but that the ma*l*e organs are always so formed that in order to attain their end, viz. the performance of impregnation, they correspond to the female. The male sexual organ is in animals sometimes very long, sometimes wanting; and it is undoubtedly just the same with the pollen-tubes, which we regard as the organ which conveys the fertilizing substance to the place where the embryo is subsequently formed. It is not even proved that impregnation in the higher plants is always effected by a pollen-tube. I myself have related cases where I had never observed the pollen-tube in young impregnated ovules; and indeed in cases which I have so frequently examined, that I am actually inclined to doubt that impregnation is there effected by a penetrating pollen-tube. But the discoveries of MM. de Mirbel and Spach on the ovules of the fruit of *Maïs*, are still more striking; for at all events impregnation cannot, in this instance, be effected by a penetrating pollen-tube, but is perhaps to be explained by a dynamic action, which the fovilla may exert on the primitive utricle, which is transformed into the embryo.

Just as this little work was going to press, a memoir by M. Bernhardi appeared*, entitled, "On the formation of seed without previous impregnation," which also treats of the subject here under consideration. M. Bernhardi believes that an adjustment of the different views which at present prevail on the sexes of plants, depends principally on three points: 1. on continued direct observations of the changes that take place in the vegetable ovula after the pollen-dust has been shed [*nach der Bestäubung*]; 2. on the determination of several phænomena which the production of hybrids presents; and 3. on the confirmation or refutation of observations, according to which numerous plants are capable, under favourable circumstances, of producing germinating seed even without previous impregnation. Now the recent memoir of M. Bernhardi treats especially of this last point; and it is well known that at various times new voices have continually been raised in favour of it. In the first place, M. Bernhardi enumerates several cases of animal propagation as to which a conviction has been come to that no previous impregnation had taken place; and if anything of this kind can occur in animals (for the incorrectness of those statements has not yet been proved), it would be conceivable in plants also. In relation to this the celebrated experiments of Spallanzani are cited, and all the observations of others referred to, who have sometimes disputed the results of those experiments, sometimes confirmed them, but more especially the observations on hemp plants, which have recently (1828) been repeated by Girou de Buzareingues, and still earlier also (1811-

* *Algemeine Gartenzeitung von* 1839, *vom* 12 *October*.

1816) by M. Bernhardi himself, and which are said entirely to confirm those results of Spallanzani. M. Bernhardi made these observations for six consecutive years, and with constant regard to all measures of precaution, has every year obtained mature seed, without the possibility of an impregnation by male flowers having preceded. There is evidently as yet nothing positive that can be objected to this, or to several other observations of the same kind. I myself have hitherto left them out of consideration, as they appeared to me not at all to be relied upon; nay, I confess that I have even thought myself justified in looking upon them as entirely incorrect, although I was well aware that it is not easy actually to support this opinion by proofs, since negative results can only be employed with the greatest precaution against positive observations. But after all that we have observed up to the present time, of the processes under which impregnation in plants takes place; after all this, it seems scarcely credible to me that a formation of embryo can occur in the higher plants without previous sexual action of the pollen. The work of M. Bernhardi has however certainly appeared very opportunely, for it will occasion the repetition of similar observations on the formation of seed without previous impregnation, and the examination of the young seeds thus produced, as to the formation of their embryo. It is but too frequent that hermaphrodite flowers occur in monœcious as well as in diœcious plants, and that in this case the pollen, at times, occurs in parts of the flower where it had scarcely been suspected; and thus actual impregnation may have ensued where it was supposed to have been perfectly guarded against.

Remarkably different as the plastic phænomena in the process of impregnation of various plants appear, equally important differences are manifested in their polyembryony, which in some plants occurs pretty regularly, in others, on the contrary, more or less accidentally. The occurrence of several embryos had, it is true, been observed long ago in a number of plants of the most varied kinds; but it is only within a very recent period that attempts have been made to find out the essential differences under which this takes place. Most usually several embryos appear at the same time in the interior of one and the same embryonal sac, as is frequently the case in *Citrus*; often the number of the young embryos is, in such cases, very considerable; thus I have seen six and even seven in *Citrus decumana*, two to three in species of *Cistus*, and even two, four, six to eight in *Helianthemum grandiflorum*; but it is very seldom that more than a *single one* of these young embryos attain to perfect development. Usually only one embryo is developed, and the adjacent ones remain undeveloped, or at least they do not arrive at perfect development. In *Hemerocallis cærulea*, where polyembrony was discovered by Robert Brown, I have nevertheless observed in nearly mature seeds six, and even seven embryos in a nearly equal stage of development. In another work I have given representations* of the occurrence of the young embryos in the interior of one and the same embryo-sac of *Helianthemum grandiflorum*; and it is precisely such cases that Schleiden regards as the most certain proofs in favour of his new view on the sexes of plants, having made the observation that exactly as many incipient embryos or germ-vesicles are formed in the embryo-sac as pollen-tubes united with it.

But it is quite otherwise with regard to the polyembryony of the *Coniferæ* and *Cycadeæ*; here the greater number of the embryos are not left to mere accident, as in the preceding cases, but their number is in exact relation to the peculiar structure which is developed in the embryo-sac. The plurality of embryos in *Cycas circinalis* was discovered by M. de Mirbel †; Robert Brown ‡ subsequently asserted that it even appeared to be normal in the *Cycadeæ*; and lastly, he made known the interesting discovery § that the plurality of embryos likewise occurs regularly in the *Coniferæ*. I have had no opportunity myself to examine the young ovula of the *Cycadeæ*, but M. de Mirbel found in *Cycas circinalis* that from four to five abortive embryos were present together with the developed embryo, and Robert Brown found it to be quite similar in the true *Coniferæ*. He observed that at first a peculiar solid body is formed in the interior of the nucleus in the impregnated ovule, which he takes to be albumen; in this body there subsequently originate several semicylindrical corpuscles, three to six in number, which are arranged in a circle

* *Pflanzen-Physiologie*, iii.. Pl. xiv. figs. 23 and 24.
† *Ann. du Mus.* t. xvi. p. 485.
‡ King, Voyages, &c., App. Botany, p. 552.
§ Fourth Report, &c. 1834, p. 596.

near the apex, and appear to differ from the mass of the albumen in colour as well as consistence. Robert Brown further observed, that a distinct embryonal funiculus existed in each of these so-called corpuscles; and that accordingly the plurality of embryos which might be observed in the *Coniferæ*, depended on the regular structure of the albumen. These interesting observations of Mr. Robert Brown were certainly the most satisfactory proofs that some deception had occurred in the observations which M. Corda soon afterwards published on the impregnation of the ovule in the red fir*.

Recently M. Horkel has made public his observations on the polyembryony of the *Coniferæ* †, being induced to do so by Treviranus's assertion that he had not been able to find either in *Pinus sylvestris*, or in *Abies excelsa*, anything of that which Robert Brown had published on the subject. The observations of M. Horkel are in general coincident with those of Brown, but he found in *Pinus Cembra*, besides the developed embryo, only two abortive rudiments. The corpuscles in the albumen M. Horkel terms *small cavities*, which he had already noticed in *Abies excelsa* in 1819. The funiculi (Brown), or the embryo bearers, with their rudimentary embryos, he observed in those plants to lie parallel and near to one another in the great cavity originating in the centre of the albumen; their number was usually three, rarely four; but he never observed more than one to be developed into an embryo. In *Taxus* the number of the embryonal rudiments is no longer so regular—two, three, and even four were noticed; but in this instance the formation of the apex of the so-called albumen is likewise no longer so regular as in *Pinus*, &c., for M. Horkel observed here, at times, only one corpuscle.

I would still make a small addition to these admirable observations of the above masters of this science, and beg to be allowed to make the following remarks. Robert Brown and Horkel call the solid body which is formed in the *Coniferæ* within the nucleus at the time of impregnation the albumen; according to my observations on *Abies excelsa*, on the Larch, &c., it consists of an opake substance, which presents about the firmness of a young gelatinous cartilage; I have never been able to observe that this substance is developed into albumen in the interior

* *Nova Acta Acad.* C. L. C., tom.. xvii. p. 599.

† *Bericht der Königl. Acad. zu Berlin*, 1839, p. 92; Magazine and Annals of Nat. Hist., vol. vii., p. 173.

of the embryo-sac; but it seemed to me as if the albumen originated from the peculiar gelatinous altered embryo-sac itself, a peculiarity with which we are already acquainted in the genus *Veronica* and several others.

I have also lately been able to follow up completely the transformation of the membrane of a simple cell into a gelatinous [sulzig] mass in the well known *Conferva bipunctata*, where, for instance, all the articulations which had entered into conjugation had entirely lost their cavities, the simple membrane having become converted into a gelatine, which filled the cavities of the joints. In the upper extremity of this peculiar cartilaginous body of the *Coniferæ*, small cavities originate, to which Mr. Brown has applied the name of corpuscula; they are usually three in number in the true *Coniferæ*; but I found as many as six in *Pinus uncinata* and *Abies excelsa*. It is difficult to describe the form and position of these cavities; they fill somewhat more than the upper third of the cartilaginous embryo-sac, and are separated from each other by tolerably firm septa, which have a downward direction, and which meet in the axis of the cartilaginous body. These septa, as also the hardish septa of these cavities, proceed in the axis of the entire body still deeper downwards, and leave in their centre a canal which extends down to the inferior third of the cartilaginous body. But after the action of the pollen-tubes has taken place, the germ-vesicles appear at the apex of these small cavities, and expand into long filaments, at the lower extremity of which the embryo is developed. Most usually a single embryo with its funiculus appears in each of these cavities; yet I once noticed three cavities and four funiculi in the Larch. In the *Coniferæ*, the funiculus of the embryos acquires an immense length and size, such as is not known in any other family with the exception of the *Cycadeæ* and *Tropæolum*; but the young embryo at the extremity of the funiculus is easily distinguished by its rounded form, and the more intense green colouring of its cells. In northern Germany, towards the end of June it has become evident in most *Coniferæ* which of the embryos is to arrive at perfect development; this embryo increases very rapidly in size towards the commencement of July, while the funiculi of the other embryos acquire a brownish colour and shrivel, but very frequently preserve themselves well to the middle of August. As early as the middle of July the funiculi have become so long, that they pierce through the inferior septa of the small cavities at the apex, and finally descend into the depth of the cartilaginous embryo-sac; indeed, the funiculi and the rudimentary embryos are sure to be found in July and the commencement of August, if the inferior portion of the cartilaginous embryo-sac be examined. But the cavities in the apex of the embryo-sac only disappear with the development of the one embryo which, lastly, early in August becomes so large that, as is well known, it fills the greatest portion of the embryo-sac, and re-absorbs its contents. The statement of Robert Brown that the embryonal funiculi in the *Coniferæ* sometimes ramify, and that then each of these ramifications terminates in a rudiment of embryo, is very remarkable. It is much to be regretted that no drawings have accompanied these observations; for the occurrence of embryos on the ramifications of the funiculus would be the best proof that the new view which has been advanced on the sexes of plants cannot be the correct one; I have never noticed this ramification of the funiculus with a number of embryos.

Lastly, *Viscum album* presents another kind of polyembryony, which in this case is founded on the plurality of the embryo-sacs; in a great many plants of *Viscum* there occur very often two, and somewhat less frequently even to the number of three, in one and the same nucleus near together. But frequently as the plurality of the embryo-sacs and of the young embryos may be observed in *Viscum*, still it is a *very* rare case for several embryos to be found in the developed seed. Gærtner, it is well known, never succeeded in finding such in the mature seed; and I have likewise dissected a very great number of seeds for that purpose, but have always found a single embryo only, while sometimes near it was still to be seen a trace of the absorbed embryo-sac. Indeed I have examined hundreds of developed fruits from such plants as in the first summer months had so frequently presented several embryos, that I found them in every sixth or seventh seed. But if the female flowers of *Viscum* be examined weekly, with regard to their polyembryony, from the time of impregnation to the middle of July and August, it will soon be found that in those cases where two embryo-sacs occur in one and the same nucleus, both are generally impregnated, and the young embryos likewise begin to be developed in them. Two such impregnated embryo-sacs observed on the 19th of July, are represented in fig. 9. Pl. I. But it is likewise frequently found that the embryo attains its full development only in one of the two embryo-sacs, while the other embryo-sac either remains entirely unimpregnated, or even stops in its development some time after impregnation, turns brown, and becomes abortive. When one of the embryo-sacs remains unimpregnated, it constantly retains its original form of a simple closed sac, and large cells are never formed in it, as represented in the drawings, figs. 6 and 7. On the other hand, a representation will be found in fig. 2. Pl. II., where the embryo-sac which has remained unimpregnated is situated near to one which is very highly developed, as may be observed towards the middle of July. In the very rare cases where I found three embryo-sacs in one and the same nucleus, I once saw in the middle of June only one embryo arrive at development, while the two other unimpregnated embryo-sacs were situated near it; and in the second case, only one embryo-sac had remained unimpregnated; and the second, although impregnated, shrivelled near to the one in the state of development. In a great number of *Viscum* seeds which I examined after the middle of July, and early in August, I always found one embryo only arrive at development; and I have never been able to observe anything to indicate that the embryo in *Viscum album* might be formed by the cohesion of two, or even three, ovula, as M. Decaisne has asserted, according to the statement in Treviranus's Physiology of Plants*. By the word ovula in this case, moreover, can only be meant the embryo-sacs with the embryos; for a cohesion of the ovula of that kind is totally impossible in *Viscum album*. When in *V. album* several embryos are contemporaneously developed in one ovule, I yet found that, at least down to the commencement of July, each embryo is imbedded in its distinct albuminous body; and therefore in this case the plurality of the embryos is very easily discovered in the horizontal section; I have nevertheless sought in vain for several embryos in the mature seed.

To judge from what we have heard on polyembryony in other cases, but especially in the *Coniferæ* and *Cycadeæ*, where a plurality of the embryos in the mature seed is so exceedingly rare, while their number at the beginning is constantly very considerable, we ought no longer to be astonished that in the ripe seed of *Viscum* the embryos are also usually simple. The duplication

* Vol. ii. p. 523. Subsequently published at length in the *Comptes Rendus*, 1839, No. 6, p. 201.

and even triplication of the root end which the seed of *Viscum* exhibits on germination, appears, however, to me to have as little to do with the plurality of the embryos as the greater number of cotyledons in the seed of the true *Coniferæ*.

The above explanation of Treviranus of the duplication and triplication of the root end of the embryo of *Viscum album*, induced me to make a more accurate examination of the impregnation and formation of the embryo in this highly interesting plant. The female flower of *Viscum* has, perhaps, with reference to the ovule, the simplest structure which can be imagined in a phanerogamous plant. There is in *Viscum* no distinct pistil, and consequently no true ovarium; but the ovule is a simple naked nucleus, whose summit projects free, and at the same time serves as stigma, as it receives the pollen direct. This naked nucleus (figs. 1, 2 and 3. Pl. I.) is inclosed all round with a calyx-like organ, upon which are seated four other leaf-like organs, which have sometimes been asserted to be sepals, sometimes petals; I shall here adopt the latter denomination. The drawing (fig. 3. Pl. I.) made with an amplification of about 40 with the simple microscope, will give a sufficient view of the structure of the female flower of our plant. *f* is the naked nucleus, the base of which is designated with *f**, and whose expanding summit *g*, clothed with small papillæ, supplies the place of the stigma of the pistil, which is here absent. On the calycoid organ, which surrounds the nucleus, two separate layers are distinguished, both by their outer limitation and by their inner structure, an outer and an inner one; they are most easily distinguished from each other by the course of the bundles of spiral tubes, which, in direct continuation of the ligneous bundles of the stalk, proceed through the short flower-stalk, and pass to the petals. In fig. 3. Pl. I. the structure of the female flower is completely and distinctly represented in a longitudinal section. The letters signify the same as in fig. 1: *f* is the naked nucleus, *g* its summit, and *f** its base; next to this nucleus is situated the interior layer of the calycoid organ *c c*, which is separated by the spiral-tube-bundles *e e*, from the outer layer *b b*. The spiral-tube-bundles *e e* proceed immediately to the petals *d d*; there are four to five such bundles in the calyx; some present ramifications, and from these originate various anastomoses, which may be seen most easily on the ripe fruits.

From the lateness of the spring this year, it was not till towards the middle of April that a distinctly perceptible cavity made its appearance; it became filled with a glutinous mucus, and, becoming more and more attenuated, extended to the stigma or summit of the nucleus; in figs. 2 and 3 this cavity is marked *i*, and the embryo-sacs are seen in it in the state in which they were in the last ten days of April. In several flowers I already found the first trace of the embryo-sacs in the beginning of the same month; they originated in the centre of the base of the nucleus, and then increased in length from below upwards; they grew therefore, as in the *Leguminosæ*, *Santalineæ*, &c., to meet the subsequently penetrating fertilizing substance. Most frequently two embryo-sacs occur in the cavity of one and the same nucleus, as, for instance, in fig. 3; some mistletoe plants always presented in one out of every six or seven flowers two embryo-sacs; but three embryo-sacs were exceedingly rare in all the plants that passed through my hands; for I noticed them only in two cases. On this plurality of the embryo-sacs, then, is the polyembryony founded in *Viscum*, by which it differs so remarkably from all other hitherto known cases.

On its first appearance the embryo-sac appears like a somewhat cylindrical utricle, which soon expands at the micropyle end, but retains at the opposite end its primitive size, even after several months (fig. 4. Pl. I. at *a*). The membrane of the embryo-sac of *Viscum* is remarkably thick and firm, such as I have scarcely ever observed in any other plant; nay, in a somewhat advanced state, one might be induced to believe that a second layer of membrane had been precipitated on the inner surface. In Plate I., figs. 4 and 5 represent the simple embryo-sacs observed in the first week of May; they are transparent sacs, mostly without any solid contents, and occur from ten to fourteen days and more previous to the shedding of the pollen by the male flowers, whence, therefore, it is clear that the number of the embryo-sacs, and consequently the number of embryos, is not dependent on the number of pollen-tubes which have to descend into the nucleus. The process of impregnation, and, what is especially remarkable, the further development of the embryo proceeds with extreme slowness in *Viscum*; it is even three or four weeks and more before the first traces of impregnation become evident in the embryo-sac.

The entrance of the pollen-tubes through the summit of the nucleus, and their connexion with the micropyle-end of the embryo-sac, which is so exceedingly easy to observe in hundreds of other plants, I have *never* been able to perceive in *Viscum*; probably the membrane of the pollen-tube is here so delicate that it is destroyed in preparing the section. The embryo-sac gradually increases in size from its first appearance to the complete development of the embryo, and its impregnation manifests itself in the following manner: at first the germ-vesicle appears in the micropyle-end of the sac; and nearly all round this vesicle is formed an opake and somewhat granular mucous substance, which constitutes the commencement of the fluid albuminous body. With the appearance of the germ-vesicle, however, there occurs likewise a remarkable change of the embryo-sac, which divides, by the formation of a greater or less number of septa, into many large cells, as may be seen in the annexed drawings. The origin of these cross septa commences at the upper end of the embryo-sac, where the young embryo is situated; and more and more of them are gradually formed from above downwards, as shown in the figures 6 to 9. About the middle of June the embryo-sac is usually divided into eight, nine, or ten large cells, and thereupon usually begins the division of these large cells by longitudinal septa, as represented in fig. 2. Pl. II. at *h h*, *i i*, &c. Sometimes some horizontal septa are likewise formed in an oblique direction as at *d*, fig. 8. Pl. I. In some early and strong plants, I found as early as the 16th of June, the embryo-sac divided by these horizontal septa into from fifteen to sixteen large cells; and these again separated by longitudinal septa into smaller cells. In the drawings of the embryo-sacs of the 16th and 19th of June, in figs. 8 and 9. Pl. I., there is seen, in almost every large cell, a cellular nucleus, and in many cells even several such; but in these nothing is more easy to observe than that the commencement of the formation of these cellular nuclei is subsequent to the formation of the large cells; and, consequently, that the cells cannot have been formed by this cellular nucleus. After the embryo-sac has in this manner become divided into smaller cells, the formation of the albuminous body takes place in them; and this is effected in the manner already known, more and more solid substance forming in the limpid fluid, which conglomerates into larger or smaller globules, around which the cellular membranes harden; and thus a delicate cellular tissue originates in the interior of those large cells of the embryo-sac, which is densely filled with solid substance, so that the entire embryo-sac is rendered perfectly opake by it. It is remarkable that the young embryo remains in this case for so long a time undeveloped; as soon as the end of May I saw it in the form of a simple globular vesicle, and in this state it remains for nearly a month, as represented at fig. 2. Pl. II. In this case the young embryo was already so much surrounded by albumen, that it was difficult to recognize it therein. With the commencement of July the embryo-sac expands very much indeed; the swelling also extends from the embryo, and descends deeper and deeper, so that very frequently in the first week of July it forms a nearly perfect ellipsoid figure; and at the lower extremity only, the chalaza-end, it is still provided with a small stipes, by which it remains attached for the whole time: it is not until after the embryo-sac, with the albuminous body, has nearly attained its perfect development, that the further development of the embryo begins.

The small size which the embryo has acquired in the middle of July is shown at fig. 4. Pl. II., from a drawing magnified twenty times of a longitudinal section of the fruit: *h* represents the embryo-sac with its short stalk; *i* and *k* the small embryo which is just about to expand, but still exhibits no trace of cotyledons. From this time the enlargement of the embryo proceeds rapidly, and as it elongates it breaks through all the transverse septa of the large cells of the embryo-sac, and is then situated exactly in the longitudinal axis of the albuminous body. Fig. 6. Pl.II. gives a representation of the embryo observed in the middle of August, likewise magnified twenty times. Figs. 4 and 5 serve to explain the structure of the fruit; on the cross section in fig. 5, *a a* exhibits the external portion of the calyx; this is the thick outer envelope of the berry, which in the ripe state acquires the white parchment-like condition, at last becomes thinner, and contains the spiral-tube-bundles which give off separate bundles to the sepals: *b b* shows the inner portion of the calyx, the cells of which are elongated during the development of the berry in a horizontal direction, so that from this cause the fruit expands horizontally or in breadth, and at last becomes quite globular. Simultaneously with this excessive elongation of the cells, there occurs a metamorphosis of the previously hardened cellular membrane into a gelatinous substance, which probably is nothing else than the viscin, which is contained in such quantity in the berries of this plant. The contents of these cells also, which at first consisted of small globules, and of a larger ball of a gummy substance, is dissolved and changed into viscin. In this visciniferous layer is situated the lenticularly compressed nucleus *c c*; it was in the young flower perfectly round, solid, and without any envelope; subsequently a cavity was formed in it for the formation of the embryo-sac, and it then expanded laterally, while the inner still persistent cellular tissue became very wide-meshed, so that even with a power of twenty these cells are very easily recognised. Lastly, the albuminous body with the embryo expands to such an extent that the entire inner cellular mass of the nucleus is dislodged, and only a few layers of cells still remain, of which the outer ones exhibit very large and beautiful spiral fibrous cells. In fig. 5. *c* represents the embryo-sac, with the albumen situated within the cavity of the nucleus, and surrounded on all sides with a mucous mass, and large latticed delicate cellular tissue. The embryo exhibits until nearly its complete development, a small funiculus, which, however, generally consists of a single cell.

What is most remarkable in the formation of the albuminous body of *Viscum*, is the previous division of the embryo-sac into large cells, a phænomenon which, however, is no longer so isolated. M. Brongniart, in his celebrated memoir on the production of the vegetable embryo, already gave a representation of the embryo-sac of *Ceratophyllum submersum*, according to which it consists of three large cells arranged near each other; but he had not then observed that these cells result by constriction from the previously quite simple embryo-sac. M. Horkel at that time pronounced the cellular embryo-sac, according to Brongniart's drawing, to be large-celled albumen, and M. Schleiden supposed he had proved this*, by representing around it a distinct embryo-sac, of the non-existence of which I think I have perfectly convinced myself. But this is not exactly the place to enter into more minute detail on the formation of the large-celled embryo-sac of *Ceratophyllum*; the formation of the albuminous body in it differs, however, from that of *Viscum* in this only, that in the latter all the large cells of the embryo-sac gradually fill from above downwards with the albuminous body; while in *Ceratophyllum* it is only formed in the three upper large cells, and the other still larger cells shrivel together as

* *Linnæa*, vol. xi. fig. 9.

soon as the embryo has broken through those upper cells with the albuminous body.

The occurrence of several embryo-sacs, and the formation of embryos in them, takes place therefore in the young fruit of *Viscum*, as we have already seen, not unfrequently; but it is not long before one of these embryos, with its enveloping embryo-sac, presents a distinct development; while the other, or even the two which occur in proximity, remain behind in their development, and are at last entirely abortive. Thence it comes to pass that in ripe seed of *Viscum* several embryos must be of exceedingly rare occurrence, if indeed they have ever actually been met with. In the summer just passed I have been able to observe several embryo-sacs, with their young embryos, as far as the first half of July; about this time the predominant development of one constantly occurred, if it had not already taken place even in June. From the commencement of August the examination of the seeds of *Viscum* becomes very difficult from the formation of viscin; but now fine cross sections may be employed, and these examined even with glasses of the highest power, to arrive at a conviction that only the one embryo, imbedded in its albuminous body, arrives at development, and that its formation can by no means ensue from a cohesion of several. The occurrence of several rootlets in the germinating seed of *Viscum* is by no means rare, and therefore, if this phænomenon were explained by a cohesion of several embryos, we should find those cohering embryos in the ripe seed to be not at all unfrequent. The fine horizontal sections of the mature seed show, however, quite distinctly, that the embryo is simple, and constantly possesses two cotyledons of usual structure; it is surprising, however, that the cauliculus exhibits almost from the radicular end two more transparent places in the cross section which are formed by somewhat large latticed cellular tissue, and are prolonged even into the two cotyledons; perhaps these led to the adoption of the supposition that the embryo is here formed by the cohesion of several.

In conclusion, I have to acknowledge my obligations to the royal gardener, L. Fintelmann, who kindly furnished me during the summer with the requisite quantity of mistletoe-plants.

Article II.

On the Combinations of the Volatile Chlorides with Ammonia, and their Constitution. By Professor Heinrich Rose*.

[From Poggendorff's *Annalen*, vol. lii. p. 57, No. 1 for 1841.]

SOME time ago I compared the compounds of the oxysalts with ammonia to the compounds which the same salts form with water. I likewise showed that the combinations of ammonia with the non-volatile chlorides, which possess so much similarity to the oxysalts in most of their properties, might be considered analogous to the combinations of the same salts with water†.

This view, little attended to at first, was subsequently generally adopted, especially after Berzelius had called attention to the distinction between the compounds of ammonia and of the oxide of ammonium.

I subsequently examined the compounds of the volatile chlorides with ammonia, and attempted to compare them with those which they produce with phosphuretted hydrogen‡. Some only however of these combinations are analogous to each other in properties. It is principally the chloride of titanium, the chloride of tin, and the chloride of aluminium which give with phosphuretted hydrogen compounds that may be compared with those of ammonia. Perchloride of iron, liquid chloride of phosphorus, and chloride of sulphur, which afford combinations with ammonia, form none with phosphuretted hydrogen, but, on the contrary, they mutually decompose each other.

Perhaps the combinations with ammonia of the chlorides may be more correctly compared with those which water forms with them, in the same manner as the ammoniacal non-volatile chlorides. The hydrates of the volatile chlorides are, however, not yet sufficiently known, but they have this in common with the analogous ammoniacal compounds, that the water or the ammonia cannot be separated from either by exposure to

* Translated and communicated by Mr. William Francis, A.L.S.
† Poggendorff's *Annalen*, vol. xx. p 147.
‡ *Ibid.*, vol. xxiv. p. 109.

heat, and that the volatile chlorides cannot be easily re-prepared from them.

The volatile combinations of chlorine take up various quantities of ammonia, but just as little as the quantity of water of crystallization can be determined *à priori* by any law, or of ammonia, in their combinations with oxysalts, or with non-volatile chlorides, can the amount of ammonia which the volatile chlorides are able to take up be predicted in advance. The somewhat empirical rule, however, appears to be followed, if we compare the different combinations with one another, that those chlorides whose radical forms with oxygen a stronger acid can take up more ammonia than those the radical of which produces with oxygen so weak an acid that it does not enter into any decided salt-like combination with ammonia.

A comparison of the compositions of the compounds with ammonia of the volatile chlorides which have hitherto been examined may make this assertion probable. I shall divide these combinations into two classes; the first class will comprise those combinations the chlorides of which correspond to weak acids; the second class those whose chlorides correspond to more powerful acids.

First Class.

1. *Chloride of titanium-ammonia*; $Ti\,\underline{Cl}^2 + 2\,\underline{N}\,\underline{H}^3$ according to my examination*, $Ti\,\underline{Cl}^2 + 3\,\underline{N}\,\underline{H}^3$ according to Persoz†.

2. *Chloride of tin-ammonia*; $Sn\,\underline{Cl}^2 + \underline{N}\,\underline{H}^3$ according to my examination‡, $Sn\,\underline{Cl}^2 + 2\,\underline{N}\,\underline{H}^3$ according to Persoz.

3. *Chloride of aluminium-ammonia*; $\underline{Al}\,\underline{Cl}^3 + 3\,\underline{N}\,\underline{H}^3$ according to my examination, agreeing with that of Persoz§.

4. *Perchloride of iron-ammonia*; $\underline{Fe}\,\underline{Cl}^3 + \underline{N}\,\underline{H}^3$ according to my examination ||.

5. *Chloride of sulphur-ammonia*; $S\,\underline{Cl} + \underline{N}\,\underline{H}^3$ according to my examination¶.

6. *Chloride of antimony-ammonia*; $\underline{Sb}\,\underline{Cl}^3 + 2\,\underline{N}\,\underline{H}^3$ according to my examination**.

* Poggendorff's *Annalen*, vol. xxiv. p. 145.

† *Annales de Chimie et de Physique*, t. xliv. p. 321. Persoz states that the combination contains in one hundred 65·861 chloride of titanium, and 34·139 ammonia. A composition calculated according to the formula above given contains in one hundred 64·89 chloride of titanium, and 35·11 ammonia.

‡ Poggendorff's *Annalen*, vol. xxiv. p. 163.

§ *Ibid.*, p. 298. || *Ibid.*, p. 301. ¶ *Ibid.*, p. 306. ** *Ibid.*, vol. xx. p. 160.

In the combinations only of ammonia with chloride of titanium, chloride of aluminium, and chloride of sulphur, does the quantity of ammonia exactly suffice to form chloride of ammonium with the chlorine of the chloride, when the chloride takes up the constituents of water; the other chlorides possess less ammonia. Nevertheless, we must not regard these combinations, as will subsequently appear, even after treatment with water, as combinations of chloride of ammonium with oxides, but as ammoniacal compounds of a peculiar kind, comparable with, and similar to the combinations of certain anhydrous acids, especially sulphuric acid with ammonia (sulphat-ammon). The solubility of most of these compounds in water, even when the oxide which they contain is of itself insoluble either in water or in solutions of ammoniacal salts, renders this probable; but still more does the circumstance that in the solutions of these combinations the quantity of ammonia can only be partially separated, and by no means entirely, by a solution of chloride of platinum, as is likewise the case in the solutions of sulphat-ammon and parasulphat-ammon.

SECOND CLASS.

To this class belong but few combinations, for it is those radicals which form the most powerful acids with oxygen, that give no corresponding combinations with chlorine; at least we are not acquainted with any chlorides corresponding to sulphuric acid, selenic acid, chromic acid, nitric acid, molybdic acid, tungstic acid, or arsenic acid; and several chlorides which, it is true, correspond to pretty strong acids, appear not to combine with ammonia. It is almost solely the liquid compounds of chlorine with phosphorus and arsenic, analogous in constitution to the phosphorous and arsenious acids which have here occasion to be mentioned.

Protochloride of phosphorus-ammonia.—According to my analysis it is composed according to the formula $\underline{P}\,\underline{Cl}^3+5\,\underline{N}\,\underline{H}^3$, and contains just so much ammonia, that when the combination is dissolved in water neutral phosphite of the oxide of ammonium and chloride of ammonium can be produced*. Persoz, although he likewise states that the combination would be converted, by treatment with water, into chloride of ammonium,

* Poggendorff's *Annalen*, vol. xxiv. p. 308; and vol. xxviii. p. 529.

and neutral phosphite of the oxide of ammonium, has found in it only 4 atoms of ammonia ($\underline{P}\underline{Cl}^3 + 4\ \underline{N}\ \underline{H}^3$)*. The liquid chloride of phosphorus, even when strongly cooled, absorbs gaseous ammonia with avidity; I have drawn especial attention to the circumstance that the combination can only be obtained pure when the chloride is brought into contact with the ammoniacal gas very slowly and with great cooling.

I had already devoted much fruitless labour to produce a combination of ammonia with the solid chloride of phosphorus corresponding to phosphoric acid, in definite proportion†. I have subsequently repeated those experiments, but with the same unfavourable result. I shall, however, briefly communicate the results of these later experiments.

The solid chloride of phosphorus is heated with the greatest violence, when treated with dried ammoniacal gas. If, however, the mass, completely saturated with ammonia, when it has become much warmed, is treated with water, phosphuretted nitrogen remains undissolved, and in the solution no phosphoric acid can be detected by reagents; it contains only chloride of ammonium. If, on the other hand, the solid chloride of phosphorus is treated slowly and under great cooling with dried gaseous ammonia, it absorbs almost none of it; it can be kept for a long time in a vessel filled with ammoniacal gas when it is perfectly closed. When it is dissolved in water, this happens with evolution of great heat, as is the case with pure chloride of phosphorus, some flocks of undissolved phosphoretted nitrogen float in the solution; but it contains only phosphoric and hydrochloric acids. These observations agree in part with those published long since by Liebig and Wöhler‡.

Protochloride of arsenic-ammonia.—I had not previously prepared this compound. Persoz states that it is composed after the formula $\underline{As}\ \underline{Cl}^3 + 2\ \underline{N}\ \underline{H}^3$§. For the experiments which I more recently made to determine the composition of this combination, I employed a protochloride of arsenic, which had been partly obtained by distillation of a mixture of powdered arsenious acid, chloride of sodium and sulphuric acid, and partly by treating metallic arsenic with chlorine gas. The protochloride

* *Annales de Chimie et de Physique*, t. xliv. p. 320.
† Poggendorff's *Annalen*, vol. xxiv. p. 311.
‡ *Annalen der Pharmacie*, vol. xi. p. 139.
§ *Annales de Chimie et de Physique*, t. xliv. p. 320.

obtained by the last method was carefully freed from adherent chlorine.

If the protochloride is treated with dry ammoniacal gas, a white powder is obtained with evolution of heat, which is perfectly soluble in water. 1·862 gramme of the well-saturated combination, dissolved in water, to which some nitric acid and then nitrate of silver were added, gave 3·323 grammes chloride of silver.

1·368 gramme of the combination from a different preparation gave, on similar treatment, 2·4195 grammes chloride of silver. If the composition of the protochloride of arsenic-ammonia be calculated in 100, according to these experiments, the following results are obtained :—

	I.	II.
Chlorine	44·02	43·63
Arsenic	31·16	30·89
Ammonia	24·82	25·48
	100·00	100·00

The second quantity of the compound examined retained some adhering ammonia; for it had been long preserved in a vessel filled with dry ammoniacal gas. The solution of this combination reacted, moreover, more strongly alkaline than that of the other quantity of the compound.

The composition found agrees with the formula $\underline{As}\,\underline{Cl}^3 + 7\,N\,H^3$, or rather $2\,\underline{As}\,\underline{Cl}^3 + 7\,\underline{N}\,\underline{H}^3$; the composition calculated according to this contains in 100,

Chlorine	44·00
Arsenic	31·14
Ammonia	24·86
	100·00.

In its solution by water there might be formed, supposing all the chlorine of the protochloride of arsenic converted into chloride of ammonium, an acid arsenite of the oxide of ammonium, $2\,\underline{\dddot{As}} + \underline{\dot{N}}\,\underline{H}^4$ (the neutral salt is most probably $\underline{\dddot{As}} + \underline{\dot{N}}\,\underline{H}^4$).

I have attempted in va in to prepare a chloride of arsenic corresponding to arsenic acid, in order to be able to examine its combination with ammonia. Liebig and Wöhler had previously failed in preparing it*. I have obtained the same results as

* *Annalen der Pharmacie*, Bd. xi. p. 150.

they, on attempting to distil a mixture of arsenic acid and chloride of sodium, with sulphuric acid. Chlorine and protochloride of arsenic are obtained, with strong effervescence. Even when metallic arsenic is treated with great excess of chlorine gas, and the protochloride of arsenic obtained kept long in contact with chlorine, no per-chloride of arsenic can be obtained.

I endeavoured to combine chloride of selenium (Se $\underline{Cl}^2$) with ammonia; but the chloride takes up none of it in the cold, and when warmed it is decomposed. In fact only the liquid chlorides appear to combine with ammonia with energy, and not the solid ones.

From the few combinations with ammonia which the chlorides form corresponding to the more powerful acids, we, however, distinctly see that they contain more ammonia than the combinations of ammonia with the chlorides forming such weak acids that they are not capable of forming salt-like compounds with ammonia. The latter take up at the utmost only so much, that on the solution of the compound in water its ammonia suffices to form with the hydrochloric acid produced the chloride of ammonium; frequently, however, they even take up much less ammonia. The other chlorides, on the contrary, take up more ammonia, and in part so much that they can form a neutral ammoniacal salt with the acid generated, besides chloride of ammonium.

Phosphorous acid is evidently a far more powerful acid than arsenious acid. On the solution of the protochloride of phosphorus-ammonia in water, a neutral phosphite of the oxide of ammonium and chloride of ammonium are consequently formed; while on treating the protochloride of arsenic-ammonia with water, besides chloride of ammonium, binarseuite of the oxide of ammonia only is produced.

We may, therefore, in all probability conclude, that if the chlorides of sulphur, of selenium and of arsenic, which correspond to sulphuric selenic and arsenic acids, were known to us in their isolated state, their combinations with ammonia, if such could be produced, would, on being treated with water, give sulphate, seleniate, and arseniate of the oxide of ammonium, besides chloride of ammonium.

Again, the compounds of the protochloride of phosphorus, and protochloride of arsenic with ammonia, must not be regarded after their treatment with water as mixtures of the chloride of ammonium with salts of the oxide of ammonium, but as peculiar ammoniacal compounds, similar to the ammons. From their solutions in water, their amount of ammonia can by no means be precipitated entirely, but only in part, by chloride of platinum.

The combination of the sulphate of the chloride of sulphur ($S\,\underline{Cl}^3 + 5\,\dddot{S}$) *with ammonia.*—An accurate knowledge of the composition of this compound is in so far of considerable interest, as by means of it some perhaps of the important theoretical questions which occupy chemists at the present time may be answered.

I have prepared this combination some time back, and made known the most important of its properties*. It was then, however, not perfectly pure; for it is exceedingly difficult to saturate the sulphate of the chloride of sulphur perfectly with ammonia. There is no volatile compound of chlorine in which it is effected with so much difficulty as in this. At the commencement, the ammoniacal gas must be conducted very slowly to the compound, which must be kept at the same time strongly cooled, in order to prevent all heating; for if the gas be passed too rapidly, and the vessel not kept cool, the whole mass becomes very much heated and yellow. The cause of this yellow colour, which arises from some sulphit-ammon (anhydrous sulphite of ammonia), I have formerly explained. Its presence is the cause why the solution of the ammoniacal combination gives with a solution of nitrate of silver a precipitate of chloride of silver which has a yellowish tint, arising from a slight intermixture of sulphuret of silver. The combination is far from being completely saturated with ammonia, when the glass in which it is prepared and preserved contains free ammonia. After a long time this is absorbed; ammoniacal gas must again be conducted to it, and the pieces of the compound be broken up as small as possible. This must be repeated from time to time, and continued so long, until free ammonia is observed in the vessel after a long interval. I have only succeeded, after the lapse of half a year, in saturating perfectly with ammonia somewhat considerable quantities of the sulphate of the chloride of sulphur.

The combination, carefully prepared, is perfectly white, and completely soluble in water. The best proof of its being satu-

* Poggendorff's *Annalen*, Bd. xliv. p. 300.

rated with ammonia, is when the aqueous solution does not in the least redden litmus paper. The litmus paper, moistened with the solution, becomes, however, red on drying; but this is the case with many ammoniacal salts, with chloride of ammonium, with the sulphate of the oxide of ammonium, with sulphat-ammon, and parasulphat-ammon. Nitrate of silver should produce a perfectly white precipitate in the solution.

The solution reacts towards a solution of a salt of barytes just as the solution of sulphat-ammon, as I have above observed: I will here add, that the opakeness of sulphate of barytes becomes stronger, especially on the addition of hydrochloric acid. No precipitate is produced by chloride of strontium, except on being boiled, and when, at the same time, free hydrochloric acid has been added. Only a part of the ammonia is thrown down from the solution by chloride of platinum.

This compound powdered attracts a very slight quantity of moisture, in the same way as sulphat-ammon, and in general most pulverized bodies, but does not deliquesce.

1·383 gramme of the compound, dissolved in water, was treated with nitric acid, and then with nitrate of silver; 1·2505 gramme chloride of silver was obtained. The oxide of silver was removed from the filtered solution by hydrochloric acid; upon which a solution of the chloride of barium was added in excess, the whole evaporated to dryness, the dried mass heated to an incipient red heat, and hydrochloric acid and water added to it; in fact, it was treated in the same manner exactly as the sulphat-ammon, when the quantity of sulphuric acid was to be determined in it: 2·040 grammes sulphate of barytes were obtained. The quantity of chloride of silver corresponds to 22·306 per cent. of chlorine, and the quantity of the sulphate of barytes to 20·35 per cent. of sulphur in the compound.

These quantities correspond to a compound, which consists of one atom of the sulphate of the chloride of sulphur, and of eighteen simple, or nine double atoms of ammonia ($\dddot{S}\,\underline{Cl}^3\,\dddot{S}^5 + 9\,\underline{N}\,\underline{H}^3$). It is thus constituted in 100:—

Chlorine	22·23
Sulphur	20·26
Oxygen	25·15
Ammonia	32·36
	100·00.

1·170 gramme of the combination prepared at another time gave, treated in the same way, 1·052 gramme of chloride of silver, and 1·733 sulphate of barytes, which corresponds to 22·18 chlorine and 20·44 per cent. sulphur, agreeing therefore both with the results of the first analysis, and also with the calculated composition.

The compound is constituted exactly as might be expected, *à priori*, from what has been previously stated. If the chloride of sulphur ($S\,\underline{Cl}^3$) could be prepared in its isolated state, it would, as is highly probable, and has already been observed, take up so much ammonia, that when the combination was treated with water, it might be considered as a combination of chloride of ammonium and of sulphate of the oxide of ammonium, or more correctly perhaps of sulphat-ammon. In this case 1 atom of the chloride has 4 double atoms of ammonia. Since now the 5 atoms of anhydrous sulphuric acid, which are contained in 1 atom of the sulphate of the chloride of sulphur, take up 5 double atoms of ammonia to form sulphat-ammon, 1 atom of the sulphate of the chloride of sulphur must combine with 9 double atoms of ammonia.

Regnault* has examined the combination of ammonia with a sulphate of the chloride of sulphur first prepared by him, which, like the chromate of the chloride of chromium, consists of 2 atoms of sulphuric acid and 1 atom of the chloride of sulphur, $S\,\underline{Cl}^3 + 2\,\dddot{S}$. He found that this compound takes up 6 double atoms of ammonia, which likewise is exactly the quantity which, *à priori*, might be admitted in its ammoniacal combination. In other respects it is essentially different from the one I had previously prepared, in so far as it deliquesces when exposed to the atmosphere, which is not the case with the other.

But Regnault neither regards the sulphate of the chloride of sulphur, nor its combination with ammonia, in the same manner as we have done in this memoir. Owing to the theory of substitution advanced by Dumas, and the views which Walter and Persoz have started on the constitution of the chromate of the chloride of chromium and similar compounds, he looks upon the combination, $S\,\underline{Cl}^3 + 2\,\dddot{S}$, as a sulphuric acid in which one-third of the oxygen is replaced by chlorine, and therefore as $\ddot{S}\,\underline{Cl}$.

* *Annales de Chimie et de Physique*, t. lxix. p. 176.

Its combination with ammonia is, according to him, a mixture of a sulphamide $\ddot{S} N H^2$ (analogous to oxamide), and of chloride of ammonium.

With regard to the first view, I have already, in a former paper, endeavoured to show that the combination $S\,Cl^2 + 5\,S\,O^3$, prepared by me, cannot well be regarded as $S + 2\frac{1}{2}\,O + Cl$.*; and the reasons which I mentioned rendered it likewise probable that Regnault's chloro-sulphuric acid should be considered as a sulphate of chloride of sulphur. In this memoir, however, additional reasons will be given in the sequel in support of my view.

As to Regnault's view respecting the nature of the ammoniacal combination, he himself admits that he had found it impossible to separate the chloride of ammonium from the sulphamide mixed with it; for both bodies he supposes have nearly the same solubility in water and in alcohol, and could be but very imperfectly separated by crystallization.

The sole reason which Regnault mentions in support of the view that the ammoniacal combination contains chloride of ammonium is, that the entire quantity of the chlorine can be thrown down from its solution as chloride of silver, by a solution of nitrate of silver. He further supposes that chloride of platinum only precipitates that portion of the ammonia from the solution which is contained in it as chloride of ammonium, and not the amide in the hypothetically adopted sulphamide.

From all the combinations of volatile chlorides with ammonia which I have examined, the amount of chlorine may, after their solution in water, be determined by nitrate of silver in the usual manner, whether they contain more or less ammonia than is requisite to form chloride of ammonium with the quantity of chlorine. All these ammoniacal compounds, at least those I have examined, have further in this respect the property that their amount of ammonia can by no means be entirely precipitated, frequently only to about one half, by chloride of platinum, from their solutions, as chloride of ammonium-platinum. It seems as if only the oxide of ammonium, not the ammonia, could be completely thrown down by chloride of platinum; for the sulphat-ammon, as well as the parasulphat-ammon, in which decidedly ammonia only is contained, give, after their solution in water, it is true, precipitates with chloride of platinum; but by far the

* Poggendorff's *Annalen*, Bd. xlvi. p. 16.

greater portion of the ammonia is not precipitated by it. If the fluids filtered from the precipitate are evaporated, they give still, on the addition of hydrate of potash, a strong ammoniacal smell. Carbonat-ammon (anhydrous carbonate of ammonia) gives, it is true, a solution in water, from which the ammonia can be entirely precipitated by chloride of platinum *; but this is indeed a proof that the carbonat-ammon is converted, upon its solution in water, into carbonate of the oxide of ammonium. The aqueous solution of carbonat-ammon likewise comports itself quite similar to the solutions of the carbonates of the oxide of ammonium; the carbonic has not lost in the solution its property of being recognized by the reagents by which it can be detected in its other combinations, as is the case with the sulphuric acid in the sulphat and parasulphat-ammon, which are not converted by water into salts of oxide of ammonium.

I have made numerous experiments to find whether chloride of ammonium mixed with a sulphamide, is contained in the compound of the sulphate of the chloride of sulphur with ammonia, prepared by me, or whether it be a peculiar combination. The results of all the experiments speak decidedly in favour of the latter view. I will lay little stress on the curious constitution which such a sulphamide must have; it, however, deserves mentioning. If we suppose, according to the theory of substitutions, the constitution of the sulphate of the chloride of sulphur as according to the formula $S + 2\frac{1}{2}\,O + Cl$, or rather as $S^2\,O^5\,\underline{Cl}$, the latter must take up $3\,\underline{N}\,\underline{H}^3$ to form the ammoniacal compound. If chloride of ammonium mixed with a sulphamide is contained in such a compound, then again the latter must be a mixture of a peculiar sulphamide with sulphat-ammon, for $S^2\,O^5\,\underline{Cl}^3 + 3\,\underline{N}\,\underline{H} = \underline{Cl}\,\underline{N}\,\underline{H}^4 + S^2\,O^5\,\underline{N}^2\,\underline{H}^5$; but the latter compound must be regarded as $\dddot{S}\,\underline{N}\,\underline{H}^3 + \ddot{S}\,\underline{N}\,\underline{H}^2$. The ammoniacal combination would, according to this, be a mixture of three compounds, namely, of chloride of ammonium, sulphat-ammon and sulphamide.

I have dissolved a considerable quantity of the ammoniacal combination in water, and evaporated the solution under the air-pump over sulphuric acid. Crystalline crusts were formed on evaporation, but it was not possible to distinguish in them di-

* Poggendorff's *Annalen*, Bd. lxvi. p. 361.

verse crystalline forms. They appeared homogeneous, although their form could not be determined. I expected to obtain crystals of parasulphat-ammon; but even these were not evident at any period of the evaporation. The evaporated mass remained for a long time moist and smeary, but at last dried perfectly. It was left *in vacuo* till it no longer decreased in weight.

1·227 gramme of the dried mass, examined in the manner above mentioned, gave 1·136 gramme chloride of silver, and 1·794 gramme sulphate of barytes. This corresponds to 22·84 per cent. chlorine, and 20·17 per cent. sulphur in the compound.

The compound dissolved in water and evaporated, was composed just as the mass originally prepared. It has, on its solution in water, taken up none of it.

From these researches it results, that the ammoniacal compound is not a mixture of several substances, but a combination of a peculiar kind. But if this is the case, its constitution leads to some considerations not quite unimportant.

According to Walter and Persoz, the volatile compounds which have recently been discovered, of acids, with chlorides analogously constituted, are considered as acids in which a portion of the oxygen is replaced by chlorine. This supposition, which has its origin in the theory of substitutions, has at first sight much in favour of it, and is recommended by its simplicity; for although oxygen, with respect to its properties, stands pretty much isolated, yet it has of all elements, on the one hand, the most similarity to sulphur, on the other, to chlorine or fluorine. It would, therefore, it is true, be a highly interesting, although to many chemists perhaps not quite unexpected fact, if an isomorphous compound were to be discovered, in which an equivalent of chlorine holds the place of oxygen.

This view, however, on closer consideration, loses in probability. If we compare the combinations which oxygen and chlorine form with hydrogen in their gaseous state, we find that they possess quite different relations of condensation. The relations of condensation of gaseous compounds which contain elements that can hold each other's place in other solid combinations, and which are isomorphous, are on the contrary always the same. This is the case in the combinations of chlorine, bromine and iodine with hydrogen, as well as in those of phosphorus and arsenic with the same element.

Berzelius likewise speaks decidedly, from other considerations, against the view that oxygen can be replaced by chlorine in the so-called basic hydrochlorate salts, as well as in the combinations of acids with chlorides here in question; and he retains the view relative to the latter compounds which I advanced when I made known the constitution of the chromate of the chloride of chromium *.

If we combine two isomorphous acids with a base, or two isomorphous bases with an acid, it is always a like proportion of base or of acid which is taken up by the isomorphous substances. And if we were to mix the two isomorphous acids and isomorphous bases in any proportion, the quantity of base or of acid which would combine with the mixture would always stand in a like proportion as the quantities which had been combined with the acids or bases separately.

Among the bases ammonia possesses a peculiar character. It only appears as a distinct base when it has taken up water, or the constituents of it, in which case it has become converted into the oxide of ammonium, which is a base perfectly analogous in its properties to the other oxybases. In the combinations of ammonia with acids (the ammons), the ammonia remains, on treatment with water, either combined as such with the acid, as in sulphat-ammon and parasulphat-ammon, or it is converted by this treatment into the oxide of ammonium, as in the carbonat-ammon and other ammons which I have recently had occasion to examine.

When two acids, which give isomorphous compounds with the same base, combine with ammonia to form neutral ammons, it is highly probable that each acid takes up an equal number of atoms of ammonia. Even when both acids have been mixed in different proportions, the ammonia taken up by this mixture must stand to them in the same proportion as to the individual acids.

If, for instance, a portion of the sulphur in the anhydrous sulphuric acid were replaced by selenium, or even by chromium, the new acid, which might be regarded as a combination of selenic acid, or of chromic acid with sulphuric acid, must take up just as many atoms of ammonia as the latter alone. But the same should likewise be the case when in the sulphuric acid the other element, the oxygen, is replaced by chlorine, in a similar manner as the sulphur by selenium or chromium.

* *Annalen der Pharmacie*, Bd. xxxi. p. 113.

But the results of the experiments above detailed show that this is not the case. Both Regnault's compound, as well as the sulphate of the chloride of sulphur I have prepared, take up more ammonia than if they were sulphuric acids in which a part of the oxygen was replaced by chlorine.

Regnault's compound is $S\,\underline{Cl}^3 + 2\,\dddot{S}$, or according to his view, $\ddot{S}\,\underline{Cl}$. If we were to regard it as a sulphuric acid in which a portion of the oxygen is replaced by chlorine, and in which the latter fully occupies the place of the oxygen, then, since one atom of sulphuric acid takes up one atom of ammonia, to form sulphat-ammon; $S\,\underline{Cl}^3 + 2\,\dddot{S}$ should take up three double atoms, and $\ddot{S}\,\underline{Cl}$ one double atom of ammonia. But according to Regnault's own researches, in the first case six, in the second case two double atoms of ammonia are taken up by the compound; twice as much therefore as, according to the theory of substitution, should be expected. The combination of the chloride of sulphur with sulphuric acid, which I have prepared, is $S\,\underline{Cl}^3 + 5\,\dddot{S}$. According to the theory of substitutions this combination would be regarded as $S + 2\frac{1}{2}\,O + Cl$, or rather as $S^2\,O^5\,\underline{Cl}$. In the first case it ought, according to this theory, to combine with six; in the second with one; and in the third with two double atoms of ammonia. But experiments have shown that, in the first case, it takes up nine, in the second one and a half, and in the third, three double atoms of ammonia.

I believe I may hence conclude that all the volatile compounds of chlorine which have hitherto for some time been considered as combinations of acids with chlorides, must still be so regarded, and not as acids in which a portion of the oxygen is replaced by chlorine.

Liebig has brought forward an important reason in favour of the theory of substitutions*. He draws attention to the isomorphism discovered by Mitscherlich between the hyperchlorates and hypermanganates, from which it results that chlorine may be replaced by manganese. But in properties chlorine and manganese have, in fact, far less similarity to each other than chlorine and oxygen. Even, however, if no resemblance can

* *Annalen der Pharmacie*, Bd. xxxi. p. 119.

be detected in the elements, manganese is a radical just as chlorine is; both can combine with oxygen, and in the highest stages of oxidation there is a very considerable analogy evident between the two. Sulphur and chromium, which can replace each other, differ in like manner, while several sulphate and chromate salts are isomorphous; whence it results that a substitution of sulphur and of chromium by chlorine, in certain combinations, is likewise possible.

If, however, a substitution of oxygen by chlorine cannot be admitted, a replacement of hydrogen by chlorine is far less probable. But this very substitution, as is well known, is the most prominent point in the theory advanced by Dumas; and a great number of cases seem to render it probable, although in numerous other cases such a substitution does not occur.

No view in modern times has, perhaps, been so stimulating and fruitful as that of Dumas on the substitution of hydrogen by chlorine. We are indebted to it for a vast series of the most excellent researches in organic chemistry, which, without this excitement, would probably not have been undertaken. But all these labours do not prove such a substitution beyond controversy, if we except the most recent discovery of De la Provostaye of the isomorphism between oxamethane and chloroxamethane*.

Although there may exist a great number of substitutions which are not distinguished by isomorphism, yet without contradiction this is the most certain indication of a true substitution. After the discovery, therefore, of De la Provostaye, no objection could be made to the substitution of hydrogen by chlorine, if this discovery were confirmed. But from the description of the forms of the crystals, the isomorphism cannot be confidently relied upon.

Should, however, the substitution of hydrogen by chlorine be irrefutably proved by several examples, by the isomorphism of compounds which these elements form, then indeed we must arrive at the conviction that in complex combinations the grouping of the atoms only, and not the different chemical nature of the elements, necessitates analogy in properties.

The same conclusions as are drawn from the combination of the sulphate of the chloride of sulphur with ammonia, likewise follow from the combination of the carbonate of the

* *Annales de Chimie et de Physique*, t. lxxv. p. 322; and Philosophical Magazine for May 1841, p. 372.

chloride of carbon (phosgene gas) with ammonia. The carbonate of the chloride of carbon, $C\,\underline{Cl}^2 + \ddot{C}$, is regarded, according to the theory of substitutions, as a carbonic acid in which half of the oxygen is replaced by an equivalent of chlorine, $\dot{C}\,\underline{Cl}$. But one atom of $\dot{C}\,\underline{Cl}$ takes up two double atoms, or $C\,\underline{Cl}^2 + \ddot{C}$ takes up four double atoms of ammonia. But anhydrous carbonic acid can only combine with a double atom of ammonia, to form carbonat-ammon, $\ddot{C} + \underline{N}\,\underline{H}^3$, even when the carbonic acid gas is mixed with the greatest excess of ammoniacal gas. But since the combination contains just as much again of ammonia as according to the theory of substitutions it should, it follows likewise from its composition, that in the carbonate of the chloride of carbon the chlorine cannot be regarded as replacing oxygen.

The carbonate of the chloride of carbon fixes quite an analogous quantity of ammonia as the sulphate of the chloride of sulphur. If the combination was treated with water, its constitution might then be conceived, as consisting of chloride of ammonium and carbonat-ammon. Regnault looks upon it as a mixture of chloride of ammonia with a carbamide, $\dot{\underline{C}}\,\underline{N}\,\underline{H}^2$, without, however, having effected the separation of the constituents *.

* *Annales de Chimie et de Physique*, t. lxxix. p. 180.

Article III.

On the Composition of Stearic Acid, and the Products of its distillation. By Professor Redtenbacher *of Prague**.

[From the *Annalen der Chemie und Pharmacie*, vol. xxxv. p. 190, for July 1840.]

We owe our knowledge of stearic acid, as well as of other fatty bodies, to the classical labours of Chevreul, as made known to us in his *Recherches sur les corps gras*. Since that time no one has further investigated this acid, no new analysis of it has been published, and all subsequent investigations have been founded upon these first results. Nevertheless, chemical science generally, as well as the methods by which the constituents of organic bodies are ascertained, have since made very important advances, especially in those points which at that time were the most difficult.

If we read the methods by which Chevreul determined the composition of the fat bodies, and consider only the great number of estimations of volume, with the necessary corrections of each analysis, which were requisite, besides those numerous precautions which indeed are still indispensable, we can only wonder at the great perseverance and accuracy with which that great naturalist conducted so many analyses, independently of the high scientific results which his work afforded. But notwithstanding the great talents of the observer, exactly with these imperfect methods rested the greater probability of an error, which, though in itself small, could easily in the fats, with their high atomic weights, amount to from one to two atoms of a constituent. The researches themselves, as was at that time customary, do not furnish us with the actually observed numerical results, but only those numbers which were deduced from them, so that a close examination of them, as well as all reasoning respecting them, is impossible. Besides, the science at that time was not acquainted with the great accuracy as well as the general applicability of estimating the atomic weights of organic acids by means of their silver-salts.

* Translated by J. H. Gilbert, Ph. D.

In the first Number of the 33rd volume of the *Annalen der Chemie und Pharmacie*, Professor Liebig published a memoir on the state of our knowledge of the constitution of the fat acids, in which he showed its uncertainty and incompleteness, as well as what would probably be the result of a renewed investigation, and it was at his request that I undertook the following work in his laboratory.

The investigation of stearic acid was the first thing to be effected, for although the best known, there yet remained some doubt concerning the products of its distillation, and it at all events must be the basis of all ulterior inquiry.

In order to study the products of the distillation of stearic acid, that obtained from ox fat in the stearic acid manufactory of Merck, of Darmstadt, and which is a raw stearic acid, quite white, crystalline, fusible at 56° C.* (133° F.), and free from wax, was dissolved in alcohol and crystallized. The crystals first deposited were again dissolved in alcohol, and this treatment repeated seven or eight times, until by the expulsion of the alcohol by evaporation with water, solution in potash, and subsequent separation by means of hydrochloric acid, the melting point of the acid was constantly 70° C. (158° F.).

Although this investigation was undertaken under the firm conviction that the numerical results of Chevreul were correct, yet in order to place it beyond doubt that the acid submitted to distillation was really stearic acid, and identical with that of Chevreul, an ultimate analysis with oxide of copper was instituted.

Chevreul found 100 parts of the hydrated stearic acid to contain

Carbon	77·4200
Hydrogen	12·4312
Oxygen	10·1488

however, 0·624 gr.† of stearic gave to me 1·727 gr. carbonic acid,

* The melting point was here, as well as in all the succeeding cases, determined as follows: a glass tube was drawn out before a lamp until it was very thin and capillary, into this the melted substance was drawn up by the mouth; the tube thus charged and a thermometer were then placed in water of a moderate temperature, which was raised by degrees; finally, that point was taken as the melting point at which the whole of the contained substance became fluid, that is, transparent. By means of water of a somewhat lower temperature the point of solidification was rendered obvious, and this was commonly from 1° to 2° C. lower than that of fusion.

† *Gr.* in this case, as well as all those which follow in this memoir, refers to *grammes* and *not to grains*.

and 0·7275 gr. water, which represented in 100 parts is

I.	Carbon	76·53
	Hydrogen	12·95
	Oxygen	10·52

The incompatibility of these results induced me to repeat the combustions several times.

II. 0·5255 gr. gave 1·452 gr. carbonic acid, and 0·603 gr. water.

III. 0·363 gr. gave 1·010 gr. carbonic acid, and 0·429 gr. water.

IV. Of the same acid once more crystallized from alcohol, 0·302 gr. gave 0·8355 gr. carbonic acid, and 0·352 gr. water.

In order to ascertain whether other specimens of stearic acid had the same composition, an acid which was in the possession of Professor Liebig, and which had the same melting point, was submitted to combustion with oxide of copper.

V. 0·345 gr. gave 0·9495 gr. carbonic acid, and 0·3995 gr. water.

With the same object in view, an acid prepared in a similar manner from a soap of ox fat, and which melted at 70° C. (158°F.), was burnt with oxide of copper, when the following result was afforded:—

VI. 0·3145 gr. of the substance gave 0·871 gr. carbonic acid, and 0·3575 gr. water.

VII. 0·3055 gr. of the same substance when burnt with oxygen yielded 0·8485 gr. carbonic acid, and 0·3485 gr. water.

The following is a comparative view of these seven analyses represented in 100 parts:—

	I.	II.	III.	IV.	V.	VI.	VII.
Carbon .	76·53	76·40	76·93	76·82	76·64	76·57	76·79
Hydrogen	12·95	12·75	13·13	12·95	12·96	12·64	12·67
Oxygen .	10·52	10·85	9·94	10·23	10·40	10·79	10·54

The stearate of silver obtained by the precipitation of an alcoholic solution of the soda salt, by means of a solution of silver, was a white, very voluminous, flocky precipitate, which however floated on the surface of the liquid; the results of its analyses were as follows:—

0·6325 gr.	of this salt	gave of silver	0·182 gr.	=28·79	per cent.
0·9495	...	...	0·2705	=28·49	...
0·5825	...	...	0·1665	=28·58	...

or 2·1645 gr. of the silver salt gave 0·619 gr. silver, that is to say, 28·62 per cent., or 30·73 per cent. oxide of silver. The atom of the anhydrous stearic acid weighs therefore 6543.

Further, when burnt with oxide of copper:—

I. 0·288 gr. of the stearate of silver yielded 0·5715 gr. carbonic acid, and 0·2335 gr. water.

II. 0·2985 gr. of this salt yielded 0·595 gr. carbonic acid, and 0·2425 gr. water.

III. 0·371 gr. of the same compound gave 0·7385 gr. carbonic acid, and 0·3035 gr. of the substance gave 0·2455 water; or in 100 parts,

	I.	II.	III.
Carbon	54·90 . .	55·12 . .	55·04
Hydrogen . . .	9·01 . .	9·03 . .	8·99
Oxygen	5·36 . .	5·12 . .	5·24
Oxide of silver. .	30·73 . .	30·73 . .	30·73

The theoretical composition of the stearate of silver, therefore, as deduced from the above, is

68 equiv.	Carbon . .	5197·6 . .	55·15	per cent.
66 ...	Hydrogen .	823·6 . .	8·74	...
5 ...	Oxygen . .	500·0 . .	5·31	...
2 ...	Oxide of silver	2903·2 . .	30·80	...
	Stearate of silver	9424·4	100·00	

The anhydrous acid is therefore composed of

68 equiv.	Carbon . .	5197·6 . .	79·70	per cent.
66 ...	Hydrogen .	823·6 . .	12·63	...
5 ...	Oxygen . .	500·0 . .	7·67	...
		6521·2	100·00	

The hydrate of this acid, in which two equivalents of water replace the two equivalents of metallic oxide, consists therefore of

68 equiv.	Carbon . .	5197·6 . .	77·04	per cent.
68 ...	Hydrogen .	848·6 . .	12·58	...
7 ...	Oxygen . .	700·0 . .	10·38	...
		6746·2	100·00	

numbers which coincide perfectly with those experimentally determined.

Nothing surprised me more than the incompatibility of the composition thus found with that of Chevreul; however, the accurate coincidence of the numbers obtained in the various combustions with oxide of copper alone, or with oxide of copper in a stream of oxygen gas (and I have here quoted all that I made), even of differently prepared acids, but especially the exactness of the determination by means of the silver salt, must dissipate all doubt respecting their correctness, even supposing I had not succeeded in forming other compounds which accorded with it; since all other salts, either from the difficulty of obtaining them in a pure state, or from the lowness of the atomic weight of the base when compared with that of the acid, and especially from the possible loss in the determination of the atomic weight owing to the dense fumes which arise, must have given a less accurate result,—a result which, should it only differ from three to five tenths per cent., would give in an atomic weight of some thousands a difference which would amount to some hundreds, and thereby render impossible or doubtful all reasoning respecting the true composition.

The neutral salt of lead was prepared by precipitating a solution of sugar of lead acidulated with acetic acid, by means of an alcoholic solution of the soda salt.

1·139 gr. of the lead salt thus prepared gave 0·2655 gr. metallic lead, and 0·0505 gr. oxide of lead; this is equal to 29·55 per cent. of oxide of lead; by calculation the quantity should be 29·95 of oxide of lead.

Further, 0·405 gr. of the lead salt afforded 0·819 gr. carbonic acid, and 0·3265 gr. water; this represents in 100 parts,

	Found.	Equiv.		Per cent.
Carbon	55·90	68	5197·6	55·83
Hydrogen	8·96	66	823·6	8·85
Oxygen	5·59	5	500·0	5·37
Oxide of lead	29·55	2	2789·0	29·95
	100·00		9310·2	100·00

Finally, I further tried to prepare the compound with oxide of æthyl (æther); for this purpose I transmitted hydrochloric acid gas through a solution of stearic acid in absolute alcohol until it was saturated. The liquid was then slightly heated, mixed with warm water, and agitated with it until all hydrochloric acid was separated.

I obtained a beautifully colourless, strongly translucent, crystalline product, which was not very hard, incapable of being distilled, having scarcely a perceptible odour, and which melted at from 30° to 31° C. (86° to 88° F.). By boiling with potass and addition of hydrochloric acid, the acid (stearic) with its melting point unchanged, and alcohol were separated. When boiled with carbonate of soda white flocks appeared in the liquid, and the remaining æther gave no rational composition. The same decomposition occurred on repeating this treatment: 0·3045 gr. of the above æther, washed only with water, yielded when burnt 0·850 gr. carbonic acid, and 0·352 gr. water; or in 100 parts,

Carbon	77·17
Hydrogen	12·84
Oxygen	9·99
	100·00

These proportions represent a compound, in which one equivalent of water in the hydrated stearic acid is replaced by one equivalent of oxide of æthyl, that is, $\overline{St} + E\,O + Aq$, which gives the following composition:—

	Found.	Equiv.		Per cent.
Carbon . .	77·17	72	5503·3	77·49
Hydrogen .	12·84	72	898·5	12·65
Oxygen . .	9·99	7	700·0	9·86
			7101·8	100·00

I did not succeed in preparing the æther described by Lassaigne, $\overline{St} + 2\,E\,O$; I consider moreover such a composition as not very probable, but rather that there exists no neutral stearic æther, but only an *acid* compound of the above-mentioned composition, $\overline{St} + E\,O + Aq$, or

$$\left.\begin{array}{l}\overline{St} + E\,O + Aq \\ \overline{St} + 2\,Aq\end{array}\right\}$$

analogous to those of the tartaric and phosphoric acids, that is to say, to compounds of bibasic or polybasic acids.

But the results may also represent a compound similar to stearine itself, in which the glyceryl is replaced by æthyl.

	C.		H.		O.
2 equiv. Anhydrous stearic acid	136	. .	132	. .	10
1 ... Oxide of æthyl . . .	4	. .	5	. .	1
3 ... Water	0	. .	3	. .	3
	C 140		H 140		O 14

and from this the following results may be deduced:—

	Found.	Equiv.		Per cent.
Carbon .	77·17 . .	140 . .	10700·9 . .	77·27
Hydrogen	12·84 . .	140 . .	1747·1 . .	12·62
Oxygen .	9·99 . .	14 . .	1400·0 . .	10·11
	100·00		13848·0	100·00

The stearate of soda was prepared by boiling the hydrated stearic acid with an excess of carbonate of soda, expressing and drying the salt thus obtained; this was now dissolved in alcohol, the solution filtered, treated with water, evaporated and dried on a water-bath.

1·500 gr. of this salt gave 0·2685 gr. carbonate of soda, or 10·51 per cent. of caustic soda; calculation gives 10·70 per cent.

Again, 0·3855 gr. of the substance yielded 0·969 gr. carbonic acid, and 0·390 gr. water; these represent in 100 parts,

Carbon	69·49
Hydrogen	11·24

The theoretical composition requires

Carbon	71·16
Hydrogen	11·30

Now supposing the whole of the soda contained in the 0·3855 gr. of the substance = 0·040 gr. to have remained behind in the combustion tube as carbonate of soda, then it is evident that the quantity of carbonic acid thus remaining would be 0·029 gr., which would raise the per centage of carbon to 71·59, and thereby render the result erroneous; since, however, in combustion with oxide of copper the *whole* of the carbonic acid never remains behind, but nearly one third of it passes off, the per centage of carbon is only raised from 69·49 to 70·87, which also agrees with the theoretical composition.

	Found.	Equiv.		Calculated.
Carbon . .	70·87 . .	68 . .	5197·6 . .	71·17
Hydrogen .	11·24 . .	66 . .	823·6 . .	11·28
Oxygen . .	7·38 . .	5 . .	500·0 . .	6·85
Soda . .	10·51 . .	2 . .	781·8 . .	10·70
			7303·0	100·00

With respect to the products of the distillation of stearic acid, it had been supposed with Chevreul, that the acid passed over unchanged, for the product obtained had the same melting point as the undistilled acid. Chevreul, indeed, in his "Recherches," page 25–26, describes the process of the distillation as perfectly as possible; nothing escaped his notice, not even the evolution of carbonic acid and water in such minute quantities. The distillation yielded to me also, with the same phænomena, a product having a melting point the same, or at most one degree lower than that of the unchanged acid; it was besides as white and crystalline as that employed, but possessed a very weak empyreumatic odour.

This curious and scarcely to be expected circumstance in an ordinary decomposition, as well as the ignorance at that time of the existence of margarone, was doubtless the reason why Chevreul did not explain truly a process which he described so correctly. The substance obtained by distillation when boiled with water, communicated to it a weak acid reaction, without depositing sebacic acid on cooling, or yielding the reactions of that acid. [The source of sebacic acid I shall show in a future memoir.]

An analysis of this distilled acid was undertaken with the expectation that it would give the same results as stearic acid; however, 0·3025 gr. of the substance gave 0·842 gr. carbonic acid, and 0·335 gr. water, that is, in 100 parts,

Carbon	76·96
Hydrogen	12·25
Oxygen	10·79

a result which differs from that of stearic acid by a half per cent. of hydrogen, although no loss can be pointed out.

The silver salt of this distilled acid was prepared in the same manner as that of the pure stearic acid; its analyses yielded the following results:—

1·002 gr. of the salt gave	0·3065 gr. silver	= 30·59 per cent.	
1·001 ...	0·304 ...	= 30·37 ...	
0·999 ...	0·3045 ...	= 30·48 ...	

that is to say, 3·002 gr. of the silver-salt yielded 0·915 gr. silver, or 30·48 per cent., or 32·74 per cent. of oxide of silver.

0·2995 gr. of the same salt burnt with oxide of copper yielded 0·559 gr. carbonic acid, and 0·2285 gr. water; and 0·305 gr. of the substance gave 0·5755 gr. carbonic acid, and 0·233 gr. water.

These numbers indicate,

	Found.				
	I.	II.	Equiv.		Per cent.
Carbon	51·60	52·16	30	2293	52·03
Hydrogen	8·48	8·49	29	362	8·21
Oxygen	7·18	6·31	3	300	6·81
Oxide of silver	32·74	32·74	1	1452	32·95
				4407	100·00

The hydrate of this acid corresponding to C^{30} H^{30} O^4 would have the following composition per cent:—

Carbon	74·76
Hydrogen	12·19
Oxygen	13·04

These results are so discordant, that with the certainty that the same acid enters into the silver salt of the distilled acid as that submitted to combustion in the state of hydrate, it could no longer be doubted that the stearic acid suffers a change in its composition by distillation; it was on this account, and because these last analyses led to the discovery of the products of the distillation of stearic acid, that they have been given here.

When the soda salt strongly dried of the distilled acid is treated with æther, in which the salt itself is nearly insoluble, there remains behind, on evaporating the æther, a white crystalline substance, similar to paraffin, and an oil having an empyreumatic odour. The soda salt thus treated, however, gave, when decomposed by hydrochloric acid, an acid having a melting point of from 60° to 61° C. (140° to 142° F.), which could not be raised higher by crystallizing from alcohol, and which agreed in its other properties with margaric acid.

0·3138 gr. of the distilled acid thus purified gave 0·8625 gr. carbonic acid, and 0·355 gr. water; or in 100 parts,

	Found.	Equiv.		Calculated.
Carbon	76·00	34	2598·8	75·92
Hydrogen	12·57	34	424·3	12·39
Oxygen	11·42	4	400·0	11·69
			3423·1	100·00

Now this is precisely the formula and composition of hydrated margaric acid.

Again, 0·972 gr. of the silver salt of this acid gave 0·271 gr. silver, or 0·291 gr. oxide of silver, = 29·95 per cent. of the oxide.

And lastly, 0·303 gr. of the same silver salt gave 0·598 gr. carbonic acid, and 0·243 gr. water: we have, therefore,

Carbon	54·57	34	2598·8	54·57
Hydrogen	8·92	33	411·8	8·65
Oxygen	6·56	3	300·0	6·30
Oxide of silver	29·95	1	1451·6	30·48
			4762·2 *	100·00

The composition of this silver salt agrees perfectly with that of margaric acid.

Margaric acid, therefore, is formed by the distillation of stearic acid, or what is the same thing, from the acid of ox tallow that of human fat is obtained.

In order to arrive at a knowledge of the accompanying products of the distillation of stearic acid, a new quantity of that acid was distilled, and the matter obtained combined with soda, from which a lime salt, by means of chloride of calcium, was prepared, since the soda-salt by treatment with æther swelled up exceedingly. Boiling æther extracted from this lime salt the two substances above noticed, and from which, on cooling, the solid crystallized out. After frequent re-crystallizations from æther and expulsion of the latter, there remained a white substance of a mother-of-pearl lustre, scaly crystalline, and which, after being fused, was translucent, brittle, hard, melted at 77° C. (171° F.), and had the properties of margarone.

* Wherein the composition of margaric acid, as here estimated, differs from that given by Chevreul, is explained by Dr. Varrentrapp in his treatise on that acid, published also in the *Annalen der Chemie und Pharmacie*, for July 1840.

I. 0·305 gr. of this substance gave 0·9235 gr. carbonic acid, and 0·3815 gr. water; or in 100 parts,

Carbon	83·72
Hydrogen	13·90
Oxygen	2·38

The stearic acid from the manufactory of Dr. Merck gave the same substance when distilled with one fourth its weight of lime; the distilled product, after being freed from stearic acid, which had passed over undecomposed, by boiling with potass, and purified by crystallization from alcohol, had a melting point of 77° C. (171° F.).

II. 0·306 gr. of this substance gave 0·928 gr. carbonic acid, and 0·382 gr. water, that is, in 100 parts,

Carbon	83·86
Hydrogen	13·87
Oxygen	2·27

The same product crystallized from æther afforded the following results, a new crystallization being effected between each combustion, without changing the melting point.

III. 0·3025 gr. gave 0·9125 gr. carbonic acid, and 0·377 gr. water.
IV. 0·3015 ... 0·907 ... 0·375 ...

	I.	II.	III.	IV.
Carbon	83·72	83·86	83·50	83·18
Hydrogen	13·90	13·87	13·85	13·82
Oxygen	2·38	2·27	2·65	3·00

It is evident, therefore, how, by purification with æther, the per centage of carbon is reduced, leaving, as the second product of the distillation of stearic acid, a body having a composition similar to that of margarone.

33	equiv.	Carbon	2522·4	83·13	per cent.
33	...	Hydrogen	411·8	13·57	...
1	...	Oxygen	100·0	3·30	...
			3034·2	100·00	

By way of experiment some quite pure stearic acid was distilled with lime; the product, after purification with æther, however, had a melting point of 82° C. (180° F.), which, by numerous crystallizations, could not be changed, although in other respects its properties were the same.

0·305 gr. of this substance gave 0·924 gr. carbonic acid, and 0·379 gr. water, or per cent.,

Carbon	83·77
Hydrogen	13·81
Oxygen	2·42

If we consider this to be an independent body, and suppose empirically the quantity of oxygen to be equal to one equivalent, then we obtain

	Found.	Equiv.		Per cent.
Carbon	83·77	45	3439·6	83·38
Hydrogen	13·81	45	561·6	13·68
Oxygen	2·43	1	100·0	2·44
	100·00		4101·2	100·00

which is equivalent to

Margarone	$C^{33} H^{33} O^{1}$
Polymeric carburetted-hydrogen . .	$C^{12} H^{12}$
	$C^{45} H^{45} O^{1}$

and such a view renders evident its constitution, but not its elevated melting point; the existence of the stearone of Bussy is therefore much to be doubted, for stearone should be formed from stearic acid by the subtraction of carbonic acid; but stearic acid, by the splitting of its atom into two new compounds, is resolved into margaric acid, and it is not very probable that both these decompositions could take place simultaneously.

Lastly, the empyreumatic oil which accompanies all the products of the distillation of stearic acid, and which, after the crystallization of the margarone, remains behind in the æther, is obtained by evaporating the latter, contaminated with margarone, of which it dissolves much. By rectification and division of the product which passes over, it may be obtained in the first portions nearly free from margarone.

I. 0·3145 gr. of this oil gave 0·9685 gr. carbonic acid, and 0·3985 gr. water.

II. 0·2425 gr. of the same body gave 0·7465 gr. carbonic acid, and 0·3095 gr. water.

	Per cent. I.	Per cent. II.	Equiv.		
Carbon	85·15	85·12	1	76·44	85·96
Hydrogen	14·08	14·18	1	12·48	14·04
Oxygen	0·77	0·70			
				88·92	100·00

Thus all the products of the distillation of stearic acid have been determined, and the result is as follows:—

The atom of stearic acid is resolved by distillation into hydrated margaric acid, margarone, water, carbonic acid, and a carburetted hydrogen.

The evolution of carbonic acid in so small a quantity had already been noticed by that acute observer Chevreul, as well as the formation of water, which, from time to time, during the whole distillation, gives rise to small explosions, even when both the retort and substance employed are perfectly dry, thus indicating the sudden formation of steam.

At the same time the nearly unchanged melting point of the product of the distillation of stearic acid is explained, notwithstanding margaric acid, fusible at 60° C. (140° F.), was formed, for it is the margarone, fusible at 77° C. (171° F.), which so elevates the melting point.

In contact with lime a body is formed having a melting point of 82° C. (180° F.); its formation, however, is perfectly explained, if it be remembered that it contains the constituents of margarone, plus those of a carburetted hydrogen.

These results, therefore, are quite analogous to those ordinarily observed in processes of distillation; the atom of a bibasic acid is resolved into water, and a monobasic acid which, still further decomposed, gives carbonic acid and a body similar to acetone; and lastly, there is formed a carburetted hydrogen, as the last product of the splitting of the organic atom in the absence of oxygen. The foregoing suggests the following scheme:

6 equiv. Hydrated margaric acid	$C^{204} H^{204} O^{24}$
1 ... Water	$H^{1} O^{1}$
1 ... Margarone	$C^{33} H^{33} O^{1}$
1 ... Carbonic acid	$C^{1} O^{2}$
Polymeric carburetted hydrogen	$C^{34} H^{34}$
4 equiv. Hydrated stearic acid	$C^{272} H^{272} O^{28}$

The whole process is represented in the foregoing, as analogy with similar methods of decomposition appears to indicate. The results found are not at variance with the idea that a margarone, that is, $\overline{Mg} - CO^2$, a body analogous to acetone, is formed; since, however, margarone enters into no known compounds, and as owing to the decomposition which it undergoes by distillation the determination of the specific gravity of its vapour cannot be effected, there is no positive proof of its constitution, nor is there any reason to prevent a different view being taken, especially if such be rendered probable by future considerations.

From the composition of the body termed margarone, we know with certainty that it contains less oxygen than the acid from which it is derived; but, if we consider the relation of the carbon to the hydrogen alone, we observe no difference in the relative proportions in which both these are found in the acid. We are acquainted with analogies also of other kinds, which justify us in comparing this manner of decomposition with others.

Analysis gives us only numerical results, the relative proportion by weight of the constituents of a body, which are valuable in science only inasmuch as by this means an idea can be expressed.

Stearic acid contains the same number of equivalents of oxygen as the hypo-sulphuric acid; we know that this latter when submitted to a high temperature is decomposed, the oxygen being divided with the radical, and we obtain as the products of the decomposition a lower and a higher oxide. The hypo-sulphurous acid behaves itself in a similar manner.

We see then that by the decomposition of stearic acid by the aid of heat, a body (margaric acid) is formed, which, for equal weights, contains more oxygen; we know, therefore, that the oxygen of the stearic acid has been divided, and that in consequence of this division a lower oxide of the same radical may have been formed.

From the results recorded we infer that stearic and margaric acids contain equal proportions of carbon and hydrogen, in such a manner that the latter may be represented by $(C^{34} H^{33}) + O^3$, and the former by $2 (C^{34} H^{33}) + O^5$. In these formulæ we cannot but recognize the existence of two oxides of the same radical, a radical which Berzelius* had already anticipated and spoken of as probable, and the above two oxides of which he compared with hypo-sulphuric and sulphuric acids, with chloric and hyper-chloric acids, and whose compositions only vary one equivalent in the carbon and hydrogen.

If, therefore, this radical $C^{34} H^{33}$, which may be called *Margaryl*, be represented by R, then margaric (margarylic) acid will be $= R + O^3$, and stearic (hypomargarylic) acid $= 2 R + O^5$.

The existence of such a radical is rendered more probable, if we consider the conversion of the one oxide into the other, as above shown; and thus viewed the composition of margarone can be otherwise represented; it can be considered as the lowest oxide of the same radical; its formula in that case would be $C^{34} H^{33} O = R + O$. This formula is identical with that at which Bussy arrived in his analysis.

The process of the distillation of stearic acid is therefore to be explained in a very simple manner, for it contains the elements of margaric acid, and the body margarone :—

3 equiv. Hydrated margaric acid	$= C^{102} H^{102} O^{12}$
1 ... Margarone	$= C^{34} H^{33} O^1$
1 ... Water	$= H^1 O^1$
2 equiv. Hydrated stearic acid	$= C^{136} H^{136} O^{14}$

and the decomposition of margaric acid, when distilled either with or without lime, is as clear :—

1 equiv. Margarone . . .	$= C^{34} H^{33} O^1$
2 ... Carbonic acid . . .	$= C^2 \quad O^4 + 2 CaO$
1 ... Water	$= H^1 O^1$
Polymeric carburetted hydrogen	$= C^{32} H^{32}$
2 equiv. Margaric acid . .	$= C^{68} H^{66} O^6$

In an experiment made by Chevreul respecting the nature and quantity of the gases developed during the distillation †, it appeared that 1000 gr. afforded about three gr. carbonic acid; according to this 26,984 gr. = 4 equiv. stearic acid would give 81 gr. carbonic acid; whilst according to the first formula here given, 276 gr. carbonic acid $= 1 CO^2$; and according to the se-

* *Lehrbuch der Chemie*, 3te Auflage, 6ter Band, S, 543; or *Traité de Chimie* (Brussels edition), vol. ii. p. 497.

† Recherches, p. 25.

cond, 1656 gr. carbonic acid $= 6CO^2$, must have been evolved. It is evident that this evolution is much greater than in the experiment of Chevreul, which indicates only one third of the quantity of carbonic acid of the first, and only $\frac{1}{20}$th of that of the second calculated formula. It may therefore with some probability be concluded, that the evolution of carbonic acid, and the formation of margarone, do not stand necessarily in connexion with each other.

There is formed therefore margarone, $C^{34} H^{33} O$, without the evolution of carbonic acid, and it is certain that the carbonic acid is to be attributed to a further decomposition; it is also evident that the carbon which remains behind stands in a very definite relation to it, since margarone, when distilled by itself, also leaves behind carbon; in consequence, however, of such a separation of carbon, a compound containing oxygen must have been formed.

Lastly, it is not uninteresting to observe, that according to the last method of deduction, the carburetted hydrogen which presents itself, together with 1 equiv. water, gives exactly the composition of anhydrous æthal; for this carburetted hydrogen, $= C^{32} H^{32} + Aq$, contains the elements of æthal:—

$$\text{Anhydrous æthal} = C^{32} H^{33} O.$$

It is known that spermaceti, when distilled, yields no æthal (Bussy and Lecanu), from which it is evident, that in the anhydrous condition it is decomposed under the influence of a high temperature; it is therefore in the highest degree probable, that the carburetted hydrogen is identical with cetene.

The existence of a common radical in the two most prevalent fat acids, must be of great importance in the theory of organic compounds.

In a memoir published by M. Bromeis in *Der Annalen der Chemie und Pharmacie* (Part I. vol. xxxv.), he has elicited the strongest proof of the identity of the radical, in the behaviour of stearic acid with nitric acid, by which, on the first action the whole mass is immediately changed into margaric acid, namely, into an acid which melts at 60° C. (140° F.), and possesses the composition and all the other properties of that prepared from human fat.

His experiments were repeated by myself and perfectly corroborated.

I have to add to this proof a new one, in the behaviour of stearic acid towards the sulphuric and chromic acids: if it be heated after the addition of a little water, the mixture becomes green, owing to the oxide of chromium formed, and a part of the stearic acid is found changed into margaric acid.

The new acid thus obtained melts at from 64° to 65° C. (147° to 149° F.); however, it can be obtained by crystallizing from alcohol, from which the stearic acid first separates, having a melting point of from 59° to 60° C. (138° to 140° F.), which therefore agrees with that of margaric acid.

Article IV.

Action of Sulphurous Acid on Hyponitric Acid (peroxide of nitrogen); crystals of the leaden chamber; theory of the formation of Sulphuric Acid. By M. F. De la Provostaye, *Professor in the College of Louis-le-Grand*.*

[From the *Annales de Chimie et de Physique*, t. 73.]

NOTWITHSTANDING the numerous investigations which have been made to elucidate the theory of the formation of sulphuric acid, chemists are by no means agreed respecting it. The principal difficulty appearing to exist in the study of the intermediate products, these have been made the subject of a special examination by MM. Clement and Desormes, by Gay-Lussac, Henry, Berzelius, Bussy, and Gaultier de Claubry†. The same products form likewise the subject of the present memoir. It will therefore be necessary to revert briefly to the results of anterior researches; results, some of which are incontrovertibly established, and others which are not altogether satisfactory. At the time of publication of the memoir of MM. Clement and Desormes‡, the combinations of oxygen with nitrogen were imperfectly known. The importance of their function in the formation of sulphuric acid was amply demonstrated by these chemists; but they erred in regarding the crystalline compound which is produced in this case as composed of sulphuric acid and nitric oxide. When decomposed by a small quantity of water in an atmosphere of carbonic acid, it gave rise to red vapours; a fact irreconcilable with that hypothesis. It is to Gay-Lussac that we are indebted for this important experiment (1816). That illustrious chemist arrived at the conclusion, that the crystals in question contained sulphuric acid, united to the acid existing in the nitrites. This conclusion was a necessary one; for at that time no other degree of oxidation of nitrogen, but nitrous acid, was known between nitric oxide and nitric acid. Further, this constituent of the crystals was considered identical

* Translated by Mr. E. A. Parnell.

† To these may be added a recent paper by Adolph Rose, on the Combination of the Hydrate of Sulphuric Acid with Nitric Oxide, a translation of which appeared in the Philosophical Magazine for February, 1841, p. 81.—Ed.

‡ *Annales de Chimie*, t. 59. 1805.

with the red vapours produced in the distillation of dry nitrate of lead; and when these vapours were put in contact with sulphuric acid, crystals were soon produced; an apparently convincing proof of the justness of the proposed views. But shortly afterwards a clear distinction was drawn between nitrous acid ($N\,O_3$) and hyponitric acid ($N\,O_4$) by M. Dulong, when it became necessary to decide which of these two acids was contained in the crystals. The experiment just cited produced a general admission of their containing hyponitric acid. More lately, Dr. William Henry* analysed some crystals found in a pipe employed in renewing the air in the leaden chambers; his analysis gave

Dry sulphuric acid .	68·800
Nitrous acid	13·073
Water	18·927

Assuming the identity of these crystals with those of the chamber, he was induced to give them the formula $N\,O_3$, $5\,S\,O_3 + 5\,H\,O$. The above numbers indeed agree better with six equivalents of water, but perhaps the author considered an excess of water would be probable in the analysis of so highly a hygrometric substance. A little later, M. Berzelius, in his *Traité de Chimie*, and M. Bussy, in a note inserted in the 16th vol. of the *Journal de Pharmacie* (1830), supported the results of this analysis; the one, by showing that sulphuric acid completely absorbed a mixture of nitric oxide and oxygen, when in the proper proportions to form nitrous acid, but left a residue undissolved if the proportions were different; and the other, by the observation that liquid hyponitrous acid introduced into a tube containing sulphuric acid, is decomposed into nitrous acid which is absorbed, and nitric acid which is diffused in white and pungent fumes.

In the same year, M. Gaultier de Claubry made an analysis of the crystals produced by the combination of sulphuric acid and hyponitric acid under the influence of water. He found in them—

Anhydrous sulphuric acid	5 equivalents.
Nitrous acid	2 equivalents.
Water	4 equivalents.

His numbers, however, really give $3\frac{1}{2}$ equivalents of water.

* Annals of Philosophy, May 1826.

Such are, so far as I am aware, the only observations which have been made on this subject. They appear to agree in establishing that the white crystals contain nitrous acid, sulphuric acid and water. But as to the proportions, important as the determination of this point is, they still leave much doubt, which, as we shall see in the sequel, is not very surprising.

This uncertainty is even increased by the recent progress of the science. As yet, the combination of dry sulphurous acid with dry hyponitric acid has not been obtained. Is this combination possible? We are led to believe that it is, and to class it with

Sulphuric acid	$S\,O_2 + O$.
Chlorosulphuric acid of Regnault . .	$S\,O_2 + Cl$.
Iodosulphuric acid	$S\,O_2 + I$.

and lastly,

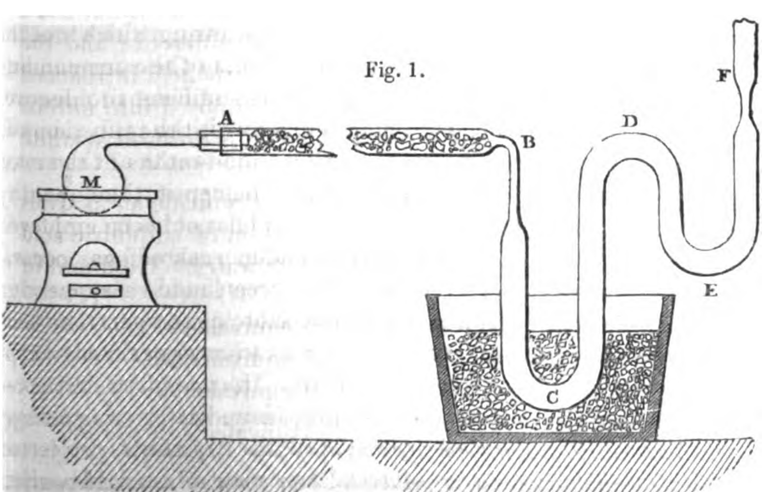
Nitrosulphuric acid of Pelouze . . .	$S\,O_2 + N\,O_2$.

This hypothesis is sanctioned by M. Dumas.

It is my intention now to examine these opinions by the institution of some new experiments.

That dry sulphurous acid gas has no action on the red vapours, also dry, is a fact with which chemists have been long acquainted. But the elasticity of gases is an obstacle to their union: it becomes then important to expose these substances to each other in the liquid state. To effect this, the following arrangement was adopted. Mercury and sulphuric acid were introduced into a small retort M fig. 1., the neck of which was con-

Fig. 1.



nected with a tube A B more than a metre in length, containing recently calcined ignited chloride of calcium. The other extremity of this tube was bent twice at C and E, as represented in the figure. The first bend C was surrounded with a freezing mixture; in this the sulphurous acid was condensed. The hyponitrous acid was introduced into the second bend E. The tube was drawn out at its extremity, and terminated by a little funnel F. The hyponitric acid for the experiment was procured by distilling well-dried nitrate of lead, and was received in a U-shaped tube, surrounded by a freezing mixture. The apparatus being arranged, the retort M was detached, and the hyponitric acid poured in by the funnel F until the volume of the liquid in E nearly equalled that of the sulphurous acid condensed in C. The extremity B was then fused and sealed before the blowpipe; F was at the same time drawn out, and the two liquids mixed: the drawn-out point was immediately opened under an inverted jar of mercury.

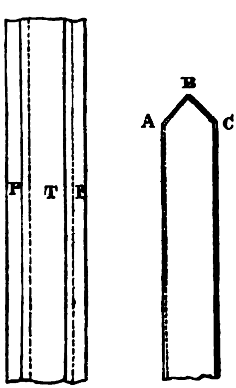
The two liquids have no sensible reaction, each resuming the gaseous state under the jar. Minute traces only of a white matter are observed attached to the sides of the tube. Thus it appears that these two bodies, when perfectly dry (at least under ordinary circumstances), do not combine in the liquid state. But if the tube be sealed at both the extremities, B and F, and the two liquids be afterwards mingled, the mixture is presently perceived to become green and turbid. By degrees a light-yellowish white deposit makes its appearance, which gradually increases for twenty-five or twenty-six hours, during which time the temperature of the tube is always above that of the surrounding bodies. The simple touch of the hand is sufficient to discover this. A small thermometer, not in contact with the tube, showed a difference of several degrees. About nine-tenths of the mass solidify. A green liquid remains above the deposit, the quantity of which is small when the hyponitric acid has not been employed in excess. On opening the tube, a sudden rush of gas occurs, which is sometimes very violent; the green liquid disappearing and diffusing red vapours. It is advisable not to open the tube until the expiration of three days, and to have previously kept it for some time in a freezing mixture. In one of the first preparations of this substance, the tube, immediately on opening, was broken in a thousand pieces, and the fragments projected with so much force, as to pierce a large pane of a neighbouring window in three different places. In two other operations, the tubes burst spontaneously in the place where they were deposited: but there is reason to believe that in these two instances the tubes had some defect, or were not hermetically sealed.

It is thus seen, that under the influence of the strong pressure which is necessary to preserve these two bodies in the liquid state, time being allowed, they can react on each other, even when perfectly dry. What then is formed in the reaction? That the combination is not effected without the previous decomposition of one of the bodies, the formation of the green liquid (which now presents itself as the subject of examination) would seem to indicate. As the collection of this liquid is attended with some difficulty, it becomes necessary to examine the solid product. Great difficulties are also presented here, arising from its extraordinary affinity for water; in consequence of which it will be necessary to detail the various successful precautions which have been adopted to avoid this source of error.

In order to obtain a pure, homogeneous, and always identical substance, it is necessary to commence by fusing the matter. The tube is opened by breaking the point, on which there is an immediate explosion; if the proper precautions, however, be adopted, it is very feeble. The extremity F is again to be sealed with a blowpipe, and the apparatus transferred to an oil-bath, the temperature of which is observed by one or two thermometers. At about 120° cent. (248° Fahr.) the tube is to be reopened. Red vapours escape for a few moments; they need not be again intercepted, as the substance does not undergo the smallest alteration. Continue to apply heat with care until fusion occurs, which commences at 217° cent. (423° Fahr.). The upper part of the oil-bath possessing an inferior temperature, the whole is not melted till the thermometer attains 230° cent. (446° Fahr.). The same points have been repeatedly observed by alternately depressing and elevating the temperature. The solidification on cooling takes place also about 217° cent. (423° Fahr.). This point is more difficult to determine than the fusing point, because the substance, which is opake when solid, becomes transparent in the liquid state, and remains so after solidification; occasionally even until 190° cent. (374° Fahr.). If the substance has been previously exposed for some time to a moist atmosphere, it melts at an inferior temperature; but this property will be again considered.

On continuing to apply heat, the substance sublimes. This happens near the boiling point of mercury, but this point has not been exactly determined. In every instance it distils without alteration, and condenses perfectly white and pure, very close to the heated portion of the tube. The colour of this substance changes rapidly with its temperature. When distilling, it is nearly as red as liquid hyponitric acid. At about 220° or 230° cent. (428° or 446° Fahr.), the liquid is yellow, possessing an oleaginous appearance, both in consistence and colour. Shortly after solidification, when it becomes opake, it acquires a very fine canary-yellow colour. This tint becomes paler, and at a low temperature the substance is presented in brilliant white silky tufts. By means of a microscope, the crystalline form is perfectly visible. In general, the crystals seem to be finer and more distinct in proportion to the rapidity of cooling. They appear to be rectangular four-sided prisms P, two opposite sides being truncated by a pair of faces T (fig. 2.). It is seldom that the summits of these flat and very elongated prisms can be distinguished. In one case, however, four or five adjoining crystals were discovered, the summits of which were well defined; the two sides A B and B C forming a right angle. For the remainder, it was impossible to distinguish whether their extremities were composed of two or four faces. We shall afterwards revert to this crystalline form.

Fig. 2.



To make an analysis of this substance, it is necessary to transfer it to another tube. This operation is accompanied with some difficulty, if the absorption of water is to be entirely prevented; it may, however, be performed without risk in the following manner. Fuse a small portion by means of a lamp, and cause it to flow into the drawn-out beak of the tube which contains it*; then break the point and direct it into a small well-dried tube previously weighed. Apply heat; the liquid flows from one tube into the other. When all the separated portion

* This substance, when anhydrous, may be fused and volatilized in a sealed tube without danger, but that is not the case when it contains water.

is thus introduced, seal the tube by means of a lamp, which must be done so expeditiously that nothing can possibly be absorbed. Another weighing gives the weight of the substance introduced. This method of operation must always be adopted when a known weight of the material is required.

The estimation of the sulphur in this compound is made without difficulty. Take an excess of diluted ammonia, and drop into it a tube containing a known weight of the substance to be analysed, the point being broken. The reaction is very violent; cover the vessel, that portions of the liquid may not be lost by the effervescence of the nitric oxide; nothing but this gas then escapes. The excess of ammonia is afterwards evolved by boiling. When the solution is neutral, precipitate, while warm, by chloride of barium, being careful to have a very slight excess. The precipitated sulphate of barytes contains no sulphite.

1st. 1·881 gramme of substance gave 3·683 grammes of sulphate of barytes, equal to 27·00 sulphur per cent.

2nd. 0·42 gramme gave 0·798 of sulphate of barytes, equal to 27·36 per cent.

Mean of the two experiments gives 27·18 sulphur.

The nitrogen was estimated by two methods; by decomposing the compound by copper, and by mercury.

For this purpose the apparatus employed in organic analysis was employed, but the oxide of copper was almost entirely replaced by other materials. At the bottom of a long green glass tube (sealed at one end), a sufficient quantity of pure carbonate of lead is placed; on this a column of recently reduced copper, of seven or eight centimetres in length, and then a small tube containing a known weight of the substance, which, on being opened, is to be filled with copper turnings. The open end is directed towards the bottom of the analysing tube; one or two centimetres of oxide of copper and twenty or twenty-two of metallic copper are next introduced, and the tube closed with a good perforated cork, which receives a chloride of calcium tube, also connected with a bent tube terminating under an inverted jar of mercury. Heat is first applied to the carbonate until the gas it evolves is completely absorbed by potash; the copper in the anterior part of the tube is then heated strongly, and afterwards that in the posterior part; but the substance to be analysed must be protected as much as possible from the action of heat until the whole tube has attained a full red heat. When this is effected, some pieces of burning charcoal are held near the substance, and gas is almost immediately disengaged with rapidity. The operation is terminated by increasing the heat, and evolving more carbonic acid, until the volume of gas in the gas receiver, which must contain a strong solution of potash, is not sensibly increased. Of three attempts to estimate the nitrogen by this means, two have been slightly defective, in consequence of the column of copper not being sufficiently long. I have assured myself that, at least in one of these instances, traces of nitric oxide were present in the nitrogen. These two attempts, the details of which I do not communicate, have afforded 11 and 11·2 per cent. of nitrogen. It was after these imperfect endeavours, that the length of the column of copper was increased to twenty-two centimetres, and copper was also introduced in the small tube. The third experiment succeeded perfectly. The numbers obtained were as follow :—

0·624 gramme gave 63·73 cubic centimetres of nitrogen gas, under a pressure of 0·746 metre*, and temperature of 16° cent. (60° Fahr.). The corrections being made, we obtain 58·08 cubic centimetres of dry gas at 0° cent. (32° Fahr.), and under a pressure of 0·76 metre. Adopting 0·975 for the density of nitrogen, this number will afford us 11·79 per cent. of this element.

The water was determined by a separate experiment; the tube not having been sufficiently dried in the preceding, water was observed to condense in the cool part of the tube previous to the decomposition of the substance. The whole water, however, collected does not amount to so much as one equivalent.

0·734 gramme of substance, decomposed in the same manner, gave

Water . . . . 0·01 gramme,

so minute a quantity as to be entirely neglected. We have then as the composition of this substance,

Sulphur	27·18
Nitrogen	11·79
Oxygen	61·03
	100·00

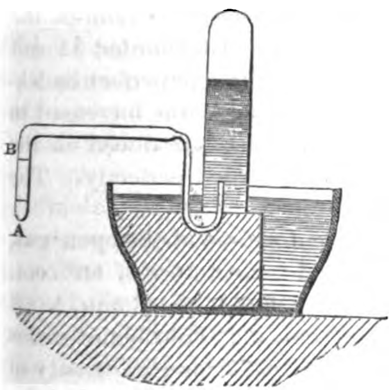
which numbers correspond very closely with the formula $S\ O_2, N\ O_4 + S\ O_2\ O$, the per centage composition for which is

* The metre is equal to 39·3710 English inches.

2 Sulphur	. . . .	27·18
1 Nitrogen	. . . .	11·96
9 Oxygen	. . . .	60·86
		100·00

The nitrogen was also determined by a second process. A quantity of the substance was introduced in a tube previously weighed (fig. 3.), which was then bent, drawn out, sealed, and again weighed. The point was afterwards broken, and pure and cold mercury introduced in A B; the extremity of the tube was then placed under an inverted jar of mercury, as represented in the figure. The whole thus arranged, the heat of a lamp was applied to the substance and the mercury. These two bodies, although without action on each other when cold, react strongly if heated, a mixture of nitric oxide and sulphurous acid being disengaged. The operation is completed by a very slight elevation of temperature; sulphate of mercury remaining in the tube.

Fig. 3.



The analysis of the gaseous mixture was attempted by several processes. Water, potash, borax, and peroxide of lead gave unsatisfactory results; this was ascertained by preliminary experiments, and might have been anticipated after the observations of M. Pelouze. Potassium has afforded me better success. In a preliminary experiment on a mixture of sulphurous acid and nitric oxide in known proportions, very near the corresponding volume of nitrogen was obtained. The residue was, however, a little too great, depending, perhaps, on some impurity of the gases. The following results were obtained by this process:—

0·501 gramme of substance heated with mercury, afforded 125·43 cubic centimetres at 21° cent. (70° Fahr.), and under a pressure of 0·764 metre; about 4 cubic centimetres more remained in the little retort, the capacity of which was 5 centimetres. The volume, reduced to 32° Fahr. and 0·76 metre, gives 121 cubic centimetres, but these 121 cubic centimetres contain 4 cubic centimetres of nitrogen, arising from the air of the small retort.

A quantity of this gas, which occupied 168 measures of a small graduated tube, was treated with potassium in excess; the residue occupied 72 measures. About one-thirtieth of this gas is excess of nitrogen, we shall therefore have for 162·4 measures, a residue of 66·4; or of 47·83 cubic centimetres of nitrogen for 117 cubic centimetres of the mixed gases. This number affords

Nitrogen . . . 12·02 per cent.

According to the same experiment, 0·501 gramme furnished 95·66 cubic centimetres of nitric oxide, and consequently 21·34 of sulphurous acid. Calculated according to the formula above given, 23·3 should have been obtained. But this accordance is as satisfactory as can be expected.

Properties.—This substance produces a deep red colour when applied to the skin, or rather the part touched appears as if deeply bruised. The tint quickly becomes yellow, and disappears in a few moments, leaving only a faint black shade. Exposed to the air, it gradually decomposes, absorbing water, and evolving hyponitric acid. In contact with a quantity of water, it immediately causes an abundant evolution of pure nitric oxide. The solution of sulphuric acid which remains, continually evolves the odour of this gas, and must consequently retain a considerable portion. Indeed, 0·662 gramme of the substance gave no more than 32 or 33 cubic centimetres of nitric oxide; while, assuming that all the nitrous acid formed from the compound ($S\,O_2\,O$, $S\,O_2\,N\,O_4$) is decomposed into nitric acid and nitric oxide, 83 cubic centimetres ought to have been collected. The hydrated bases also decompose this substance. Anhydrous baryta has no action while cold, but at an elevated temperature it suddenly becomes incandescent. Red vapours appear in the tube, and sulphate of barytes remains.

The action of dry ammoniacal gas has not been sufficiently studied. I have merely ascertained that a very hard white crust is formed on the surface of the substance, which prevents further absorption.

The body produced appears to be a species of sulphamide: no trace of decomposition is perceptible.

If a current of dry ammoniacal gas be passed through the melted substance, a large quantity of nitrogen is disengaged;

while a white substance, presenting the properties of sulphate of ammonia, remains.

Nitric, oxalic, and acetic acids decompose this substance. Hydrochloric acid gives a species of aqua regia. Concentrated sulphuric acid is without action on it in the cold; advantage of which property has been taken to obtain its density in the solid state: it is 2·14. Aided by heat it dissolves in sulphuric acid without decomposition; but the mixture is not immediately effected, and the difference in density is such that the acid floats some time on the fused substance. When the mixture is complete, it possesses a greenish-yellow colour. The production of this colour is so very sensible a character, that traces of this substance in sulphuric acid may be recognized by its occurrence. The two substances are not separated by heat, both distilling together. If the sulphuric acid be in large excess, the whole is liquid and colourless while cold; in the contrary case, the mass is solid, semi-transparent, almost colourless, and retains only a slight tint of yellow, with more or less green. In the hydrated state, or already combined with sulphuric acid, it dissolves, however, in sulphuric acid in the cold.

The solution can be effected in all proportions, and what is very remarkable, in returning to the solid condition it uniformly produces crystals, possessing great analogy in form. The larger the proportion of sulphuric acid in the crystals, the more fusible they are. The melted mass is commonly very viscid, and it is doubtless for this reason, that when once liquid, it is very tardy in resuming the solid state. One of these solutions, which melted at about 60° cent. (140° Fahr.), remained liquid at a temperature which was not above 10° cent. (50° Fahr.). When agitated, solidification is determined; the temperature rises considerably, as might be expected, and small, heavy white tufts, suddenly appear dispersed through the liquid, and remain suspended in the place where they were produced. The presence of one of these determines the rapid formation of many others. When the pure substance is exposed to air, it attracts moisture from all parts of its surface. The azotized compound is partly destroyed at the surface, sulphuric acid remaining; but the substance is so compact, and so feebly acted on by the free sulphuric acid, that the quantity of the latter augments very slowly, and only until the acid has absorbed a certain quantity of water. On heating, a homogeneous mixture is obtained, forming a solution quite similar to that which I have mentioned above. It would appear that within certain limits water replaces the nitric oxide which is evolved, in nearly equal weights. 3·209 grammes of substance fusing at 217° cent. (422° Fahr.) were introduced into a small tube, in which the air circulated with difficulty. After seven or eight hours the weight had not sensibly varied, although nitric oxide was continually evolved. At the expiration of about twenty hours there was an increase of 5 or 6 milligrammes; but this arose from the thin stratum of sulphuric acid absorbing a little water; for on heating until fusion occurred, which took place at 160° cent. (320° Fahr.), the weight became precisely as at first, that is, 3·209 grammes; doubtless because the water decomposed one portion of the substance, disengaging a quantity of nitric oxide exactly equal to its own weight.

With indigo the sulphuric solution gives a series of magnificent colours, provided it is sufficiently concentrated; a green in the cold, which, on heating, changes to a rose-colour, and then through every possible shade to purple, more or less deep.

The composition and properties of this substance once well established, it is very easy to comprehend the reaction which has given birth to it. This can, in fact, be represented by the following formula:—

$$2\,(N\,O_4 + S\,O_2) = \begin{cases} S\,O_2\,O \\ S\,O_2\,N\,O_4 \end{cases} \text{and } N\,O_3.$$

It is therefore nitrous acid, which, on opening the tube, assumes the gaseous state with such violence as to shatter the vessel, if care is not taken to adopt the necessary precautions. It is also nitrous acid, combined with hyponitric acid in excess, which forms the green liquid of which we have previously spoken. It exhibits, in fact, all the properties which M. Dulong observed that acid to possess (*Ann. de Ch. et de Ph.*, t. ii. p. 323).

As to the rational formula, it may either be

$$N\,O_3 + 2\,S\,O_3, \text{ or } \begin{cases} S\,O_2\,N\,O_4 \\ S\,O_2\,O. \end{cases}$$

It may be advanced against the last view,—1st, that if sulphurous acid be found partly in a state of combination with oxygen and partly with hyponitric acid, it is not apparent why, on presenting two liquids to each other, capable of combining without decomposition, there is formed anhydrous sulphuric acid.

2nd. If a mixture of nitric oxide and oxygen gases, in proper proportions to constitute nitrous acid (or a mixture of hyponitric acid and nitric oxide) be placed in contact with sulphuric acid, crystals may be obtained. This method, and all others which I can describe, repose on this fact, that the elements of sulphuric acid and nitrous acids, being placed in contact, determine the formation of white crystals. Now, how can the existence of sulphurous acid in a body thus formed be admitted? Is it possible that the nitrous acid deoxidizes the sulphuric acid? There is, however, a radical identity between these crystals and those which have been particularly described in this paper; for the first dissolve without decomposition in sulphuric acid in the cold, and when heated the crystals become coloured precisely as the anhydrous compound. This last, dissolved in the proper quantity of sulphuric acid, crystallizes on cooling. The crystals offer very diversified forms, but the arrangement represented in fig. 2. can always be distinguished. Now, in preparing crystals by all the various processes, and fusing them, they affect precisely the same form on crystallizing.

To these arguments it may be replied, in the first place, that the body under consideration is the most stable compound which can be produced in the reaction. Indeed, the substance $S\,O_2 + N\,O_4$, which cannot be exhibited except in combination with $S\,O_2\,O$, reminds us of a combination, the discovery of which is due to M. Henry Rose, and which he has designated by the name of sulphate of perchloride of sulphur; representing its composition by the formula $S\,Cl_6 + 5\,S\,O_3$. The analogy of these two combinations will be rendered more apparent by writing this last formula in the manner $S_6\,Cl_6\,O_{15}$, or $S_2\,Cl_2\,O_5$, or lastly,

$$\begin{cases} S\,O_2,\ Cl \\ S\,O_2,\ O, \end{cases}$$

which exactly corresponds with

$$\begin{cases} S\,O_2,\ N\,O_4 \\ S\,O_2,\ O. \end{cases}$$

It might be expected, according to this manner of viewing it, that under the influence of anhydrous sulphuric acid, dry sulphurous and hyponitric acids should unite without mutual decomposition; this has, in fact, been verified in the following manner:—

In one of the double bent tubes employed in the preparation of the substance (fig. 1.), the vapour of anhydrous sulphuric acid was condensed. Liquid sulphurous and hyponitric acids were prepared separately, each perfectly dry. The sulphurous acid was first poured on the anhydrous sulphuric acid in the bend C; and afterwards the hyponitric acid was introduced into E, as in former experiments, closing the extremities with a lamp, and mixing the liquids. The combination was effected almost immediately, and without the smallest apparent decomposition. The green liquid was now no longer produced. The substance thus obtained fused at 215° cent. (419° Fahr.), and presented all the characters of the substance obtained by the action of dry sulphurous and hyponitric acids.

In the second place, on comparing the crystals of the pure anhydrous substance with those of anhydrous sulphuric acid, they are found to be absolutely similar: the same silky tufts, the same elongated needles. Viewed through a microscope, it would be impossible to distinguish one from the other, if contained in the same tube. This apparent isomorphism seems to establish clearly that the new substance ought to be regarded as containing anhydrous sulphuric acid, in which one equivalent of oxygen is replaced by one equivalent of hyponitric acid.

In this hypothesis it is easy to explain the property which is possessed by sulphuric acid, of dissolving this substance in all proportions, and always producing (within very extensive limits) solid crystallizable compounds; the reason of which would not be apparent on any contrary supposition *. I am not aware whether satisfactory experiments have been performed to demonstrate the existence of a di-hydrated sulphuric acid ($HO + 2SO_3$), which is admitted by some chemists. If there have not, can it not be presumed that anhydrous sulphuric acid likewise is dissolved in several proportions, and forms several crystallizable compounds with oil of vitriol, that is, with sulphuric acid containing one proportion of water?

Nothing is now easier than to explain the discordant results which have hitherto been obtained by distinguished chemists. It is evident that they have analysed different substances; regarded as identical merely for this reason,—that they crystallize. This entirely insufficient character ought to be replaced by one much more satisfactory in this instance,—namely, the temperature of the point at which they fuse.

The preceding researches throw great light on the reactions

* It is proper to remark that the formula of the crystals examined by Dr. Henry may be expressed thus: $SO_2, NO_4 + HO + 4(SO_2O, HO)$; or, adopting six proportions of water, $SO_2, NO_4 + SO_2O + 3(SO_3\ 2HO)$.

which occur in the leaden chamber, and afford a clear elucidation of the theory of the manufacturing process for sulphuric acid.

First, of the new process, in which sulphurous acid, nitric acid and aqueous vapour are conducted into the leaden chambers. In order to ascertain what takes place in this process, a current of sulphurous acid was passed into a flask containing nitric acid; the latter, again, was connected, by means of a bent tube, with a flask containing oil of vitriol, that with a vessel moistened with water, and the last with a dry flask. The nitric acid was entirely decomposed, the first flask containing in a short time nothing more than free sulphuric acid. Red vapours travelled from the first to the second vessel: sulphurous acid likewise entered it, for white crystals were produced even in the last vessel of the series. All the sulphuric acid contained in the second vessel became a crystallized mass, of a faint greenish yellow colour. The reactions are, therefore, in the main, of the same nature as in the ordinary process.

Secondly, of the common process. In a chamber, the bottom of which is covered with sulphuric acid, and into which steam is continually injected, sulphurous acid, nitric oxide and air, or, in other terms, sulphurous acid and hyponitric acid in the nascent state, are introduced. It is generally admitted that these two bodies, which do not combine when dry, can unite under the influence of water, in the state of sulphurous and nitrous acids; and, in the second place, that the crystals formed are decomposed by the smallest quantity of water in excess.

It was, at least, singular, that water could thus produce two absolutely contrary effects; but we can now affirm, that this idea is groundless, since, according to my experiments, water immediately causes the decomposition of the anhydrous compound, which would evidently never have occurred, had water possessed a tendency to preserve the union of its constituents.

Let us examine matters a little closer.

If sulphurous acid and oxygen gases be mixed and put in contact with water, sulphuric acid is gradually produced. But this acid is more expeditiously produced from sulphurous and hyponitric acids; but even in the last case the reaction is by no means rapid. These same bodies, on the contrary, act with great energy in the presence of anhydrous or hydrated sulphuric acid, forming, in the first instance, the compound $S\,O_2\,O$, $S\,O_2$

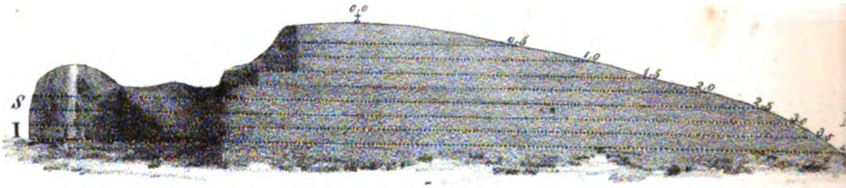
NO_4; and in the second, SO_2O, SO_2NO_4 + hydrated sulphuric acid.

It is therefore evident—1st, that the crystals are never formed without the presence of free sulphuric acid; 2nd, that when free they are invariably destroyed by water. If water appear indispensable to the formation of crystals, in the experiment commonly exhibited at the lecture-table, it acts only in an indirect manner, by giving rise to sulphuric acid. In fact, the crystals may be obtained much more readily, and in larger quantity, if sulphuric acid be previously employed to moisten the flask used in this experiment.

In the new manufacturing process the nitric acid yields one portion of its oxygen to sulphurous acid, which becomes sulphuric acid. Proceeding from this as a starting-point, reduced as the nitric acid is, to the state of hyponitric, it acts as the hyponitric acid does, resulting from the union of nitric oxide with the oxygen of the air in the old process; that is to say, it alternately yields oxygen to the sulphurous acid, and abstracts it from the air: but these changes require the intervention of sulphuric acid and of water. The water may possess two very distinct functions; it can act directly by condensing and bringing the sulphurous and hyponitric acids into closer contact, and thus enable the first to obtain oxygen from the second. But this, which is a slow reaction, is not the most important function of water, as it acts much more efficaciously in another manner in concert with sulphuric acid. The latter determines a rapid formation of white crystals, and also of a thick and heavy vapour of a fawn colour, which contains a considerable quantity; these are *decomposed by the water*; becoming immediately transformed into hydrated sulphuric acid, and nitrous acid or nitric oxide. The reaction of the latter recommences, and may be continued indefinitely. Such is, if I am not deceived, the true account of the changes in the manufacturing process for sulphuric acid.

To sum up briefly the results of this investigation; they are,—1st, the formation of a new substance through the action of sulphurous acid on hyponitric acid; 2nd, an explanation of the discordant results obtained by other chemists in examining the crystals of the leaden chamber; and, 3rdly, a more complete and exact theory of the complicated phænomena which are presented in the manufacture of sulphuric acid.

Fig: 1.



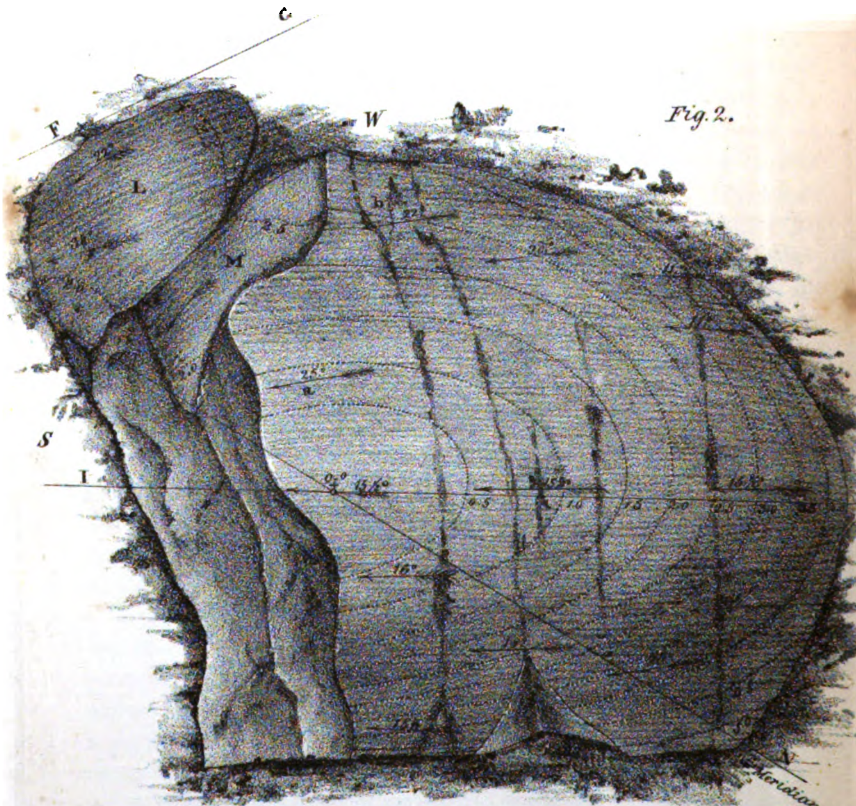


Fig. 2.

J Basire lith.

in conjunction with some common geological occurrence, of which these furrows may be the yet remaining marks. Proceeding with such a view, I determined to investigate how far these furrows extend over all our mountains; and whether their direction is everywhere the same; as well as (should they not be found alike) the nature of the variations which occur, and the circumstances under which they have originated.

From my avocations for a time confining me to the immediate vicinity of Fahlun, I have there more minutely investigated the mountain-furrows, and come to a result which pointed out a plan for the researches which I afterwards had an opportunity of making in other parts of Sweden, as well as for those made by the students of the mining-school, who united zeal to an accurate knowledge of the object in view. It is the result of these investigations that I have now the honour of presenting to the Royal Academy, with the view that investigations on this subject may be made by others in places where they have not hitherto been undertaken; and I shall also draw attention as much as possible to the method which I have pursued, in order to prevent errors in the observations. I shall first describe my investigations in the district of Fahlun, and, generally, give an account of the experiments which circumstances at the time induced me to undertake. It is impossible to make this hypothesis clear to every one, and to give life to the details, without first saying some few words on the theory respecting the cause of this phænomenon, which gives the investigation its leading feature and enables the reader to comprehend the details,—without which clue it would appear of little importance.

The circumstances which I have noticed lead to the conjecture that a mass of large and small stones, sand and gravel, by the action of water, have been rolled and washed forward over the already exposed surface of the earth, by which these stones, rolling against each other in their course, have produced the pebbles which lie collected in heaps on the extensive hills which we call *åsar* (ridges), and of which the heaviest, less subject to roll under the pressure of the weight of the masses of stone lying above them, have slidden over and around the surface of the mountain and furrowed it, in the same manner as a polished surface of marble is furrowed by grains of sand under the pressure of the finger when drawn briskly over it. To name this probable geological occurrence I shall call it the *Boulder-flood* (*Rullstensflod*), from the name *Boulder* (*Rullsten, Gerölle*), by which we distinguish the stones that are rolled, from the *Earth-stones* (*Jordsten*, *Geschiebe*, *Blocs erratiques*), by which we understand the large loose stones deposited immediately under the surface of the earth, seldom or never rolled blocks of stone, which appear to have been conveyed to their present situations by an agency quite different.

Should it be wished to give the boulder-flood a scientific name derived from the Greek, and suitable to every language, it may be called the *Petridelaunian*-flood, from *πετρίδιον*, a little stone; and *ἐλαύνω*, I roll forward.

§ 1.

On the direction of the boulderstone-furrows.

From the investigations I have had an opportunity of making on many places over the Swedish mountains, it has appeared that the northern sides of the hills and mountains are all rounded and destitute of peaks and angles, whilst, on the contrary, on the southern ends sharp peaks and corners still remain, evidently of a very altered broken surface, a consequence of the geological occurrences which rent asunder and uplifted the mountains; and the furrows run directly over the mountains between these oppositely-shaped formations, as if the steep northern part of the mountain had offered the flood a resistance, by which it had been smoothed down; on the contrary, on the southern end, the mass has continued its course without, in its fall, breaking off and furrowing this inclining surface of the rock, from which it was protected by the force of the raging flood.

To give an idea of my first investigations for an explanation of this phænomenon, I have annexed a sketch of a rock adjoining the mines of Fahlun to the westward, both with a Surface-plan, and Section (Plate III.). The lower end 4·0 of the section, fig. 1, lies close to the surface of the earth. These heights are seen by the same figure, observing that the distance between each of the horizontal lines on the section 3·5, 3·0, 2·5, is half a Swedish foot. The end K lies towards the north, and the end I towards the south, from which is shown what is before mentioned, namely, that on the northern side the edges are worn away, whilst those on the southern side remain.

The same appears on fig. 2, which shows the same rock on a ground plan. The point near the top marked ♁ is a station made for laying down the maps of the mine. The oval-formed lines around that point show the places on the surface of the rock which lie 0·5, 1·0, 1·5, 2 feet, &c. under ♁, and correspond with the same lines on the section, which is taken in the line I K, and gives a correct idea of the form of the rock. From this figure it is rendered still more evident that all the angles on the northern, or, properly, the north-western side, are worn away, whilst on the south-eastern side the pointed ends remain. There are also no furrows to be found, except on the upper surface of the rock L and M, which lies even with the rock from 2 to 2·5 feet under ʒ.

The arrows on fig. 2 denote the direction of the furrows, which, besides, is marked with lines, as well as described on the map, as it has been read off immediately from the compass when the line of degrees which denotes 360°, or 0°, and 180° have been laid parallel with the furrows*. By this it may be seen, that already, on such a small rock as this, not thirty feet long, the furrows diverge considerably from each other in their direction.

The furrows, which commence at K and end at I, go over the highest ridge of the rock, and all follow the same direction, as if they had been drawn with a line, and could thus be considered as *normal*. On standing opposite, on the northern side of the rock, or from whence the boulder-flood came, and turning towards the south, it will be found that the furrows on the western side diverge towards the right, whilst those on the eastern side diverge towards the left. That this difference is not accidental is very evident, for when the rising part of the rock made an impenetrable barrier against the rushing stream, this last-mentioned force must have diverged on both sides, and it would be in truth more inconceivable had the furrows not deviated as they now do. All this is very natural, as well as an evident sign that the boulder-stream has passed from the north to the south, and could not possibly have gone in the opposite direction. This deviation, however, on both sides, concerns principally the furrows which are on the northern end of the rock, on each side of the normal furrows; whilst, on the contrary, when lateral furrows come up on the sides of the point ʒ, they approach towards the normal direction, and this of itself is very natural, as the consequence of the boulder-stream having gone from the north to the south.

The deviation which the furrows make at *a* and *b*, as well as

* The degrees on the compass are numbered from north, east, and around to the north. 90° stands thus to the east, 180° south, 270° west, and 360°, or 0° north. This method of numbering is for the better avoiding any uncertainty, otherwise one might easily mistake the east for the west when the needle points to the east, and the furrows lie to the west of the north line.

on the slopes L and M on the southern part of the great rock, is contrary in respect to the normal furrows, so that it appears less easy to be explained. On the slopes L and M it may be perceived that the motion has been of the same nature as that caused by the water in a river when it makes an eddy behind some projecting point; such may also be seen at many different places, as, for example, near Skyttgrufvan (the shooting mine); but at this place, of which we now speak, it does not give a satisfactory result, and cannot hold good with the points *a* and *b*. In order to investigate this further, I caused the western side to be cleared of the rubbish with which it was for the greater part covered. The phænomenon then presented itself still more evidently, as well as showed that this deviation is more to be regarded as the rushing forward of a stream from the slope on the western side of the hill; for when I measured a horizontal line from the plain on this side, in the level of the point *b*, it did not run to the south, but towards the south-east, and about in the direction of the line F G; it is thus probable that the height of the neighbouring land has been the cause of this; it is not also improbable that even another circumstance might have contributed thereto, as all the circumstances show plainly that the boulder-stream had a considerable rapidity, as shall be shown hereafter; and if so, probably behind a rock such as this a kind of suction had originated, of the same nature as that which Venturi and others have observed in hydraulics, and the result of this must have been the same as it now appears on the southern end of this rock.

It may be seen here also that the furrows on the eastern side, for example, which is very steep, run in the same manner as has been before remarked, not directly up and down, but nearly horizontal, according to its given direction.

Such rocks as these are to be found in many places; but near Askersund, one-eighth of a Swedish mile to the southward, to the left of the road leading to Stjernsund, there is one to be seen which is uncommonly beautiful, and from this cause, that on its southern side is a mass of rock situated between two large loose horizontal pieces of the same, projecting a little beyond its original bed; but the power has not been sufficient to form a perfect separation and convey it to a greater distance: it sits on the wall, as the miners express themselves, and has wedged itself fast, so that it could not be separated without the application of a greater force. The same rule which holds good as to the direction of the furrows on a small scale, as on this and similar rocks, I have seen verified when I have examined *the rocks on a large scale,* or *mountains.*

In order to explain this, whilst exercising the students of the mining-school, I measured a considerable tract here around Fahlun, and, with the assistance of the mining and other maps, I constructed a map*, on which, by actual levelling, the lines are placed as on the ground, corresponding to every tenth foot above the surface of the water of lake Runn. These lines, placed here with the same design as those on the sketch of the rock in Pl. III., give, without any transverse section, a sufficient idea of the configuration of the surface of the earth on the place in question; and, in order to see how this slope lies with regard to those nearest surrounding, the heights were inserted by Mr. H. Wegelin, who has studied the tract around here, and determined them according to the ordinary method of surveying.

The first thing that I did was to see if the compass was attracted by any ores in this district. For this purpose I tried the direction of the needle on many places on the measured and marked base-sectional lines, after the following method,—viz. that the sight line of the telescope was taken in the direction of the base line, and the observation made on the needle, to see if it everywhere, on the same line, pointed to the same degree. At the point of intersection of the lines the observations were combined, by which I not unfrequently found that the compass, in the neighbourhood of the copper-mines, was affected by the magnetical pyrites contained in the ore; but as this, however, never made the difference of half a degree on the needle which I used, I have not made any remark on it†.

The hill at this place, which presented the best situation for making investigations with the view in question, is just that on the northern end of which the mines are situated, between Fahlu-å (rivulet) on the eastern side and the lakes of Önsbacksdammen, and Wällen on the western. This hill has three tops, which are very plainly furrowed:—*Grufrisbergstoppen* furthest to the west, *Pilbobergstoppen* in the middle, and *Galbergstoppen* further easterly. On every side of this hill slopes are found with somewhat distinct furrows, with exceptions on the southern side near the top, where, however, some indistinct slopes are met with, otherwise the rock *in situ* is covered on this side with

* [This map is Plate vii. in the *Vetenskaps-Acad. Handlingar.*—Ed.]

† In mining researches, such, however, would demand attention.

a mass of sand, rubbish and boulders which extend themselves to a long hill, which my time would not allow me to level. On most places the furrows are so evident, that their direction, on a length from ten to twenty feet, was often perfectly straight, and could be determined with certainty to within half a degree. It was seldom that it was uncertain to a whole degree, but in such cases the medium of several observations was taken, or if the different furrows appeared very various, the variations were remarked.

The results of the observations are as follows, and are noted here as they have been read off from the compass.

	Direction of the Furrows.
Grufrisbergstoppen	9½° to 10½°
Pilbobergstoppen	10 to 10½
Galbergstoppen	10
N.B. On these three tops the furrows have thus the same direction.	
The northern foot of Grufrisberg and Pilboberg has evident, but variable furrows.	
a. Above Gammelbergs or Tallbacks-dammen	7½, 10 to 11½
b. Below Tallbacksdammen or Krondiket	9
North-western and western sides of the same hills.	
a. At Wällan's hole dam	9½
b. Further to the south	8½
c. Still further south and nearer the top	9
N.B. The furrows diverge here somewhat to the right, yet very inconsiderably, whilst a large hill lies opposite, on the southern side of lake Wällan, as shown by the map.	
North-east side of the same hill:	
a. Near Ingarfsdammen, as well as the new crossteen pits west of Storgrufvan	12½
b. Slopes higher up on the hill or rock, which are described in the foregoing; the ridge	15½
c. Many slopes still higher up	16
d. A smooth slope near Fredriks Konsthjul-hus	43
N.B. The furrows diverge here very considerably to the left, as may be seen by standing to the north and facing the south. All the before-mentioned places are likewise situated near the foot of the hill; but on ascending the deviation becomes less, and at last the furrows take the normal direction.	
On the eastern side near the top	10 to 10½

From the above description we might be justified in drawing this conclusion, *that the furrows, with respect to their deviation on a large mountain, follow the same run as on a small rock*, in the way already mentioned, and I have even seen this corroborated in all other places. It is also necessary that the observations should be made with judgment, and however easy this may be in itself, it still requires a long practice in order to make one competent to undertake it.

§ 2.

Some observations on the determination of the direction of the furrows.

When I commenced my observations on the furrows, it occurred several times, when I again visited the same district and had not borne in mind the places which I had before observed, that different results were obtained; not varying very much, but more so than would have been caused by the indistinctness of the furrows. Upon a closer investigation, the error was found in the greater number of cases to be caused by inattention to the situation of the place observed, as well as whether the furrows were normal or not; in other words, whether the furrows observed went over the top of the hill on which the observations were made, or if they in their continuation went over the eastern or western side. As far as the observations were continued around Fahlun, it was necessary to observe whether they were made on the top of the hill or on the northern end; but as in other places it has been found that the furrows do not always go so nearly from north to south, it was therefore necessary to try another method in order to perfect the observations. For this reason I gave up fixing my attention to the compass direction; but in its stead at first, as near as possible, from a view of the surrounding country, by examining the maps, and from information collected from persons well acquainted with the locality, I procured a good idea of the form of the hills in question; after which I had no other resource than to observe the furrows on every place possible, as well as to note them down; then with the compass placed on the map and laid parallel with the north and south line, I easily found which furrows at the foot of the mountain passed over the top. The side of the mountain where the furrows are found I call *the opposing side* (stötsidan), on the hypothesis that the flood of boulders had rushed against it; and the opposite side of the mountain I call the *lee-side* (läsidan). After having gained experience, and particularly when, making my observations, I did not find myself surrounded by high wood, it soon appeared which side of a lesser slope (of not more than one Swedish mile in extent) was the opposing side. Assuming that we are always viewing the hill from this side, the opposite side will always be the lee-side, and the other two the *right* and the *left*; the former, in regard to its being pretty nearly the compass direction, the western, and the latter the eastern side.

Taking notice of this, which could not be done at all times on a journey through tracts before unexplored, I could almost be certain to find the normal furrows on a hill, should there be any there at all to be seen, and in this manner was almost sure to discover the principal direction of the furrows.

For the sake of perspicuity some difference should be made between the *normal furrows* of the slopes and the *side furrows*; and with the latter between *the side furrows to the right*, and *the side furrows to the left*.

I likewise found something of importance to observe, namely, that *the same furrows may be normal furrows as belonging to a lesser mountain height, but likewise side furrows with respect to another greater height* lying in the neighbourhood. Such are the furrows which follow the ridge of the rock sketched on Plate III., and which, according to the compass, had $15\frac{1}{2}°$ normal furrows on the rock; but side furrows to the left as belonging to the Pilboberg. Therefore it should be remembered, that

The normal furrows on a rock may be side furrows on a mountain;

The normal furrows of a mountain may be the side furrows of a higher mountain;—as well as that

The normal furrows of a large hill may be the side furrows of less elevated land.

Even on a plain where the furrows generally run parallel, side furrows from a mountain in the neighbourhood are to be met with; which, for example, is the case on approaching to Hunneberg or Kinnekulle, from the plain of West Gothland, and regarding which it is necessary to be cautious that they are not considered of greater importance than belongs to them, for determining the direction of the furrows in a more general view.

Another circumstance to which attention should be paid, even with the furrows well defined, is the texture of the mountain, as this is often not without influence on their direction. If the direction of the mountain is nearly the same as that of the furrows, as well as if the strata are composed of alternate hard and soft substances, it seems to me that the furrows follow the direction of the strata. What I am certain of, and have often seen, is, that it thus happens on a small scale, but I am uncertain if it be the case on a larger. The texture of the mountain has also its influence on the side furrows; for if it be coarse-granular, so that the surface will not receive a polish and be smooth, a greater friction has been caused against the boulder-stream, and the deviation would be greater.

A third circumstance as regards the determination of the furrows is, that when they are found on a hard mineral which receives a fine polish, it would be necessary to stand on such a side as to have them quite distinct, so that through the change of light and shade they strike the eye. Observations are often unsuccessful after dusk; for before one has finished reading off the degrees on the compass, the small furrows have already become invisible*.

Nearly everywhere in the southern part of Sweden the mountains are composed of such compact substances that they readily receive the furrows. Such are found here around Fahlun, as well as northerly towards Siljan; at Stjernsund; nearly everywhere in Roslagen; southerly of Sala; around Arboga; between Linköping and Åtved; around Ronneby; Wimmerby and Eksjö; on Tiveden, from Mariestad to Kinnekulle; on Billingen, as well as from Lidköping to Wenersborg. It is more certain to meet with good furrows on the rocks which have been sheltered from the action of the atmosphere, likewise in pits from whence material is fetched for repairing the roads, on arable land where the rock has been exposed by the plough, as well as on the sides of lakes; and above all on the sea-shore, where the rocks, from the sinking of the water, or the elevation of the land, have been recently exposed.

It may happen, however, and sometimes on large tracts, for example, in the north-western part of East Gothland, as well as southerly on the road passing through Westervik, that the mountains are composed of such substances as will not receive the furrows, or that these in the lapse of time have, through

* It should further be observed in determining the direction of the furrows, whether fine and coarse furrows are mixed with each other, also if they run parallel, or if they diverge from each other. In the latter case, the direction should be determined in the same manner as the finer furrows, because these have, without diverging, followed the direction of the flood; but, on the contrary, the coarser furrows are traces from the large boulders, which, with their angles and more than ordinary force, have rushed against and scrubbed the mountain-slope; and as it seldom could occur that this angle ran directly before the centre of gravity of this large stone, it may be frequently seen, and in particular on coarse-grained substances, that the large furrows go in crooked lines, often diverging to the right and often to the left over the fine furrows. On harder substances, such as porphyry and trap, they are less crooked. Furrows of this kind may be met with on the hard volcanic substances covering the top of Billingen, which are of a surprising regularity.

decomposition, been obliterated. An example of the latter case we have near Kararfvet, where the gneiss is decomposed, and the kernels of quartz, which are beautifully furrowed, remain from three to six lines above the surface of the gneiss.

Where no evident furrows are apparent, I have still, with the assistance of long practice, been enabled, in most places, as well as on such rocks as rise above the surface of the earth, to decide on the direction of the furrows to a certainty within from three to ten degrees.

For such purpose I have accurately observed,—

1st. The opposing side, to find out which part directly resisted the boulder-stream, which is generally apparent from the form of the rock, as well as by the large hollows which have been formed in the places wherein the pieces have been loosened.

2nd. The lee-side; there the place is easily distinguished where the points still remain, and show that they formerly had been, as one says, to leeward; as well as the direction in which pieces have been separated; and

3rd. The hollowness of the surface after the pieces have been broken off, by which it has often been possible to perceive plainly the direction in which these pieces have been broken loose.

Experience and care could in this way produce very good observations, as well as give the direction of the normal furrows, by which can mostly be seen, if possibly they are side furrows, belonging to a height in the neighbourhood.

On some occasions it has appeared to me, that, under such circumstances, the most certain indication is given by the opposing side, which, for example, was the case with my observations around Norrtelje; on the other hand, in the tract of Medevi, the lee-side has shown more plainly where the boulder-flood has passed. In the latter place, however, one is not certain to within eight degrees, partly from this circumstance, that the rocks to a great degree are coarse-grained and brittle; partly also that the whole tract is full of smaller steep rocky hills, which make the direction of the furrows in general nearly undeterminate. The same is the case from Söderköping to near Kalmar, and it appears to be uncertain if it be not the texture of the rocks which is the principal cause of the unevenness of the high land, as well as the consequent irregularity in the direction of the furrows. Directly to the south of the church of Döderhult, a well-furrowed slope, however, appears on the left of the road.

§ 3.

Observations on the direction of the boulder furrows with respect to the compass-direction in the southern half of Sweden.*

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
FIRST PART.				
In the district around Fahlun.				
A. *The hills south of the mines, or the hill at the foot of which the mines are situated*	+ M on			
The top of the Grufrisberg	9¼° to 10½°	16° 30′	26° to 27°	E.
The top of the Pilboberg.........	10 to 10½	16 30	26½ to 27	E.
The top of the Galgberg	10	16 30	26½	E.
On the northern end of the hills of Grufrisberg and Pilboberg.				
a. At Gammelbergs-dam or Tallbacks-dam, many slopes ...	7½, 10 to 11½	16 30	24, 26½ to 28	E.
b. Below the Krondik	9	16 30	25½	E.
On the western side of the hills of Grufrisberg and Pilboberg.				
a. At Wällan's hole-dam	9½	16 30	26	E.
b. Further to the south on the strand of Lake Wällan, below Göthgarden	8½	16 30	25	E.
c. Still further to the south, on the road to the Grufris......	9	16 30	25½	E.
On the eastern side of the hills of Grufrisberg and Pilboberg.				
a. Near Ingarfsdammen by Grufbacksgardarne	12½	16 30	29	E.
b. A slope higher up on the hill, namely, that which the surveying point is on, and which is given in profile	15½	16 30	32	E.
c. Slopes higher up, south of the path to Konung Fredriks Konst..........................	16	16 30	32½	E.
d. A steep slope below the before-mentioned Konst-hjulhus...	43	16 30	59½	E.
e. Many slopes near the top, but on the eastern side of the same	10 to 10½	16 30	26½ to 27	E.
In the valley between the hills of Grufrisberg and Pilboberg, or on the eastern side of Grufrisberg.				
a. By the end of the Schradik...	12	16 30	28½	E.

* Everywhere in the south of Sweden the boulder-stones have gone from north to south, however seldom after the meridian, but more generally to the south-east or south-west; this is shown in the last column of the Tables, where E. signifies that they have taken their course south-easterly, and W. that they have gone south-westerly. The preceding column shows how many degrees they have diverged to the one or the other side.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
b. By a large stone near the top of Pilboberg	° 11½	° ′ 16 30	° 28	E.
c. Still nearer thereto	11	16 30	27½	E.
On the hill between the Pilboberg and Galgberg.				
a. By the mark near the Kommissions Grafven	11½	16 30	28	E.
b. Above the mark on the road to Källvik	11	16 30	28	E.
On the eastern side of the Galgberg.				
By the southern end of Daglöstägts-dam	12 to 13	16 30	28½ to 29½	E.
B. *The hills of Kararf and Warggard, north-west and north from the mine.*				
The north side of the top of Igeltjern	about 8 to 12*	16 30	24½ to 28½	E.
The top of Kunterberg, not very plain	10	16 30	26½	E.
On the *eastern* side of the hill.				
a. Near Kärarfvet.................	13	16 30	29½	E.
b. Further down towards Gamleberg's smelting-house	12½	16 30	29	E.
c. On the hills near Ostanfors-An	11	16 30	27½	E.
On the *western* side.				
On a rock by the side of Onsbacksdam	10½	16 30	27	E.
C. *Hills to the east of the mine, as well as on the eastern side of Fahlu-A.*				
a. On the hill of the Jungfruberg, where the road passes over by Hälla to Skuggarfvet ...	9	16 30	25½	E.
b. Further to the south towards the Jernlindsberg, where the road goes by the Sjulsarfv to Sundborn	10 to 14	16 30	26½ to 30½	E.
c. On Rottnebyskogen, near the road leading to Främsbacka and Korsnäs	9	16 30	25½	E.
SECOND PART.				
North-west as well as north of Fahlun.				
(The northern part of Dalecarlia.)				
N.B.—The greater part of these observations have been made by Mr. Wegelin, and some by the Russian Government's Mining officers, Rachette and Hiriakoff.				
A. *On the road west of Fahlun, on the course of Vester-Dalelfven.*				
Near the inn at Smedsbo.........	7	17 45	24¾	E.

* This observation was made with a compass not graduated.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
On the point of land formed between Oster- and Vester-Dalelfven, on the road from *Gagnef* to *Floda*, near *Mockfjärd*, many slopes with indistinct furrows	8°	17° ′	17°	E.
On the same road nearer Floda, very plain furrows	350	17 15	7¼	E.
B. *On the road from Fahlun, to the east of Siljans-sjön, as well as upwards towards the Norwegian boundary.*				
North of the church of Bjursas, near the old inn at Sörskogs, very plain furrows..............	8 to 18	16 45	24¾ to 34¾	E.
On one place 357°.				
On the Stussberg, north of Lake Axen	13	16 45	29¾	E.
Further to the westward, near Siljan, at the inn at Sjugare...	11	17	28	E.
Northward on the road from Ore church to Skattungby chapel, on the southern side of Lake Skattung, by Digermorsbacke (hill)	10	17	27	E.
In Elfdahls parish on the road to Särna, namely, by Lake Fiscal, to the eastward of Bunkrisbodarne; *finely polished porphyritic slope*	359	18	17	E.
One mile (Swedish) from Nornäs	0	18 15	18¼	E.
Three-eighths of a mile (Swedish) from Särna church, on the highest point of the Slätberg, *finely furrowed sandstone slopes*, which gently incline in the same direction as the furrows	10	18 30	28½	E.
THIRD PART.				
To the eastward of Fahlun, on the road below Gefle, on both sides of Dalelfven, as well as further on the sea-shore towards Oregrund.				
A. *On the road to Gefle, passing by Husby.*				
Between Uppbo in the parish of Skedvie and Husby	7	16 15	23¼	E.
To the eastward of Stjernsund, by the end of Lake Grycken, near the town of Stigsbo......	5	16 15	21¼	E.
Near the inn at Thorsaker	0	16	16	E.
Half a mile (Swedish) south-east of Gefle	313 to 303	15 30	31½ to 41½	W.
B. *On the course of Dalelfven from Husby church towards Elfkarleby.*				
By Kloster's mine.................	7 to 8	16 15	23¼ to 24¼	E.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
At Garpenberg (the medium) ...	° 5	° ′ 16 15	° 21¼	E.
By Avesta Lillfors (rapids)	358	16 15	14¼	E.
Helvetes-fall (water-fall).........	0	16 15	16¼	E.
On the southern side of the river opposite Hedsunda church ...	305 to 315	15 45	39¼ to 29¼	W.
Further from the river southward, on the road to Aby-inn	0 to 5	15 30	15½ to 20½	E.
Further easterly, on the road to Tierp	340 to 350	15 30	4½ W. to 5½	E.
At Söderfors works by Dalelfven, partly by the Tegelbruk (the brick-works), and partly quarter of a mile (Swedish) easterly	320 to 330	15 30	24½ to 14½	W.
By Elfkarleby, obscure furrows	325 to 335	15 30	19½ to 9½	W.
C. *Along the sea-shore from Elfkarleby eastward to Oregrund.*				
At Carlholms mine, or north of Wessland	322	15 15	22¾	W.
Near Löfsta mine	335	15 15	9¾	W.
At Löfsta mine	330	15 15	14¾	W.
Where the road turns off to Dannemora	336	15	9	W.
Three-eighths of a mile (Swedish) from Löfsta	339	15	6	W.
Five-eighths of a mile (Swedish) from Löfsta	343	15	2	W.
Quarter of a mile east of Frebbenbo	346	15	1	E.
At Berkinge blast-furnace	348	15	3	E.
Halfway from thence to Forsmark	352	16	8	E.
South of Forsmark by Johannisfors and by Simonde	343	14 45	2¼	W.
At Ledsund-bridge	349	14 45	3¾	E.
At Sneslinge, opposite the end of Kallerö bay	356	14 45	10¾	E.
At North Skedika..................	358	14 45	12¾	E.
At Sundsby	15	14 45	29¾	E.
Near Oregrund, opposite a smaller bay	3	14 45	17¾	E.
Around Oregrund (on the projecting land)......................	13	14 45	27¾	E.
FOURTH PART.				
South-east of Fahlun, on the roads below Sala and Upsala, and from thence in many other directions.				
A. *On the road from Sala to Upsala.*				
To Avesta (see 1st part, C., also the 3rd part, B.)				
At Sala Dams	344	16	0	
Three-eighths of a mile (Swedish) north of Sala.....................	349	16	5	E.
Near Sala	348	16	4	E.
Three quarters of a mile (Swedish) east of Sala	340	15 45	4¼	W.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
At Brunsäters inn.................	340° to 333°	15° 45′	4¼° to 11¼°	W.
At Bredsjö and at Kölfva.........	350 349	15 30	5½ „ 4½	E.
At Aland church-town............	349	15 30	4½	E.
At Wänge church.................	352	15 30	7½	E.
At Qvarnbolund, quarter of a mile (Swed.) from Upsala	353	15 15	8¼	E.
At Rickomberga, one-sixteenth of a mile (Swed.) to the westward of Upsala..................	356	15 15	11¼	E.
B. *On the road north-easterly from Upsala to Löfsta, by way of Alunda.*				
By the inn at Edinge (east of Rasbo)	350	15 0	5	E.
Northward from thence near Tuna church	349	15 0	4	E.
North of Alunda church by the inn at Haberga	342	15 0	3	W.
Half-way from thence towards Söderby, or on the road to Dannemora, by the town of Löddby	345	15 0	0	W.
Around Dannemora (Iron Works) the compass was unsettled and the furrows undetermined.				
North of the inn at Bro, where the road turns towards Forsmark, the furrows were pretty clear, but the compass was attracted by the iron ore.........	356			
One mile (Swed.) north of the bridge at Gräsbo	342	15 0	3	W.
A quarter of a mile (Swed.) further towards the north.........	342	15 0	3	W.
By the inn at Hakanbo	343	15 0	2	W.
¼ of a mile (Swed.) from thence	341	15 0	4	W.
Near Löfsta, where the road comes from Forsmark	336	15 0	9	W.
N.B. This last observation is the same as is mentioned in the *Third Part*, C.				
C. *On the two roads which go to the eastward from Upsala to Nortelje, the one by Knutby and Skebo, the other by Mora stones and Finsta.*				
a. The north road.				
By the town of Kumla, east of Upsala	351	15 15	6¼	E.
By Lenna blast-furnace	335 to 337	15 15	9¾ to 7¾	W.
By the inn at Gränby, east of Almunge church	333 „ 337	15 0	12 „ 8	W.
By the inn at Gränsta	357	14 45	11¾	E.
North-west from thence, as well as north-east from Gränby, by the town of Solberga	346	14 45	¾	E.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
A little to the eastward of Knutby church	2°	14° 45′	16¾°	E.
By Burvik	2	14 45	16¼	E.
¼ of a mile (Swed.) nearer the inn at Bro, by Ytterby town, on the slope on which the quarter-milestone stands; furrowed in two ways, viz.	18	14 45	32¾	E.
And	349	14 45	3¾	E.
By the inn at Bro	7	14 30	21½	E.
Around Ununge church	352 to 359	14 30	6½ to 13½	E.
By Skebo	352 „ 355	14 30	6½ „ 9½	E.
At Harbroholm (Gärdet to the west of the house)	351 „ 353	14 30	5½ „ 7½	E.
b. The south road.				
By the town of Berga, south of Danmarks church	346	15 15	1¼	E.
By the inn at Hjelmsta (south of Länna; *see a, the north road*)	337	15 0	8	W.
¼ of a mile further to the eastward	335	15 0	10	W.
Nearer the inn at Lindberga (south of Gränby)	329	15 0	16	W.
At Lindberga	327	14 45	18¼	W.
¾ of a mile (Swed.) to the eastward of Lindberga	334	14 45	11¼	W.
By Häradsgränsen	339	14 45	6¼	W.
Near the inn at Rimbo	353	14 45	7¾	E.
By Rimbo (south of Burvik)	5	14 45	19¾	E.
Half a mile (Swed.) further to the eastward (south of Bro)	7	14 45	21¾	E.
Near Husby church	357	14 30	11½	E.
Further to the east, ⅞ of a mile (Swed.) from Norrtelje	359	14 30	13½	E.
At Norrtelje	5	14 30	19½	E.
D. *The roads northward from Norrtelje to Oregrund.*				
a. Northward towards Old Grisslehamn:				
¾ of a mile (Swed.) from Norrtelje by the town of Haberga	3	14 30	17½	E.
By the inn at Kragsta	347	14 30	1½	E.
¼ a mile (Swed.) from thence to the north	348	14 30	2½	E.
By Edstuna church	351	14 30	5½	E.
By the inn at Svanberga	353	14 30	7½	E.
By the house at Broby	349 to 351	14 30	3½ to 5½	E.
By the town of Toftinge	346	14 30	½	E.
By the town of Hussinge	345	14 30	½	W.
By the town of Gasvik (regular furrows)	346	14 15	¼	E.
By Rangarnö (on Väddön)	349	14 15	3¼	E.
On the western foot of Vardkasberg	346	14 15	¼	E.
On the top of Vardkasberg	347	14 15	1¼	E.
By Senneby	348	14 15	2¼	E.
By Old Grisslehamn	355	14 15	9¼	E.
The eastern point of land projecting from the same place	356	14 15	10¼	E.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
b. From Skebo, northward to Herrängen:				
By Edbo church	359	14 30	13½	E.
By Häfverö church	3 to 5	14 30	17½ to 19½	E.
By Kusbyn	9	14 30	21½	E.
By Korgnäs old blast-furnace ...	10 to 16	14 30	24½ to 30½	E.
By Herrängen	10 „ 16	14 30	24½ „ 30½	E.
c. From Skebo, northward to Oregrund:				
By the inn at Hammarby.........	2	14 30	16½	E.
By the house at Lunda	1	14 30	15½	E.
By the town of Sätra	1	14 30	15½	E.
By Midskog	6	14 30	20½	E.
By Graska..........................	8	14 45	22¾	E.
By Sanda	8	14 45	22¾	E.
½ a mile (Swed.) south of Harg.	7	14 45	21¾	E.
In Hargs Skärgard (beautiful furrows)..........................	10	14 45	24¾	E.
By the inn at Marka	2	14 45	16¾	E.
Near Barstels church	5	14 45	19¾	E.
By Assjö	7	14 45	21¼	E.
Near Osthammar	5	14 45	19¾	E.
By the house at Moen...........	7	14 45	21¾	E.
At Norrskedika and Oregrund (see the Third Part, C.).				
E. *On the road to the southward from Norrtelje to Stockholm.*				
By the inn at Halls	352	14 45	6¾	E.
⅜ of a mile (Swed.) south of the inn at Brottby	348	14 45	2¾	E.
By Angarns church and the house at Rörby..................	350	14 45	4¾	E.
By the inn at Eusta	348	15 0	3	E.
¼ of a mile (Swed.) from thence	345	15 0	0	E.
By Mörby, near Stocksund	347	15 0	2	E.
F. *On the roads southerly from Upsala to Stockholm.*				
a. By the inn at Marsta ...	345 to 347	15 0	0 to 2	E.
b. Between Ashusby and Rotebro by Seby	344	15 0	1	W.
G. *On the roads southerly from Stockholm to Norrköping.*				
By the inn at Aby	351	15 15	6¼	E.
By Nykoping........................	9	15 30	24½	E.
By Jäder's inn, in a valley stretching to the eastward	21	15 30	36½	E.
By the inn at Wreta...............	7	15 45	22¾	E.
By Krokek	358	16 0	14	E.
FIFTH PART.				
Southerly from Fahlun towards Mälaren and Hjelmaren.				
A. *The road below Stockholm* (to Sala, see the Fourth Part, A.).				
Near the inn at Tärna	348	15 45	3¾	E.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
By the tenth milestone from Stockholm	356°	15° 45′	° 11¼	E.
Half-way to Carleby	348	15 45	3¼	E.
By the inn at Carleby, or Simtuna church	356	15 45	11¼	E.
Between Wansjö and Langtora inn (south of Brunsäter), see the Fourth Part, A	337	15 45	7¼	W.
By the inn at Tunalund*, as well as on the road to Gran, on the land full of high mountain-hummocks, the furrows vary considerably, namely from	347 to 4	15 30	2½ to 19½	E.
By Gran, about	357	15 15	12¼	E.
By the house at Torresta	349	15 15	4¼	E.
On the, so-called, Brogärden	348	15 15	3¼	E.
East of Bro church	342	15 15	2¾	W.
At the inn at Tibble	340	15 15	4¾	W.
½ a mile (Swed.) nearer the inn at Barkarby	342	15 0	3	W.
At Barkarby	335 to 333	15 0	10 to 12	W.
By Sponga church	342	15 0	3	W.
By the house at Rogsta	343	15 0	2	W.
On the many mountain-hillocks around *Stockholm*, the furrows are very various.				
On Kungsholmen:				
By the church	352	15 0	7	E.
By the site of Kronqvarnen (Crown-mill)	2	15 0	17	E.
On Skeppsholmen	349 to 352	15 0	4 to 7	E.
On Ladugardsgärdet	347	15 0	2	E.
B. *The road from Fahlun below Köping by Mälaren, likewise from thence to Orebro by Hjelmaren.*				
Between Naglarby and Rusgarden's inn, on the northern end of Silfberg's heights	4	16 30	20½	E.
To the eastward of this road by the town of Lervik in Säther†	7	16 30	23½	E.
On the southern end of Silfberg	352	16 30	8½	E.
At Rusgarden	350	16 30	6½	E.
Between Rusgarden and Bommarsbo	347 to 349	16 45	3¾ to 5¾	E.
On Mörtkärnberget	347	16 45	3¾	E.
Near the inn at Smedjebacken, or Norrbärkes church	349	16 45	5¾	E.
At Söderbärke church	357 to 3	16 30	13½ to 19½	E.
On the road to Norberg	352	16 30	8½	E.
By the inn at Bysala	351	16 30	7½	E.
By the inn at Gisslarbo	351	16 30	7½	E.
Between Köping and Arboga, likewise from thence, passing				

* In the south-west from hence, around *Westeras*, at Edsberga, the furrows vary to 330°, at Munktorp 340°; the observations were made by M. Wegelin.

† This observation was made by MM. Baer and Franzén.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
the inn at Fellingsbro, to the bottom of Käglan wood, the furrows are more regular than I have hitherto seen them, and deviating but little from	° 355½	° ′ 16 30	° 12	E.
On the northern end of Käglan.	355½	16 45	12¼	E.
On the hill at Käglan wood by the road at Ringaby	353	16 45	9¾	E.
On the western end	343 to 349	16 45	¼ to 5¾	W.
By the inn at Glanshammar ...	345 „ 346	16 45	1¾ „ 2¾	E.
Nearer Orebro	336 „ 346	16 45	7¼ W. „ 2¾	E.
C. *The road from Fahlun by Linde to Orebro.*				
(The observations made by MM. Hiriakoff and Rachette, and also by the Mining-Sheriff Fitinghoff.)				
From Fahlun to Smedjebacken; see the Fifth Part, B.				
South of the inn at Ostanby ...	5	17 0	22	E.
By the inn at Hällsjö	3	17 0	20	E.
By the inn at Högforss, near the highest hill	351	17 0	8	E.
On the western end	348	17 0	5	E.
At Finngrufvorne (the Finn-mines) belonging to Nya Kopparberget (the new copper mountain): namely, on the western side of the hill	350	17 0	7	E.
On the eastern side of the hill...	10	17 0	27	E.
A little to the south, on a smaller mountain-top, the western end	355	17 0	12	E.
The eastern end	5	17 0	22	E.
By the inn at Bredsjö	357	17 0	14	E.
Around Linde	353 to 357	16 45	9¾ to 13¾	E.
By the inn at Bondbyn	352	16 45	8¾	E.
By the inn at Dylta	333	16 45	10¼	W.
By Hofsta church..................	333	16 45	10¼	W.
D. *On the road from Orebro south, by Hjelmaren and Mälaren.*				
(The observations made by MM. Franzén and Baer, students in the mining-school.)				
By the inn at Resta	7	16 45	23¾	E.
By the inn at Orsta	8	16 45	24¾	E.
West of Torshälla..................	10	16 0	26	E.
To the eastward of Eksag near Strengnäs	8	15 30	23½	E.
½ of a mile (Swed.) south of Strengnäs, by the town of Hammarn	10	15 30	25½	E.
By the town of Solberga, ½ of a mile (Swed.) from Aker	7	15 30	22½	E.
On the northernmost point of land at Utön, on the best place (observed by C. Beronius)	343	14 45	2	W.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
	° °	° ′	° °	
Sixth Part.				
South-west of Fahlun, on the road to Philipstad.				
(The observations made by M. Fitinghoff*.)				
To the New Copper-mountain (see the Fifth Part, C.).........				
By the works at Stjernfors	358	17 0	15	E.
By Jöhnshytten, in the parish of Hjulsjö	350	17 15	7¼	E.
By the inn at Nytorps	344 to 353	17 15	1¼ to 10¼	E.
Around Philipstad many observations have been made, varying from 343° to 350°, the medium	346	17 30	3½	E.
Seventh Part.				
South-east of Orebro.				
On the roads to the eastward of Wettern and Lagaan to Carlskrona.				
A. *The roads southerly to Linköping.*				
a. By Källmo.				
½ a mile (Swed.) south of Orebro, indistinct furrows	332	16 45	11¼	W.
North of the inn at Källmo	347	16 45	3¾	E.
A slope on the north-western end of lake Boren, to the east of Carlshult	336	17 0	7	W.
N.B. The slopes on the whole of this road are composed of loose substances, mostly coarse-granular granite; otherwise sandbanks.				
b. By Askersund and Wadstena.				
On the road to Wredstorp, sandbanks with gravel composed of alum-slate	342	17 0	1	W.
By Askersund	332	17 0	11	W.
By the town of Forsa, ½ a mile (Swed.) north of the inn at Ra	330	17 0	13	W.
¼ of a mile (Swed.) east of Ra, on an extensive hill by the town of Hulta	325	17 0	18	W.
Around Medevi-well, the furrows are indistinct on coarse granular granite, about	341	17 0	2	W.
On the north-western point of Omberg	316 to 323	17 15	26¼ to 19¼	W.
By Linköping, directly to the eastward of the town, indistinct furrows	3	16 15	19¼	E.

* Many other observations have been made in the parish of Grangärdes; but the compass appears to have been attracted by the iron ores.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
B. *The road on the coast of Ostersjö down to Carlskrona.*	°	° ′	°	
By the inn at Kumla...............	5	16 15	21¼	E.
By the house at Minsjö, on the south shore of lake Asplangen.	2	16 15	18¼	E.
By the inn at Hälla	2	16 15	18¼	E.
Around Söderköping, often fast mountain slopes, but steep, and never with distinct furrows ...				
By Ed's works, indistinct furrows	28	16	44	E.
By Vestervik........................	24 to 30	15 45	39¾ to 45¾	E.
At the inn at Getterums (Stags-head)............................	27	16 0	43	E.
By Ishult's inn	26	16 0	42	E.
By Jämserum's inn	18	16 0	34	E.
By Doderhult church, a little to the southward, on a plainly-furrowed slope under the road...	23	16 0	39	E.
By Paskalavik	23	16 0	39	E.
By Mönsteras, the rocks in the bay are composed of *a coarse granular sandstone*, with indistinct furrows	23 to 28	16 0	39 to 44	E.
N.B. On the whole of the above road only sand and boulders are to be met with, composed often of sandstone; no fast slopes are to be met with before one reaches the south of Brömsebro, or when one comes nearly one mile (Swed.) within the boundary of Bleking.				
In the neighbourhood of the town of Löckeryd, large flat slabs with indistinct furrows, about	327	16 30	16½	W.
N.B. Since these slabs have been furrowed they must evidently have been scrubbed.				
At Ramdala church, plain furrows	331	16 30	12½	W.
At Lösen's church.................	333	16 30	10½	W.
N.B. In the same neighbourhood, a short distance from the road on the northern side, large rocks are found, which by the furrowing have been crossed and removed to the southward.				
At and near Carlskrona the furrows were very various, the medium about	340	16 30	3½	W.
In the dock, 21 feet under the surface of the sea	11	16 30	27½	E.
C. *The road from Carlskrona by Rönneby and Wexiö to Taberg by the southern end of Wettern.*				
At and passing Rönneby several				

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
observations were made; but in this district, where the boulders had gone over the hills, the furrows, as at Carlskrona, were very various. The greater part around were......	° ° 12	° ′ 16 45	° ° 28¾	E.
To the north of and above the inn at Barkaryds	12	17 0	29	E.
At the inn at Djursmala	7 to 8	17 0	24 to 25	E.
North of Isasa	5	17 0	22	E.
North of Qvarnmala...............	2	17 0	19	E.
By the inn at Urasa	358	17 0	15	E.
At *Wexiö*	2 to 3	17 0	19 to 20	E.
By Ohr's inn........................	354	17 15	11¼	E.
At the inn at Bo	342	17 15	¾	W.
Further north	346	17 15	3¼	E.
At Starhult, a hilly country, and thus not good for observations	8 to 20	17 15	25¼ to 37¼	E.
Kohult's inn	357 to 359	17 30	14½ to 16½	E.
Svennarum	360 to 4	17 30	17½ to 21½	E.
North of Stigamo by Krakerum.	4	17 30	21½	E.
On the Taberg, even to the top, evident marks of the revolution which caused the furrows are apparent, but no furrows remain. From the figure of the mountain it might however be inferred that they had made about 11° towards the sight-line on Omberg*, and as this line makes an angle of 22½° to the east of the meridian, the furrows should go	....	....	11½	W.
At the inn at Barnarps............	4	17 30	21½	E.
N.B. On the side of Wexiö the land is very hilly, and the furrows therefore very various.				
D. *The road from the Taberg eastward across the country to near Westervik, and from thence to the north-west back to Linköping.* N.B. This tract, one of the most hilly in Sweden, exhibited, what was to be expected, very various furrows.				
At Ingaryd's inn	355 to 357	17 30	12½ to 14½	E.
Near Esperyd's inn	337 to 342	17 15	5¾ to ¾	W.
At Marientorp's inn...............	359	17 0	16	E.
At *Eksjö*.............................	337 to 342	17 0	6 to 1	W.
A little to the eastward of Staden	349	17 0	6	E.
On the, so called, Källarpslien (high hill), near Ekeberg's inn	359½	17 0	16½	E.
Around the inn at Rödkulla, and the hill on which Pelarne church lies, the furrows are				

* As the mountain is composed of iron ore the compass was of no service.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
particularly plain, but vary very much, from 0° to 43°; on the place best situated	° ° 14	° ′ 16 30	° ° 30½	E.
At Odestorp, to the east of Wimmerby	14	16 15	30¼	E.
At Grönhult inn	15 to 18	16 15	31¼ to 34¼	E.
At Ankarsrum's works............	15 to 23	16 0	31 to 39	E.
At Hallingeberg's inn	10 to 14	16 0	26 to 30	E.
At Eneby inn	14 to 18	16 0	30 to 34	E.
By Dalhelm's inn..................	21	16 15	37¼	E.
At Mashult's inn	7 to 10	16 15	23¼ to 26¼	E.
At Orsäter's inn, half a mile (Swed.) to the north of Atved	21 to 27	16 15	37¼ to 43¼	E.
At the inn at Fillinge, on several places............................	21	16 30	37½	E.
At Linköping, see this Part, A.				
Eighth Part. *South-west of Orebro.* *On the roads between Wettern and Wenern to Uddevalla.*				
A. *The road over Kinnekulle, to Lidköping.*				
To Wredstorp, see the Seventh Part, A, *b*.				
West of Wredstorp	332	17 15	10¾	W.
By the inn at Bodarne............	342	17 15	¾	W.
On Tiveden by Staffasmossen ...	318	17 30	24½	W.
At Hofva	344	17 30	1½	E.
¾ of a mile (Swed.) north of the inn at Hasselrörs the furrows are on *coarse granular sandstone*	342	17 45	¼	W.
Near Mariæstad	332	17 45	10¼	W.
A well-furrowed slope below Slottshäktet on the shores of Wenern; uncertain if the compass was attracted	283	17 45	59¼	W.
Near the house at Sjöberga, to the west of Björsäter	326	17 45	16¼	W.
South of Arnäs, near Forshem...	318 to 320	18 0	24 to 22	W.
By the inn at Forshem............	319	18 0	23	W.
On the top of Kinnekulle.........	316 to 321	18 0	26 to 21	W.
N.B. From thence on the western side of Kinnekulle the limestone-slopes are so much acted on by the atmosphere as to make the furrows invisible. From this place sand hills extend to Lidköping.				
B. *The road from Lidköping between Halle- and Hunneberg, passing by Wenersborg to Uddevalla, and afterwards to Trollhättan.*				
a. On the setting out:				
By the inn at Malby	310	18 15	31¾	W.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direc-tion.
Further west	309°	18° 15′	32¾°	W.
By Täng's inn	304	18 30	37½	W.
Further to the south-west	301	18 15	40¾	W.
By Grästorp's inn	298	18 30	43½	W.
½ of a mile (Swed.) from thence	296	18 30	45½	W.
Near where the road turns to the north-west, to come between Hunne- and Halleberg	309	18 45	32¼	W.
Further on by Flo church	316	18 45	35¼	W.
On Hunneberg (*on trap*)	298	18 45	43¼	W.
N.B. There are no furrows to be found between the mountains before about a quarter of a mile (Swed.) from Halleberg, where the granite slopes appear.				
On the granite slopes in the lee of Halleberg	297	18 45	44¼	W.
Near Götha river, or Ronnum's bridge	296	18 45	45¼	W.
At *Wenersborg*	294	18 45	47¼	W.
West of Wenersborg	293	18 45	48¼	W.
By southern Ryrs church	283	19 0	58	W.
By the inn at Räkneby	283	19 0	58	W.
Near *Uddevalla*	281	19 0	60	W.
At *Uddevalla*	280	19 0	61	W.
b. On the other road to Trollhättan.				
On the eastern side of the Götha river, at the turn towards Trollhättan	302	18 45	39¼	W.
Further south in the lee of Hunneberg	306	18 45	35¼	W.
At Trollhättan	292	18 45	49¼	W.
C. *The road from Trollhättan, passing by Lidköping, over the eastern foot of Kinnekulle to Arnäs by Wenern*				
¼ of a mile (Swed.) nearer the inn at Bursle	280	18 45	61¼	W.
N.B. Here the country begins to be composed of sand; it had hitherto, from Ronmunsbro, or all along in the lee of Halleberg and Hunneberg, consisted of pulverized limestone and clay slate, with blocks of trap. A change in the soil may be perceived at a distance of several hundred yards.				
A little north of Bursle	285	18 45	56¼	W.
On the boundary of the district	288	18 45	53¼	W.
On coming to where the trap columns appear, and Halleberg separates from Hunneberg	310	18 45	31¼	W.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
Near Grästorp, on a place newly observed	° 301	° ′ 18 30	° 40½	W.
N.B. Afterwards the former road by Lidköping is followed.				
On the east of Kinnekulle:				
By Husaby church, *on sandstone*	328	18 0	14	W.
⅛th of a mile (Swed.) further to the north, also *on sandstone*	327	18 0	15	W.
At Fallösa church, *on gneiss*......	327	18 0	15	W.
North of Broqvarn	317	18 0	25	W.
At Forshem, see this Part, A.				
D. *The road from Wenern by Billingen to Wettern by Forsvik, and from thence northerly to Askersund.*				
To the east of the inn at Enebacken by Holmstaqvarn	317	17 45	25¼	W.
Further towards Kinneskogen ...	320	17 45	22¼	W.
Nearer Timmerdala church	320	17 45	22¼	W.
N.W. point of *Billingen, on trap*	315	17 45	27¼	W.
Further in on the plateau.........	320	17 45	22¼	W.
Road by Billingen between Varnhem and *Sköfde, on trap*	320	17 45	22¼	W.
....	322	17 45	20¼	W.
Further on..........................	325	17 45	17¼	W.
All these observations have likewise been made on the mountain plateau.				
By the inn at Tibro	336	17 45	6¼	W.
By Spethult's inn..................	336	17 30	6½	W.
By the house at Masebo	336	17 15	6¾	W.
By the southern side of lake Viken, or by Hulängen's inn ...	336	17 15	6¾	W.
At the inn at Ofverby, near Undenäs church........................	338	17 15	4¾	W.
N.B. The road afterwards continues on the eastern shore of Unden, and is particularly hilly, even when it goes over the high land at Askersund.				
At the turn of the Stökiska road.	349	17 15	6¼	E.
By the town of Svanhult.........	359	17 15	16¼	E.
By Kungsbacken	349	17 15	6¼	E.
North of the inn at Sannerud, near Aboholm	350	17 15	7¼	E.
Near the boundary of the district where high land appears to be	359	17 15	16¼	E.
At Skalltorp's inn..................	357	17 15	14¼	E.
On the road to Askersund	349	17 0	6¼	E.
NINTH PART. *The coast from Uddevalla to Christiania.*				
The observations are here made at *Strömstad* *	295	19 32	45 28	W.
and *Christiania*	298	19 50	42 10	W.
by M. Boström, M.A., who on many places found the furrows as they are here given.				

* All the angles laid down on the Map, except those at Strömstad and Christiania,

To make the difference in direction of the furrows plainer than it has been made by studying the foregoing description, they are drawn on the accompanying map (Plate IV.): the principal furrows which have been hitherto observed will be found thereon, as well as their direction *. On observing this map it is seen that the difference in the direction of the furrows is, on a distance of only from six to ten (Swedish) miles, very considerable; that which at first most awakened my attention, was the great disparity found between the furrows at Fahlun and those south of Gefle. They run here $26\frac{1}{2}°$ easterly; but, on the contrary, at Gefle above 30° westerly (Third Part, A). When I first had these observations only, I began to suspect that some indication regarding their divergence from the direction was to be found. My journey extended, however, only to Söderfors, and the same way back. Afterwards, on another tour, I found in the road by Upsala that the furrows in the neighbourhood of Grisslehamn had, from the western direction at Gefle and Söderfors, again reverted to 7° easterly. This did not assist in explaining the cause, but, on the contrary, appeared to show that the furrows run without any regularity. Finally, I investigated the whole coast, from Gamle, Grisslehamn to Gefle, and found the direction of the furrows at every place as they are now shown on the map.

By this, as well as when I was enabled on the charts of the gulf of Bothnia to learn and know the rocks which, under the name of the Finngrund (Finn Shallow), lie beneath the surface of the sea, it was apparent that the furrows at Gefle and Oregrund were side furrows, on each side of the Finngrund, by which the rocks on the coast assisted in a considerable degree to increase the deviation.

Another and not less important difference in the direction is that which is found between the furrows on the eastern coast around Westervik, as well as on the western coast of Uddevalla. To explain the relation they bear, I have, on the basis of M. Hisinger's researches, as well as my own† on my journey, made

do not include the magnetic variation, but these apparently do; thus—

Strömstad	19° 32′ + 45° 28′ = 65°	Map, 63°.
Christiania	19° 50′ + 42° 10′ = 62°	Map, 63°.

* [The variation of the furrows to the east or west of south is shown on the Map by the figures on the right or left hand at the head of the arrows.—Ed.]

† I had not an hypsometrical instrument, only a level which turned once.

observations on the configuration of the land, and sketched the profile * extending across the country.

The first of these follows the Dalelfven through Osterdalerne, as far as to Snöhättan in Norway; thus to the westward much beyond what the map shows (Pl. IV.), and the object of which has been to bring forward something which shall be explained hereafter.

The *second* profile goes over the flat land in Eastern and Western Göthland. The land here is generally very low, and the hills which appear on the profile lie partly isolated, partly in connexion with the hills further to the southward; so that they do not properly belong to this profile, although, from the higher mountain to the southward, they jut like two projecting points to the north, on both sides of lake Wettern.

The *third* profile shows the land south of Jönköping, whence it quickly rises to a considerable height, and stretches itself to a broad plateau over the province [Län] of Jönköping, as well as over a considerable part of the provinces of Wennersborg, Wexiö, and Calmar.

If we now compare the *second* and *third* of these profiles, we find that the boulder-flood, when it passed over the 58° of latitude, rushed upon a hill considerably higher than that which it had just before passed. Besides, the greater hill was composed of much harder substances than those which appear in East Göthland, on the northern side of the Götha Canal; and there, to all appearance, a considerable hill, composed of a coarse-grained loose granite, has been carried away by the boulder stream †. It is thus highly probable, although it cannot until after a more minute investigation be considered as decided, that the hill to the south of Jönköping has caused the important deviations which the furrows make at Westervik and Uddevalla, as well as that these furrows are side furrows belonging to the before-mentioned hill.

What is further to be remarked in regard to the difference in the direction of the furrows in different places, must be reserved until we shall have to make some reflections on the general direction of the boulder-flood over the whole of Scandinavia. In the mean time it is probable, that however great

* [This is pl. ix. in the *Vetenskaps-Acad. Handlingar.*—Ed.]

† For building on the sides of the Götha Canal the only granite for use was that which is met with as boulder stones.

this difference may be, we can nevertheless hope, that it will be possible at some future time to discover the cause; although the theory which I now have the honour to bring forward is not accurately confirmed. The phænomenon, at least, appears not to be the result of a condition of elements which has been as changeable as the wind.

What I have hitherto had the honour to adduce, are for the most part facts alone. They have been collected with care, but require however to be confirmed by observations repeated with more nicety, in every place where opportunity offers, even out of the route where observations have been already made, before any satisfactory conclusion can be drawn. I might here conclude this treatise; but with a hope that a more general interest may be taken in the subject, I wish to state some points of view in which I consider it ought to be regarded, and which possibly may serve as a guide to future observers of this phænomenon. This shall be done in the following paragraphs; nevertheless I request that every one who may give it his attention, and commence making observations, will take care in every place to search for the *normal furrows*. He must not, however, spare himself the trouble of observing all the side-furrows, and carefully noting them; for it often happens that on the same spot one cannot exactly see those which are side-furrows if they do not touch a lesser hill: nor until after the investigation of the tract, when from some hill clear of wood the whole can be overlooked, is it easy to say which furrows are normal.

Having now concluded the description of my observations on furrows, may I be permitted to accompany them with a more explanatory detail of the hypothesis to which they have given rise, and which served me as a clue in pursuing my investigations? The geologist cannot proceed in the way pursued in other sciences; he can only bring forward the results of what has been; but what the cause is he may guess, without coming to other or greater certainty than that which is gained by increased probabilities for the hypothesis.

Geology, as a perfect science, would be nothing more than a history of the changes which the surface of the earth has undergone from the commencement of her revolving round the sun. But this history is irrecoverably lost; it has been written by no human hand, and no nation has been found which could transplant its traditions. We must thus, from what we see, endeavour to form an idea of the occurrences which have passed; and this conjecture, which in other sciences would be an unwarrantable assumption, instead of the result of a well-grounded investigation, is in geology a necessary and essential part of the science. In the mean time conjectures are made in various ways, according to the different views taken by different persons; and the opinions of those who have made the nearest approach to the geological cause, have never the confirmation of certainty. I therefore foresee that the phænomenon which I have described may possibly by many be ascribed to other causes than those which according to my own views appear most probable for explaining the origin of the furrows and boulders.

§ 5.

Of the period when the Boulder-flood took place.

If we trace the furrows on a hard rocky substance which is not acted upon by the atmosphere, we might believe that they were formed only a few years back, and that they never could be a monument of events more ancient than the commencement of human chronology. Should we assume, with some geologists, that the action of the atmosphere on the mountains has formed the valleys, this would oblige us to infer that the boulder-furrows in various places were not many centuries old. But that such a notion must evidently be incorrect, is to be inferred from two certain facts. The first is that which has come to our knowledge through the investigations made during Napoleon's Egyptian campaigns, that in the blocks of stone found remaining at the quarries from which the building materials were taken for the pyramids, and which appearing to have been too large for them to remove, were thus left in their rough-hewed state, the marks of the chisel remain as fresh as if they had been made only a few years back; they are, however, now nearly three thousand years old*. Secondly: At the great fall of Avesta, as well as at the so-called little fall, various slabs are to be found with uncommonly beautiful furrows, which make an angle of from 75° to 86° towards the present direction of the Dalelfven. The Dalelfven has also passed over these furrows perhaps at a much remoter epoch than when the Egyptian

* The pyramids themselves are said to be covered with limestone slabs which are decomposed by the atmosphere.

pyramids were built; it has carried with it sand, gravel, and stones, which naturally must have caused abrasion; but after ages of time this has not been great enough at any place to have lessened the distinctness of the boulder-furrows. From this their evident and unaltered condition we seem justified in receiving them, not as belonging to monuments of later times, but as those which must be ascribed to a remote age.

Nevertheless they are of later date than the Swedish geological formations of *transition sandstone* and *lime* in Eastern and Western Göthland and Dalecarlia, more recent than the *trap* of the West Göthland mountains, the *porphyry* in Dalecarlia, and the *red sandstone* formation (Keuper-bildningar) at Omberg, since all these in part show traces of furrows, and in part are found as fragments among the boulders. I am not aware if the *green sandstone* and *chalk* in Scania are more recent* than the furrows.

§ 6.

Have our mountains been changed in their position, elevated or depressed, since the Petridelaunian furrows have been formed?

This question I proposed to myself from the first commencement of my observations, and my particular attention has thus been drawn to it. No satisfactory solution, however, has been obtained. If we examine the mountains which lie in the neighbourhood of Fahlun, both easterly and southerly, and as high up as Särna church (above 1500 Swedish feet), we find no reasonable grounds to suspect that they have been displaced from the position in which they were when the boulder-stream passed over them; for so far as I have hitherto been able to investigate the direction of the side-furrows, it everywhere agrees with them. However, this needs a more accurate examination upon those mountains which on all sides have furrowed slopes, or which lie in any way isolated. In the mean time we may conclude that if some change had taken place, several combined localities must have been elevated above their former level, for neither large nor small fissures or faults are to be found. Older fissures which are filled with trap often appear, but the trap is furrowed.

South of Bröms, on the boundary between the districts of

[* On this subject the reader is referred to Mr. Lyell's paper on the islands of Seeland and Möen, in the Geol. Trans., 2nd Series, vol. v. pt. 1, in which the boulder formation of the Baltic is shown to overlie the chalk of Seeland and Möen.—Ed.]

Calmar and Blekinge, I observed a phænomenon which appeared to be an exception to the before-mentioned hypothesis. The road goes there for a great way over level slabs of granite, on which furrows are not visible, for the granite is too coarse-grained to receive them; however, by observing the opposing and the lee-sides, the direction of the furrows may in some measure be determined. It is at least certain that the surface has been worn away by the force of the boulder-flood. But the surface of the slabs do not lie successively, but are broken, and the pieces which had lain together during the attrition lie now with their sides irregularly elevated or sunken, much in the same manner as pieces of ice formed at high water, but the water having fallen the ice rests on an uneven bottom. From this no other conclusion can be drawn than that the displacement has been occasioned by some subterraneous cause. Going further to the west, approaching the church of Lösen, there are seen, about two hundred steps from the road by a small cottage, some rocks of a very peculiar form. Investigating these more closely it will be found that they are all pieces of a large slab which has been furrowed on the surface; and that this slab, by a horizontal fissure, inclining somewhat towards the north, has been separated from the rock beneath. It is also soon discovered that this slab has been, by a shock from the north, removed a little further south, and that it has partly been crushed to pieces by the shock, and partly gone asunder by its own weight upon the uneven bed underneath. Something of the kind must also have been the case with the before-mentioned rocks, the removal of which does not thus proceed from one of the subterranean powers of the earth, but from an external one. Assuming the hypothesis, that our mountains, considered singly, remain unremoved after the boulder-flood, it would not be altered by the observations at Bröms.

What, on the contrary, relates to the changes of level on a large scale as yet remains still more dubious. Mr. H. Wegelin, who has made various investigations with respect to the Petrídelaunian phænomenon, and has extended them even to the mountains bounding Norway, Herjeadalen and Dalecarlia, has since communicated to me the observation, that the mountain ridges in these places do not consist of any fixed continuous strata, but of a mass of large and small pieces of rock which have sharp edges, and therefore have never been rolled, but were all rent asunder or shattered to pieces on the spot. If, on any part of these, furrows were met with, it would be clear that the shattering had taken place after the passing of the boulders; but no such discovery has hitherto been made. Meanwhile it is also uncertain whether the shattering to pieces occurred before that flood, because, probably, the flood could not have reached so high up as to the mountain ridges. The circumstance which Mr. Wegelin observed on the higher rocks around this place, namely, large detached fragments which are not worn by rolling, makes the case so much more doubtful, until we are enabled to investigate the circumstances more narrowly. Where I have had an opportunity of seeing the same phænomenon in some porphyry rocks to the westward of Lake Siljan, similar fragments had loosened themselves, by the action of the atmosphere, from the steep and fissured cliffs.

§ 7.

Of the probable rapidity of the Boulder-stream.

When I first began my observations, I remarked in several places, that if two furrowed rocks succeeded each other in the direction of the furrows, and were near each other, the northernmost was on the resisting side, and, if it was not too steep, furrowed as low down as the plain; whilst, on the contrary, the resisting side of the latter had been sheltered by the projecting rock situated before it; by which it is evident that where the boulders passed over the highest top of the first rock they struck on the latter at a certain depth. If we can therefore determine the point where the boulder left the former rock, as well as the point where it struck upon the latter, and measure their distance both horizontally and on the diverging line, we might hope that, assuming the dip to a second was the same formerly as now, we could thus calculate how great the horizontal rapidity of the boulders had been.

A diligent search has also been made to find a point where such a measurement could be undertaken, but hitherto without success, partly on account of the distance being too inconsiderable between the points, partly because the places where there might have been some hopes of finding such a situation have been covered with large masses of earth; and besides this, there is a circumstance which prevents its ever properly succeeding, namely, that the boulders were surrounded by gravel and sand, which, along with the water, prevented their free descent, and makes all calculation impossible.

A hope was afterwards entertained that on the eastern or western end of some level rock we might be able to follow a furrow to a much greater distance, in order to see how much it had sunk in a certain length; but even this hope was disappointed; and, had it succeeded, the calculation nevertheless would be always difficult whilst the friction is unknown.

§ 8.

How long the Boulder-stream may have continued. Was it uninterrupted?

The hope of receiving an answer to these questions has always been, and still continues, very feeble. Nevertheless it is possible to determine within what limits we are to look for the answer to the former question. From some phænomena we may ascertain thus much, that this flood was not one that quickly subsided, or suddenly ceased.

The *first reason* for the hypothesis that it continued for a long period is this,—that it could crush enormous masses of hard rocky substances, such as gneiss and granite, to a fine sand, with which whole countries of Europe are covered, often to a considerable depth. Nevertheless these accumulations of sand are very inconsiderable in comparison with those which form the banks at the bottom of the sea. Besides, it is not improbable that the masses of sand which constitute the great deserts of Africa and Asia also are the produce of such a flood, which thus could not have quickly subsided.

The *second reason* for concluding that it lasted for a long time is drawn from the phænomenon known by us under the name of Jättegrytor (giant-caldrons), or spheroidical hollows in the solid rocks, which are described by Thorbern Bergman in *dess Fysiska beskrifning om Jordklotet*, § 150*, and in most cases must be the relics of the boulder-flood; I say, in most cases, for there are giant-caldrons which are made by the usual operation of our rivers. Such are those near Avesta; and I saw

* Bergman's Natural History of our Globe.

some uncommonly beautiful ones some years since while they were digging the canal on the side of Ostra Dalelfven, by Gagnefs Grada. The canal was made where the river formerly had its course, and it was necessary to blast the rocks in order to make sluice-gates at the place where the river formerly had a fall of nine feet; several giant-caldrons were met with in the lower part of the fall from one to three feet deep, and some large enough for a person to stand up in. In each of these a large stone was always found, and sometimes several small ones, which have all been worn round, so that they all had a spheroidical form with a smooth surface. On the course of the river lower down M. Berndtson (mining officer) found a similar stone of porphyry a perfect spheroid, and which probably had been worn away to such a degree as to have become too light to remain in the giant-caldron. That these giant-caldrons have been caused by falls of water is unquestionable, and they are only mentioned here incidentally, and not as belonging to those which are to be adduced as evidence upon the point which we are now considering.

The *petridelaunian* giant-caldrons are of another kind. These are seldom situated at the place of a waterfall, but are often on the brow of a hill where the petridelaunian flood has gone upwards.

Such a giant-caldron, although inconsiderable, and only in the commencement of its formation, exists in the neighbourhood of Stockholm, on the side of a hill between Roslagstull and Albano. The petridelaunian flood has come there from the north over Brunnsviken, and has gone up over the steep mountain at Albano. On the western side of this mountain, near the present road, is a hole, the southern side of which projects a little, and consists of hard granite. Into this hole one or more boulder-stones had been conveyed, which could not be removed by the boulder-flood, as the projecting side prevented it, but continued to be whirled round by the flood, and thus formed the giant-caldron which has been partly destroyed in making the road.

One of the finest giant-caldrons of this sort, and which has been formed in precisely the same way, is at Trollhätta, namely, the one in which several royal personages have carved their names. It is so large that it can comfortably contain twelve persons. It is situated so high on the mountain above the river, that it cannot be supposed to have originated from the latter. The furrows found in the neighbourhood also show that the origin of the giant-caldron must be ascribed to the boulder-flood.

That such a giant-caldron could not be formed in a short time is evident, as this cannot have been the effect of force; the hollowing must have been produced by a long-continued friction.

The *second question*, namely, whether the stream had proceeded uninterruptedly or occurred periodically, is still more difficult to answer. The requisite data could not be quickly collected, for they are to be derived principally from excavations of the earth, or from sections made by the action of water; and as such works are not undertaken expressly in behalf of geology, we must wait until the circumstances present themselves for making observations. These ought not to be neglected, for after a short time the loose earth falls together, and its vertical section is no longer visible; and we ought to be the more anxious that such observations should be made, as the explanations derived from them may be of importance in an agronomical relation.

From the little which hitherto has been observed, there is reason for believing that this flood was not continuous. The alternations in our strata appear to show this. I have seen, for example, in a pit from whence the gravel for repairing the roads has been taken north of Krigsberg church in East Göthland, common granite sand alternating with another sort, which was probably sandstone sand, at all events not granite sand; and it is difficult to understand how this could be the case without some interruption in the flow of the stream.

This question will probably at some future time be treated just as circumstantially as there is now little to say on the subject.

§ 9.

Was the Boulder-flood of great violence?

Had the questions in the two preceding sections been answered, this would also have been solved; but in default of this some phænomena must here be mentioned, which in the mean time may serve to prepare the way for a solution of it.

a. It has already been mentioned, that there are to be observed at Blekinge large rocks which have been removed up-hill, in a southerly direction from the beds on which they rested; and it would appear that this could only be accounted for as the consequence of the rushing force of the boulder-flood.

Besides, we have from various geological authors the fact that large detached rocks are met with on the Danish islands as well as in the north of Germany. But as others maintain that the ice has contributed to this removal, we must here mention a large stone which probably has not been removed in such a manner, because it is not far from its former position. It is situated on the eastern side of the height on which Pelarne church in Smaland is built, and is calculated, according to measurement, to weigh at least seven million pounds.

To this class of phænomena belongs the experience of the fact, that in Sweden generally, on opening new mines, the solid rock is seldom found at a less depth than from one to two fathoms. Until we reach this, the mountain is commonly as if it had been shivered, and is full of fractures and fissures which are filled with fine slimy powder of the same or some other rock, but not unfrequently end in a large open chasm. This is very clearly seen about one mile from Fahlun in the newly commenced mine at Skinnarängen and Kraknäs.

b. A still more remarkable phænomenon of this class, and which apparently proves that the petridelaunian flood has acted with great power, is found in the hills of transition rocks in West Göthland. These hills rest on granite from 150 to 330 feet above the sea, and are themselves from 500 to 700 feet high, all composed of the same rocks in the same order, namely, undermost sandstone, above alum-slate, still higher limestone, and uppermost clay-slate.

According to the opinion of Baron Berzelius*, which is generally received, these transition rocks have covered the whole of the plain of West Göthland, but were by some violent cause broken up and conveyed away, except in those places where a hard, tough trappean rock, ejected in a state of fusion from the interior of the earth, spread itself in small sheets, and thus protected the underlying loose transition strata. This view continually gained strength, and I was fortunate enough last summer to find a further confirmation of it. In the *first* instance I found, as far north as the foot of Tiveden, a quarter of a mile (Swedish) north of the inn at Hasserörs, small balls

* *Årsberättelsen*, 1825, p. 285.

of sandstone remaining, which were furrowed. The rocks lying above had been carried away, and this clearly by the boulder-flood. In the *second* place, I found that the trap on the top of the mountain had furrows which, particularly on Billingen, were uncommonly plain and regular; and in the *third* place, I was convinced, in a way not admitting of mistake, that the yet remaining parts of the transition mountain would have been completely removed, so that no one could have had the least idea of their former presence, if the trap had not lain as an unassailable heavy mass on the loose transition rocks, and presented a firm barrier against the attack of the petridelaunian flood.

This appears plainly at Hunneberg. Travelling from Lidköping to Wennersberg the road passes between the two mountains Hunneberg and Halleberg, whose transition rocks are seen in a vertical section covered above with a mass of trap more than a hundred feet thick, the surface of which consists of a row of equally high perpendicular pillars of the common basaltic form. At the foot of the mountain, from 100 to 300 feet high, lies a mass of shattered pieces of trap which often hide the profile of the transition rocks. The whole can be seen at a distance of several miles, and presents a very uncommon appearance; even the pillars can be distinguished, because the top of the mountain is flat to the edge without any rounding off, and the sides are perpendicular. The first approach to Hunneberg is at a place where a rivulet descends from it to Dettern*, and where the mountain is hollowed out in a defile through which it can be ascended. Further on, or nearer to the inn at Munkesten, the earth has been removed for the purpose of quarrying limestone; and as there the fallen down trap has been removed, it appeared evident that the underlying loose transition rocks had been carried away by the boulder-flood; the trap therefore projected, or overhung, so that a small path was formed between the shattered masses on the one side and the transition rocks on the other, with the trap for a roof, resembling a covered passage along the side of the mountain. The shattered mass at the foot of the mountain consists of nothing but loose pieces that have fallen from the overhanging trap, the remains of which show themselves in the beautiful forms of the basaltic pillars. It is clear that, since the trap lay so firmly fixed, the flood would with much greater difficulty carry away the loose rocks lying under

* A bay of Wernern.

it, as they could thus be attacked only at their edges, where they offer a much greater resistance; much in the same manner as it is more difficult to tear the leaves out of a book which is placed between other books.

Notwithstanding the protecting power of the basalt against the flood, it is however possible that nearly the whole would have been carried away unless a *fourth* incident had occurred, which still further confirms the accuracy of Baron Berzelius's view of the case.

The hollow before mentioned down which the rivulet flows, and by which one can ascend, is not, as might be supposed, the consequence of the action of the rivulet. There are larger watercourses on the other side of the mountain, which have not been capable of forming the least excavation in the trap. This hollow is undoubtedly the opening through which the trap ascended, and where, on cooling, it contracted and left a hollow, one side of which has been torn off by the boulder-flood. This appears evident, if we attentively follow the deepest part of the excavation from the bottom upwards, when it will be perceived that the trap has forced itself up through the clay-slate, the laminæ of which are bent upwards on both sides, so that those which are near to the trap or in contact with it, have been changed by the action of heat to a hard rock, in which scarcely any trace of lamellæ can be seen.

At the spot now mentioned the boulder-flood encountered, under the trap, not the loose transition rocks, but true trap and hard burnt clay-slate, which made an invincible resistance; and as this place is situated just on the opposing side of the mountain, it follows that it would, in a considerable degree, protect the whole mountain from being washed away.

On the other mountains of West Göthland, it appears that a large bed of hard limestone has afforded the same protection.

A *fifth* reason for Baron Berzelius's view is, that fragments of the strata from the higher part of the mountain are met with among gravel and boulders only on the side of the Hunneberg which lies to leeward; and that the transition from these fragments to such as are composed chiefly of sandstone sand, is, for example near the inn at Bursle, so distinct, that in a distance of about two thousand feet, we remove from the one detritus to the other.

Baron Berzelius's view thus appears to possess as much certainty as any explanation of causes so remote can be capable of; and for the same reason probably we must also assign to the boulder-flood a considerable force.

In the provinces of Orebro and Calmar, East Göthland, and Smaland, we find traces of something similar. From four to five miles (Swedish) from Orebro, south of Wredstorp, and the inn at Svennevad, we meet with a gravel of limestone and alum-slate, just in the direction of the boulder-flood from the former situation of these minerals; and high up on the hills south-east from Jönköping, the sandstone of East Göthland is to be met with.

It is besides very commonly to be seen that the boulders are conveyed upwards over the mountain slopes, which have from 33° to 40° inclination, to effect which not a little force must certainly have been required.

§ 10.

The depth of the Boulder-flood.

This question cannot be answered before it is first decided whether the ridges of land have since been considerably elevated. If this has not taken place, we now know that the furrows are found twenty-one feet below the surface of the sea (at Carlskrona), as well as somewhat more than 1500 feet above it (near Särna in Dalecarlia). In the mean time, we might conclude that such mountains as Högkarnsklak, Pilboberg, Taberg, Omberg, Billingen, and Kinnekulle, have retained their relative heights above the surrounding land, such as they were when the boulder-flood passed over them; and under these circumstances it has produced its effects at a depth of at least eight hundred feet, and has to a great height carried with it boulders which have furrowed the mountains.

§ 11.

On the general direction of the furrows in the South of Sweden.

On this point also it is now too early to form a decided judgement; but some reflections may be made, and ought not to be neglected, as they afford an opportunity of making closer investigations, particularly such as are caused by adverse opinions;

for in this way the value of observations on which the answer must depend will be most effectually tested.

While looking at the map (Pl. IV.) on which the furrows in the south of Sweden are traced, we cannot refrain from asking this question: *Has the boulder-flood come from the highest tops of the mountains of this country, and thence taken its course in all directions*; or, *has it come still further from the north, and gone over the whole country; at least so much of it as lay under the level of the flood?* But the map also shows, that we can hope for an answer to these preliminary questions, only when the furrows shall have been observed in the north of Sweden, in Norway, and Finland, and possibly from thence find a solution of the principal question. For a more general answer, it will probably be necessary to observe the petridelaunian phænomena in Russia and North America, where it is already known they are to be found.

Something, however, may be said hypothetically, judging from the experience already acquired.

If we observe the direction of the furrows around Fahlun and Uddevalla, as well as around Westervik, these would appear to show that the flood had come from the highest land, and had flowed down from thence on all sides. Whether this occurred when the land-ridges were elevated, or after the land-ridges had attained their height, from a water-spout or something similar, is a question which will first admit of being answered, when it has been ascertained whether the water streamed in all directions.

It is possible that it happened, as maintained by Keferstein and other geologists; but except the circumstance mentioned of the direction of the furrows, no other known fact speaks for it. We shall see what experience in some instances says on this head.

a. The highest points of land in Scandinavia lie at Snöhättan, north-west from Christiania; the furrows at the last-named place should therefore run towards the south-east; whereas they go towards the south-west, or at right angles with the hypothetical direction.

b. In the same way the boulder-flood should, if it had descended at Gefle, have gone towards the south-east; but here also, as well as along the northern coast of Uppland, the furrows run to the south-west, and not down-hill, but ascending, and at many places over very steep slopes and hills, as at Carlholm, at Oregrund, at Harg, at Grisslehamn, on Väddon (island), over Vardkasberget, and at other places.

c. Eastward of Carlskrona, the circumstances are nearly identical.

From these indications it appears probable that the boulder-flood did not come from the high land, but that its direction was over the Gulf of Bothnia and Bottenhafvet, to the south-west or south-south-west.

This is contradicted indeed by the direction of the furrows around Fahlun and Westervik, should these be normal-furrows; but if they, on a closer examination, should be found to be side-furrows, namely, those around Fahlun, from the high land of Dalecarlia, and those near Westervik to be side-furrows from the great heights south of Jönköping, they would then prove nothing against the hypothesis that the boulder-stream came from the north-east*.

On the contrary, this assumption is further supported by the following observations:—

d. The great collection of lakes (vattendragen) and islands: Wenerns, Wetterns, Gottlands, Olands, stretching to the south-west. The bights of the Mälar stretch out in another direction, as well as the furrows in its neighbourhood; but these are continued from the Fahlun district, and become afterwards side-furrows to the considerable heights south of Strengnäs and Eskilstuna.

e. The sand-banks in the sea, south of Finngrunden, south of Göthland's Sandö (sand-islands), south of Göthland, and south of Bornholm, also have a south-westerly direction.

f. On the northern or resisting side of the point which the Swedish coast forms outside Stockholm (Roslagen and Södertörn) the sea is tolerably free from rocks; whilst, on the contrary, the southern or lee-side of the same point is full of them. This does not appear so clearly on the accompanying map as on maps on a larger scale, provincial maps, and M. af Klint's sea-chart. To judge from the new general map of Russia, the

* A circumstance which makes it very probable that the furrows around Fahlun are side-furrows, is the steepness of all the mountains here on their eastern sides, where the slopes also are most exposed.

northern coast of the Gulf of Finland should in the same manner be the lee-side; the southern coast, the opposing side. A good map of North America would serve to show the direction of the furrows in that country.

g. In conclusion, we must moreover, as a proof of the often-mentioned assumption, adduce the considerable difference which is found in the direction of the furrows on the tops of Omberg, Taberg, Billingen, Kinnekulle, and Stora Tiveden (east of Finnrödja church), as well as the furrows at the eastern foot of these mountains. On the summits the flood has followed the general direction; whilst, on the contrary, near the foot of the mountain, they follow the irregularities of the country in order to flow forward between them.

The true directions can, however, only be discovered at some future time.

§ 12.

Of Sand Hills.

The question of the formation of sand-banks is apparently connected with the subject of the foregoing section, and will receive its explanation when the main question is answered. In the mean time I have directed my attention to them, as their existence had previously excited so much inquiry.

In the next place I must remark, that they are not correctly laid down on the maps of Baron Hermelius and M. af Forsell. They are seldom found of such great extent as shown on the maps; they are composed of several small ridges, which appear to have no other connexion between them than that the public road goes over the whole of them, for this reason only, that our forefathers, in road-making, appear in preference to have chosen the elevated lands.

The lesser hills, on the other hand, are situated oftenest on the lee-side of some rock; but they are sometimes found without any such protection.

They have mostly the same direction as the furrows in the same tract, and seem readily to have fixed themselves where these have diverged or converged; but, in general, it appears equally difficult to discover any rule for their formation as to explain the cause of the snow-drifts after a storm even when the wind has been unvarying. The least unevenness on an otherwise level piece of ice, will give rise to a large snow-drift of the most remarkable form. There are, besides, banks of sand and gravel of inexplicable shapes continually forming, even under our eyes, in streams and by great floods.

§ 13.

Of the Force which caused the Boulder-flood.

The ultimate cause of this force will probably for ever be concealed from our researches. This knowledge belongs alone to Him who shaped the world, and whose causes are always proportioned to the object to be effected; these we can generally afterwards see, but for this a long time is requisite. In this manner we now discover, that however destructive it might appear to us mortals for such a flood to have passed over the earth, yet we find that if this had not occurred, if it had not reduced a great part of the surface of the earth to gravel, sand, and mould, it would have consisted only of crags on which nothing could exist; being incapable of sustaining vegetation, neither animals nor man could have resided upon it.

But if it is not permitted for us to see the first cause of this force, we need not nevertheless give up the hope of being able to find out its nature.

When we see that the boulders have been in motion against the fast rocky surface of the mountains, and that the former have struck with a considerable force against the latter, two probable suppositions offer themselves as to the manner in which this manifestation of force might have arisen: either, that the boulders were thus, by some exterior force, set in motion towards the surface of the earth, which in relation to them was at rest; or else, that the earth was moved in a direction according to which the boulders were relatively at rest.

The arguments in favour of the hypothesis of a change in the position of the earth's axis and velocity of rotation, and the consequent effect on the precession of the equinoxes, as well as the opposing arguments, are sufficiently well known, but are not of such a nature as to offer any encouragement to our adding this hypothesis to the number of existing theories.

We will therefore limit ourselves to this question only,—whether any indication is to be found which will show whether the boulders moved towards the earth, or the earth in a direction opposite to the masses of boulders. Such an indication might possibly be found.

If, for instance, the observations on the furrows could be extended all over the world, that is to say, if furrows are found in the southern in the same manner as in the northern hemisphere, it would no doubt enlighten us considerably in regard to the before-mentioned question.

It is likewise credible, that if the boulder-masses moved against the earth by their own force, or still more by a *vis à tergo*, in such a case the rapidity of the boulder-masses must have been greater on the surface than in depth; while, on the contrary, the rapidity at the greatest depth must have been greatest, in case the earth had moved itself against the boulder-masses. Now, should any indication be found by which it could be determined whether the rapidity had been greater on the surface or the contrary, the answer might be given with some degree of certainty. A closer investigation of the mountains of West Göthland might possibly throw some light on the subject.

If the boulder-masses were moved by a *vis à tergo*, it would appear that it would be more difficult to detach large pieces of rock from the lee-side of the mountain, and that some difference would present itself in the situation and character of the masses which lie to the leeward.

The section also of any considerable masses of gravel and sand might afford some explanation; for where the rapidity has been very great, the fine earth must have been washed away, and the gravel have remained. The effects in this respect are very remarkable below Avestad, where Dalelfven has cut its way through a bed of sand a hundred and fourteen feet thick, below which smooth rocks are found, which are furrowed at right angles to the river's present direction. Had the boulder-flood been moved by a *vis à tergo*, it would soon have filled the dale in which Dalelfven now flows, while crossing it, and the rocks around have been less furrowed.

But the most important of these observations must be left to futurity.

§ 14.

Some concluding Remarks.

(A.) Those who would make observations on the furrows should be provided with the following instruments:—

1st. *A small hand-compass,* with a square bottom plate, having a well-adjusted needle on an agate bearing, which rests on a finely pointed pivot which can be often changed. In order that this might be done by any one, the pivot should be inserted in a hole drilled into a coarser piece, and fastened with shell-lac. On the bottom plate there should be a graduated semicircle over which a plumb-line can freely traverse, for determining the inclination of the side of the mountain. Two of the sides of the bottom plate should be parallel with that diameter of the graduated face of the compass which unites 0° with 180°. In making observations the compass is placed horizontally, and afterwards turned parallel with the furrows the direction of which you wish to observe. When the needle, after a gentle tapping on the compass, stands still, the number of degrees shown by the needle's north or + end is to be read off, and noted.

2nd. *An azimuth compass,* partly for occasionally showing the magnetic variation of the needle, partly for determining by it the direction of the furrows in places where the magnetic needle is disturbed by ore or other causes.

3rd. *A portable leveling-instrument,* the telescope of which should magnify from ten to sixteen times, and the vernier read to fifteen seconds.

4th. *A graduated measuring tape* of 150 feet.

5th. *A good telescope* which magnifies above twenty times.

6th. *Two portable barometers,* or two *Hypsometrical thermometers* of Mohrstadt's and Gintl's improved construction.

In tracts less explored, one should be supplied with,—

7th. *A sextant* with an *artificial horizon,* and a *chronometer,* for the accurate observations of the latitudes, longitudes and azimuths. The telescope should also be such that occultations of the stars could be observed with it, as well as eclipses of Jupiter's satellites; and it should have a stand.

8th. For investigating the nature of minerals and earthy substances, one should be provided with a blow-pipe apparatus, as well as the most common reagents.

(B.) Besides the scientific interest deserving of encouragement which attends researches on this subject, and which is in itself a sufficient motive with those who find a pleasure in the study, investigations regarding the boulders and the nature of the loose strata of the earth likewise offer something which may be of utility in an œconomical point of view.

1st. In mining, something serviceable would be found in a knowledge of this subject. If, for example, a valuable mineral is met with among gravel or boulders, by tracing out the furrows in the same tract, we may be certain that, according to their indication, the situation of the mineral in the solid rock will be met with. If the boulders are small, the place whence they came may be far distant, particularly if but few specimens of them are to be found; but if the boulders are large and the specimens numerous, their proper situation is not far off. Thus about forty years since, a considerable quantity of copper ore-boulders were found to the southward of the Fahlun mines, and Mr. Wegelin discovered the original place before Broddbo Albit-granit on the Varggard hills. In the same manner, from the alum-slate boulders in Nerike, may be found the stratum of alum-slate. There is also no reason now to be led astray, and expect to find Arholm sandstone in the rocks in any place in Uppland. In any new shafts sunk in searching for ore, when, after a trifling depth, a bed of clay occurs which cuts off the continuation of the minerals, we should not be deterred by this, or be uncertain as to what should be done to find the direction of the ore.

2nd. By a knowledge of the direction of the boulders, we may also be able to throw light on the labours of the agriculturist. One of the principal objects in agriculture is, to determine the component parts of the soil. In most cases the soil is composed more or less of fine sand, or slime of different minerals. To judge of the fitness of the soil for different sorts of seeds, grass, and trees, it is necessary to know of what substances it is composed; for even if it were so that it was composed of nothing more than what is termed sand-earth (sand-jord), it is by no means indifferent whether it consists of Rapakivi-sand, as in Finland, of Felspar-sand, as in Dalecarlia, whose so-called clay (leror) is nothing more than a very fine felspar slime; or of sandstone-sand, as is probably the case with the wastes of West Göthland. This is to be determined with certainty by a good chemical analysis; but, besides that this is difficult if it is to be done so as to lead to any conclusion, we can in no way so easily attain the object, as when we know of what minerals the soil is composed at a given place. In this respect we need not be doubtful regarding the fruitful plain which lies on the lee-side of Hunneberg, or on Westgotha-Fahlbygd, and we may nearly everywhere come to the same decision if we closely investigate the relative circumstances, unless the case be such as in Uppland and Roslagen, where the soil has come from the sea-shore. The tract around Elfkarleby is by far the greater part a *delta* of the Dalelfven.

3rd. For navigation also, a knowledge of the *resisting* and the *lee-sides* of the coast, as well as the points and islands, may be serviceable; for on the resisting side the rocks are generally cleared from sand and gravel, and are very smooth, and have only isolated beds; while the lee-side, on the contrary, has sharp peaks, pointed sunken rocks, and reefs of gravel and sand. The right-hand and left-hand sides have generally the deepest channels; and, on the contrary, the resisting sides and lee-sides are the shallowest. If the question be as to the digging of a canal, we shall not so soon meet the rock on the lee-side.

APPENDIX TO THE FOREGOING ESSAY.

Later observations, made partly in Sweden, partly in other countries, communicated in letters to the Royal Academy of Sciences; by N. G. SEFSTRÖM.

Vienna, June 8, 1836.

On a journey, during which my route was in a southerly direction towards Vienna, I have been enabled to make some further observations *on the boulder-stones and their course*, which I wish to communicate as an appendix to my essay on the Furrows on the Swedish mountains.

On the mountain plateau which lies to the south of Jönköping, and which, about 1000 feet high, extends itself on the eastern and western coasts of Sweden, I now took another road which crossed the one I had previously taken, from Carlskrona to Jönköping, by way of Eksjö to Christianstad. On this plateau I made several observations on the furrows, which correspond with what I have mentioned in my former essay; but as soon as I came down from the plateau, westward of Wexiö, to about five hundred feet above the level of the sea, no shelves were afterwards discernible. The country consisted now, on the lee-side of the heights, of sand-hills and ridges. The ridges were composed in many places altogether of boulder-stones; the hills had, in the beds of their streams, a mixture of clay. Somewhat to the north of Christianstad there again appeared, though rarely, shelves, not however such as were suitable for determining with accuracy the direction of the furrows. I could only see that they, as at Carlskrona and Brömsbro, went decidedly to the *south-west*, apparently more south-westerly than at these places. Between Christianstad and Ystad, the large sand-hills are intimately mixed with shelly gravel (snäckgrus), powdered chalk (kritpulver), and pieces of cretaceous flint (kritflintstenbitar).

In Pomerania the pieces of flint are in still greater abundance, to such a degree, that even to Pasewalch it answers to separate them from the sand by riddling, for the purpose of making the new high road. Passing Prenzlow and Berlin, the sand is less mixed with cretaceous flints. Around Jüterböck the sandbe gins to contain a mixture of pure white quartz, which, by attrition, has been worn down to the size of walnuts and hazel-nuts. This increases around Elsterwerda, and for about three Swedish miles further eastward. At Morizburg, near Dresden, the solid rock is again visible, and appears to have been very much battered by the boulder-stones, but it is without furrows. I had not sufficient time to visit Freyberg, where I certainly should have procured the most correct information; but not, however, altogether to give up the advantage of the passage over the Erzgebirge and Sudeterne, I took the road through the Saxon Switzerland, as it is called, so famous for its high sandstone rocks, with perpendicular precipices on all sides, and whose form does not appear to be compatible with the occurrence of any boulder-flood. As this tract might be cited as an evidence against the flood being general, and thus raising an apparent objection to it, I could not neglect to make myself acquainted with it.

When we approach this district from above Pirna, and see Königstein, Lilienstein, and Bährenstein, which are pretty accurately represented in the prints that are usually for sale in the shops, we cannot understand how such isolated rocks of a loose sandstone, so upright, high and steep, but so small in circumference, should have withstood a flood which we suppose carried away whole tracts of transition strata in West Göthland. But when we come nearer, we perceive, partly that the sandstone of these rocks is of a much harder nature, and more especially that the steep precipices have not been affected by the petridelaunian flood, but that, on the contrary, they are formed of clean unchanged surfaces, produced by the falling down of the large masses of stone which now lie around the foot of the remaining rocks, similar to those at Hunneberg and Halleberg in Sweden. Had I not previously had the opportunity of acquiring a correct knowledge of these last-named rocks, it would have indeed been difficult to comprehend the true cause, although it now appears to me very probable, and even discernible in the above-mentioned prints.

The undermost layers of sandstone, being looser, have evidently been undermined; and, in consequence, the uppermost, overhanging like the trap in West Göthland, have since fallen down. The existence of the steep sandstone precipices is not however everywhere to be attributed to this cause; but many appear to have originated in the rending of the sandstone masses, when the basalt, which is now to be found on the higher mountains in the neighbourhood, was forced up. That such is the case may be seen by attentively tracing the layers of sandstone along the banks of the Elbe, where we find that the layers between the great crevices incline here and there; and probably these deep crevices, which are found in the masses of sandstone after the protrusion of the basalt, have in some degree assisted to make the undermost loose layers of sandstone accessible to the boulder-flood, particularly in those places where the crevices lie in the direction of the flood, and are open at both ends. The nature of the locality seems to favour this opinion; for in the cross valleys, and those closed at the upper end, the working of the flood is evidently less. Besides, the looser sandstone has also resisted the force of the flood, viz. where it is closely intersected with veins of basalt, in the way in which it occurs at *der kleine und grosse Backofen*, and which have sheltered Schramsteine, which lies to the southward. On the lower part of the precipices in that neighbourhood, traces of the course of this flood are seen, whilst the upper part of the precipices have clean surfaces. The tops, on the contrary, are always rounded. I have therefore no doubt but that the boulder-flood even here has acted in the same way as in Sweden.

But to this action two questions appertain, to which I have not yet received an accurate answer. The one is, *how high has the flood reached here?* The highest mountain I visited is the Winterberg, south-east of Schandau. This basaltic crater, about 1820 Parisian feet above the sea, had been strongly attacked and undermined by the boulder-flood; but to judge from appearances, the flood has gone over much higher mountains than are found in this tract. This, however, cannot be considered as certain. The other question is, *what was its direction here?* There are many evident indications that it went from north to south, as there are few places where we cannot plainly distinguish the resisting and the lee-sides of the mountains; that is, if observations are made on the heights, without descending into the valleys which everywhere intersect each other, and where the flood has sought its way through many outlets; but as on no spot are distinct furrows to be discovered, and the compass was often disturbed by the basalt disseminated throughout the sandstone, it was impossible, during the short visit which I was enabled to make in this nearly inaccessible tract, to determine with nicety the direction. Thus much, however, could be seen, that the deviation from the meridian is inconsiderable, and that the divergence is towards the south-west.

From the Switzerland of Saxony I proceeded in a boat up the Elbe to Tetschen, and thence westward by land to Töplitz. On the latter road, we have, on the northern side, first sandstone with basalt, afterwards gneiss, and then porphyry. In the same order as these changes occur in the solid rocks, so the boulders alternate in the earthy stratum of the tract to the southward, and are found at a considerable distance from their original situation; but, on the contrary, very near the basalt rocks which remain with southern sides. We thus see from this that the flood has here gone southward. From Töplitz I took the road to this place *viâ* Prague. As far as could be ascertained at a distance, none of the Bohemian volcanoes are of more recent date than the boulder-flood; nevertheless, it is uncertain whether this will be confirmed by a more minute investigation; for my own part, I will not absolutely affirm what I could not personally verify. Near Prague, westward of the road, we have Tablizerberg, which offered great facilities for making observations on the furrows. I remained in Prague, and made an excursion from thence to that mountain. It is admirably situated for such observations, being isolated on a large level plain. It bears undoubted traces, which are not to be mistaken, of the violent course of the boulder-flood from north to south; but the stratum, composed of siliceous slate, is in part so hard that it could not be furrowed by the boulders of gneiss, trap, sandstone, and clay-slate, passing over it, more especially as clay-slate forms the greatest part of these boulders; it is in part also fractured in such a manner, that one cannot immediately discern in which direction the resisting and lee-sides lie. The probability is, that the flood had from ten to fifteen degrees' variation to the south-west. Around Prague, as well as on the road to that place, we meet with boulders evidently of a northern origin. The most remarkable among them are a species which are met with between Tabor and Neuhaus, and which consist almost entirely of pure quartz, from the size of a grain of sand to that of a man's head. Every place is full of them, and this district is on both sides pretty clearly defined. The original situation of this quartz is in the northernmost point of Bohemia, around Friedland, where the high mountain Jeschen, south-west of Reichenberg, consists almost wholly of such quartz; and these boulders extend still further southward, east of Budweis, in the district of Böhmisch-Grazen, where several glass-houses are situated, on account of the easy access to this quartz.

Linz in Upper Austria, 26th June, 1836.

Since I last had the honour of addressing you, I have had an opportunity, between Vienna and Steyer, whilst on a journey to the mines in Lower Austria and Styria, of making some observations on the furrows and their course in the alpine districts of Austria, which I wish to have added to those which I before have had the honour to communicate on this subject.

As is well known, these Alps are composed principally of the peculiar kind of limestone, which at one time was called Alpine limestone, but has latterly begun to be considered as corresponding to the lowest part of the Jura limestone. From what I recollected of my former visit to these Alps, and my impression that they consisted of a collection of pyramidal mountain masses, with steeply inclined or often perpendicular precipices, I had little or no hope of finding furrows here. It is also known from geological works, that boulders do not occur here. But even at a short distance from Vienna, you find not only boulders, although of a smaller kind, but also that the mountains have a considerable resemblance to those of Scandinavia, in their northern ends being more sloped, whilst the southern are steeper. With respect to the subalpine mountains, which for a great part are composed of the rocks called by Keferstein *Flüsch*, they generally have a rounded form, but their detached rocks (sandstone and clay-slate) contain no traces of furrows. As to what further regards these limestone alps, I found, contrary to what I had thought I remembered, and directly the reverse of most of the drawings which represent them (and which artists have availed themselves of to give them a cragged appearance), viz. that the greater part of them on the top are crossed, and rounded on the upper surface, and that these rounded surfaces often extend to large table-lands, which are correctly given on the new Ordnance maps, whilst, on the contrary, they are not shown on any of the older maps*. These mountain-plateaus are, on the southern sides, often bounded by sharp peaks and perpendicular precipices, whence the fragments are evidently broken off. This is the case even on the northern sides, yet less frequently; and in general they are less steep, and the alps there more accessible, although the strata often run in quite a contrary direction, which is the case around Reifling in Ensdalen.

I was also told that it had been ascertained by the Archduke John, and his secretary M. Zahlbrüchner, that these plateaus, on their small rounded surfaces, are distinctly furrowed, often to the size of the wheel-tracks of a public road; but unfortunately the unsettled state of the weather and my ill-health, as well as the actual object of my journey, prevented me from making an excursion to such a plateau, which is seldom less than 5000 feet high, and the greater part of which was then covered with snow. Even at a height of 8000 feet such furrows occur, as has indeed been reported by engineer Schmuz of Steyer. I could not therefore come to any conclusion regarding the more accurate determination of this case; but believe the most paradoxical, viz. the peaks and precipices, even on the northern side, originated from the same cause as those at Halleberg and

* The reason why the Alps in landscape-drawings have always a craggy appearance, arises for the most part from the artists generally choosing a deep valley for their station; from which they cannot see the plateaus, but only their uppermost projecting points and peaks.

Hunneberg, and the Saxon Switzerland. It is even possible that these mountains may have been elevated since the formation of the furrows; but if such be the case, it will be ascertained by an accurate investigation, determining the direction of the furrows, which, in respect to the heights, must have the usual deviation from the normal direction, that is, if the mountain lies unmoved from its original site. It is very remarkable that the granite alps in Southern Styria have exactly the same rounded form as our Swedish mountains, rounded on the northern, and precipitous on the southern side; but my time was not sufficient to allow me to investigate them further. Thus the principal or most remarkable precipitous mountains appear in most places to have been formed in this way; the loose rocks (Flüsch) under the calcareous alps have been undermined, after which the overhanging harder rocks have fallen down. At other places, however, this is evidently not the cause.

Munich, 29th June, 1836.

Next to the elucidations for illustrating the petridelaunian phænomenon which are to be obtained from the observation of furrows in these districts, are undoubtedly the facts which are to be derived from investigations on the boulder-stones in different places, but which I have not been enabled to undertake. I have, however, seen thus much, that it is a complete error to suppose that boulder-stones do not occur in alpine districts, for they are found there in equally as large quantities as with us, nevertheless not of so large size, because the rocks which were rolled could be easier crushed; nor so conspicuous, because they are covered by a more luxuriant vegetation. With respect to the direction of the boulder-flood *from north to south*, or the reverse, viz. as is supposed to be the case in Western Switzerland and Jura, as regards the deviation of this flood to the east or west, the observations on the boulder-stones will give the most decisive results. To this point I have therefore directed my attention during my journey, but I am too little acquainted with the minerals which occur in the fixed rocks north and south, to say with certainty whence the boulders which I have seen on my way originated. The necessary information is not to be obtained in such cases from geognostical works and maps, owing to the many uncertain, and by different authors, differently-named minerals, of which they are constructed. From personal inspection of the formations that compose the fixed rocks in the north and south, good observations could easily be obtained; for when going along the Alps from east to west, or crossways, frequent alternations of well-characterized boulders are met with, which cannot possibly be mistaken. I found that sort of white quartz which occurs in Bohemia among the boulder-stones, to the south of the Danube in Ensdalen, and even close to Altenmark, as well as abundantly around Linz, where a smaller kind of these boulders formed at least a tenth part of the paving-stones of the streets; I did not see large boulders of this quartz. This was especially remarkable when I separated from M. Zahlbrüchner at Ens, where he took the road to the eastward and I to the westward. During my journey I had only district maps in separate sheets to make use of, which gave me the erroneous idea that Mölk and St. Pölten were situated to the south of Budweis in Bohemia, where I had found the quartz boulders mentioned in my former letter; I accordingly requested M. Zahlbrüchner to see if these were found at Mölk and St. Pölten. I was therefore very much surprised to find them at Linz, until I observed by means of a more general map, how this place was actually situated with regard to Budweis, from whence it lies to the southward, some degrees westerly.

They were not to be found some miles further westward; but between Salzburg and Munich, and northerly of the last-named place, quartz boulders are again met with. In general, the masses of detritus and boulders at the northern foot of the Alps were composed principally of primitive formations, and in particular near the great alpine rivers, such as the Salza, Inn, and Iser, where limestone boulders predominate.

Frankfurt on the Maine, 4th July, 1836.

On my journey from Munich to this place *viâ* Nüremberg, I have also seen very distinct alternations of different sorts of boulders, which to me appear so characteristic that no doubt can exist of their origin to one who is well acquainted with the formations of the German mountains. The great and frequently occurring alternations of soils of different origin are evidences of the same agency, and this is a fact which has been already long known. Between alluvium and diluvium a clear distinction ought however to be drawn. The plains at Munich and to the northward, for a distance of two miles (Swedish), are covered with a particularly barren mass of diluvial alpine gravel. At Eichstadt the alpine limestone appears in the solid rock above the loose Lias and Keuper formations. Even here the alpine limestone has also formed mountains similar to those of the Alps, with precipitous sides. The Spessart, which for the most part is composed of granite and gneiss, has in its external configuration such a similarity to those Scandinavian mountains which consist of the same hard materials as in Smaland, that it is particularly striking to the eye.

Cologne, 4th September.

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Since my last communication on the subject of the boulder-stones, I have passed over the Taunus, where I remained some time at the baths in Langenschwalbach, and have since been in England.

The Taunus, as is known, is composed principally of a mass of clay-slate, which is considerably elevated above the surrounding country. The upper surface is pretty even, but covered with small hillocks, which apparently were rounded by the boulder-flood. The higher peaks which remain, and which could not be worn away by the flood, are composed of harder substances—trap, schaalstein, harder kinds of clay-slate, and the like. The deep valleys, on the contrary, which are found in this tract, and which have very precipitous sides, do not seem to have been caused by the flood, but appear in most places to be of a more recent origin.

A circumstance which had drawn my attention during my journey from Munich was here explained. On this road, particularly around Nüremberg, there occur in the superficial detritus (dam-jord), which otherwise is composed of clay mixed with sand, large and small grains of a snow-white quartz of such a singular appearance that it could not be mistaken for any other sort of quartz. I was, therefore, very curious to discover where this quartz occurs *in situ*, particularly since, to my knowledge, it does not belong to the minerals which are found northward. Contrary to all expectation, however, it was found in the clay-slate formation of which the Taunus is composed, and which stretches easterly towards Cassel, as well as through the whole of the southern part of the Hartz. It is found there in veins, often of such considerable dimensions that they are worked to procure materials for road-making. When the slate was ground by the boulder-flood to a fine powder, the quartz remained in this loose mass as gravel. The greater part of these grains are, however, rounded; but they are recognised by their peculiar white colour, often passing into yellow, their drusy cavities, and the thin laminæ of slate frequently contained in them.

In England my stay was not long, and I made a tour only to Birmingham, South Wales, and by way of Bristol back again to London. Here also are seen distinct traces of the boulder-flood, particularly around Bristol; but I saw no furrows. The loose rocks which occurred, afforded no guide for a more exact determination of the direction of the flood.

Ankarsrum, near Westervik, 14th October, 1836.

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From Cologne I took the road to Berlin, across the Hartz, with the view of there finding traces of furrows; but in vain, partly from the rocks being of such a nature as not to receive a polish, partly from the furrows having been obliterated by the action of the atmosphere, which, for example, is the case with the shell-limestone. There is, however, no doubt that the boulder-flood has even gone over the Hartz. Hofrath Professor Hausmann, with whom I conversed in Göttingen on this subject, said that he himself was already convinced of it, although he allowed himself to be silenced, as many geologists are of a contrary opinion. Among other things he adduced in proof of this, the rents which occur in the shell-limestone (snäck-kalken), and which many geologists consider to have been caused by a certain portion having been lifted up by a subterranean power when the rents were formed; but which, according to M. Hausmann's observations, is not the case; for when the causes are attentively traced, it is seen that a flood having carried away the soft marl which underlies the muschelkalk along the northern side of that formation, the overhanging edges of the limestone fell by their own weight, and the crevices in the muschelkalk have thus originated.

At Berlin, Professor G. Rose informed me, that during the summer he had visited the limestone quarries of Rüdersdorf, east of the city, and that the manager there had remarked as highly singular, that, upon removing the earth, which was done last spring, in order to get at a new opening in the limestone quarries, he had found the upper surface of the limestone beds under the marshy earth worn away or polished, of which evident traces appeared. Professor Rose had endeavoured to ascertain in what direction these worn surfaces ran; but they were then broken up, and no one, whilst they were visible, had paid any particular attention to them. In the mean time, this is a proof that furrows are found even in Germany. Above Pirna I saw something similar on a hard sandstone; but as such markings are not evident to an unpractised eye, I could not succeed in drawing attention to them. Besides, their situation was not good for determining their direction.

In Berlin I had an opportunity of inspecting several English charts, too expensive for me to purchase. The most explanatory I procured, however, by purchase. On one of these, viz. *Purdy's General Chart of the Atlantic Ocean*, London, 1816, we see, without possibility of mistake, that the sand-banks in the North Sea and Skager-rack lie to leeward of the southern coast of Norway. Further westward, or between Norway and Scotland, the sea is deep; and this deep channel further extends itself as far as Hull, on the English coast. On the southern coast of Norway there is a deep channel, which was either excavated by the torrents of water flowing from the Christiania fiord, or else from the cataract which has rushed over the southern mountain heights of Norway.

The sand-banks lie in the same manner south of England and Ireland, as well as south of the north-western cape of France, or south of Brittany. The northern coast of Spain, on the contrary, is perfectly scoured, so that in almost every instance the rocks are bare; just as on the northern coasts of the Shetland and Orkney Islands, and the northernmost coast of Scotland.

In the lee of the Portuguese coast sandy ground is found, but no proper sand-banks before passing Cape St. Mary, where one is met with to the leeward of the south-western coast of Andalusia.

The north-western coast of Africa has no considerable sand-banks, but at Cape Blanco, where that coast takes a perfect southerly direction, a sand-bank has been formed; and from Cape de Verd, where the coast trends to the south-east, it extends to the breadth of several geographical miles. Along the coast of Guinea the banks are very inconsiderable, but again make their appearance outside the Cape of Good Hope, where the well-known far-extended Needle Banks are situated. To judge from the description and drawings, the Table Mountain at the Cape of Good Hope is similarly circumstanced with the sandstone rocks in the Saxon Switzerland. From this it is probable, that the boulder-flood which passed over Scandinavia, Germany and England in a southern direction, has also proceeded over the south of Europe and Africa in the same direction, viz. south-west.

How it afterwards took its course we cannot determine; but from the information I can obtain relative to the navigation around Van Diemen's Land and New Holland, and the other Australian islands, the southern coasts there are rocky, the northern, on the contrary, surrounded by sand-banks; so that the flood very probably flowed there towards the north. To judge from two charts published by Horsburgh of the navigation around Sumatra and Java, where rocks encompass the southern coasts and sand-banks the northern, one would also be led to suppose that the flood had here also gone to the north. If this should be confirmed by observations made on the spot, the flood should have gone towards the north over the eastern part of Asia, towards the north-west over Samoida, to the west of north over Nova Zembla, and to the south over Greenland. From this may possibly be explained the appearance of skeletons of elephants and other animals in the north of Siberia, as well as in other countries in whose climates those animals scarcely could live. But hypotheses are not explanations, and therefore I here break off the detail of what I might have gathered from such an uncertain source.

Fahlun, 2nd November, 1836.

Since my return I have had an opportunity of arranging the observations on the furrows, which were partly made by my friends during my absence, and partly by myself, whilst on my way home. The principal contributor has been Mr. H. Wegelin, who visited the places in Nerike and West Göthland, where I had before been, and investigated the whole of the western coast, and the coast of Sconia, where observations were wanting. Without having any knowledge of the results which I had obtained at the first-named places, his observations, to my great satisfaction, corroborate what I had myself ascertained.

The results communicated by him are shown in the following Table:—

Furrow observations by Mr. Wegelin, 1836.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
a. On a journey from Eskilstuna to Götheborg and Sconia:				
Between Hallsta and Nästorp ...	0° to 10°	16° 0′	16° to 26°	E.
" Juleta and Forsby	0 to 10	16 15	16¼ to 26¼	E.
" Sörby and Blacksta on the boundary of transition limestone, indistinct	330	16 45	13¾	W.
Near the house at Pasa, between Bodarne and Hofva (on the northern end of Tiveden)	350	17 15	7¼	E.
Hummeltorp, nearer Hofva	345 to 350	17 15	2¼	E.
Half a mile (Swedish) south of Hofva	347	17 30	4½	E.
Between Mariestad and Björsäter, quarter mile (Swed.) from Mariestad	335	17 45	7¼	W.
Between Björsäter and Forshem, on granite	330	18 0	12	W.
At Forshem	310 to 320	18 0	32 to 22	W.
On Kinnekulle, the trap .... 310° plainest	310 to 350	18 0	32 W. to 8	E.
Sandstone south of west, indistinct surfaces	330	18 0	12	W.
At Tholsjö, on the road to Kalland's island	335	18 15	6¾	W.
Near the inn at Ekebo, near Läckö	310 to 320	18 15	31¾ to 21¾	W.
Between Tholsjö and Orslösa ...	310	18 30	31½	W.
At Orslösa on trap and veins of red granite	300	18 30	41½	W.
West of the house at Särsta, and other places, plain	295	18 30	46½	W.
Nearer Grästorp, plain	290	18 30	51½	W.
At Grästorp, less plain	300	18 30	41½	W.
From thence over Halleberg (plain), to Wenersborg (indistinct)............................	300	18 45	41¼	W.
North of Holms inn (a high situation)	320	19 0	21	W.
At Allera, quarter mile (Swed.) north of Bäck in a valley	320	19 0	21	W.
Top of Grindsvall (mountain) in Rommeland district (a well-situated place)	280 to 285	19 0	61 to 56	W.
At Lindomma church, plain......	300	19 15	40¾	W.
South of Kungsbacka	285	19 15	55¼	W.
At Ormevalla church.............	285	19 0	56	W.
North of Warberg..................	290	19 0	51	W.
Skaudden in Sconia, south of Torekow fishing-place (on trap, very distinct)....................	315	18 30	26½	W.
At the village of Tamarps, three-quarters of a mile (Swed.) west of Cimbritshamn, on orthoceratic limestone	340	17 30	2½	W.
At Cimbritshamn (sandsto. plain)	290	17 30	52½	W.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
A trap hillock, quarter mile (Swe.) south of Cimbritshamn	290°	17° 30′	52½°	W.
b. On a journey from Cimbritshamn to Mariefred by Mälaren:				
Near Broby inn, north of Christianstad	320	17 30	22½	W.
One-eighth of a mile (Swed.) south of Omberg	305	17 15	37¾	W.
At Hällesta inn (plain)	355	16 30	11½	E.
At Bratteberg and Regnaholm ...	355	16 30	11½	E.
At Tisnehult	10	16 30	26½	E.
At Stensjö inn	15	16 15	31¼	E.
Yxstaholm, north of Flens inn (not plain)	0	16 0	16	E.
Half mile (Swed.) south of Wadsbro inn, near Malmkoping......	0	15 45	15¾	E.
Between Byringe and Lägsta, by Berga	0	15 30	15½	E.
c. North of Westeras on the road to Avestad:				
At Salbo, not very distinct	355	16 0	11	E.

Sefström's Observations, 1836.

Between Linköping and Christianstad.				
By Kumla inn	16	16 45	32¾	E.
Between Säthälla and Bona	347	17 0	4	E.
Near Komsta	350	17 15	7¼	E.
Between Bo and Matkull, indistinct	357	17 15	14¼	E.
Near the inn at Marklunda in Sconia	330	17 30	12½	W.
Between Wexiö and Wimmerby.				
At Bergqvara, westward of Wexiö	5	17 0	22	E.
Half a mile (Swed.) east of Areda	0	17 0	17	E.
Half a mile (Swed.) north of Serap	6	16 45	24¾	E.
Three-quarters of a mile (Swed.) north of Molilla	15	16 30	31½	E.

Mining-Sheriff C. B. Fittinghoff's Observations, 1836.

In Dalecarlia.				
By Säfnäs church in West Bergslagen, on the north side of the height	352	17 15	8¾	E.
At Grangesberget (at Ormberget the compass was evidently attracted by the large masses of iron ore)	340	17	3	W.
In Nerike.				
Half a mile (Swed.) north of the New Kopparberg's (copper mountain) church in the northern end of the Fingrufve height	350	17	7	E.

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
Quarter of a mile south of the church on the road to Linde, on the top of a hill...............	351°	17° 0′	8°	E.
Quarter of a mile south of Stjernfor's works.................... ...	352	17 0	8	E.
South-west of Nora Stad, west of Elfhyttan on the height between Svartelfs and Nora-vattendragen, on the so-called Vargasarne........................	349¼	17 15	6¾	E.
In Wermland.				
a. Northward from Philipstad:				
To the north-west, at Igel heights in the parish of Gasborn, south of Säfnäs church	350	17 15	7¼	E.
North-west of Philipstad, at Bosjö and Skaltjern, on the heights between Fernebo and Brattfors parishes	347	17 30	4½	E.
Near this place on the height of the Kongberg..............	347	17 30	4½	E.
Further north in the parish of Ekhärad, near the inn at North Skoga, on the western side of Clarelfven (The clear river)...	334	18 0	8	W.
b. On a journey from Philipstad, north-west to the Norwegian boundary:				
Half a mile (Swed.) west of Nyed church or Molkolms inn at Solberg, on the north-eastern side of a height	341	18 0	1	W.
Farther north on the western side of Clarelfven, in the parish of Upper Ullerud, near Olsäters inn	340	18 0	2	W.
Still further north, by Norsjö, on the height between Clarelfven's valley and Lake Wisten.........	332	18 0	10	W.
In the parish of Emtervik at Högberg's inn, on the hill on the eastern side of Mellanfryken	337	18 15	4¾	W.
At Björke works in the parish of Sunne, on the eastern side of the southern end of North Fryken	330	18 15	11¾	W.
On the same side, further north, by Lysvik	332	18 30	9½	W.
In the parish of Ostmark on the Norwegian boundary; on the Klackberg, by Längerud's house	5	18 45	23¾	E.
N.B. This last observation has given an unexpected result, when one compares this with all the observations easterly, as well as M. Boström's observations at				

	Direction of the furrows by the compass.	Magnetic variation.	Furrows diverge from the meridian.	Direction.
Strömstad and *Christiania*; but M. Fittinghoff assures me that there was no indication of the compass having varied in the Klackberg, for it gave similar results on several points of observation; on the contrary, it appears that the direction of the furrows had received an easterly course, through the mountains which occur in the neighbourhood to the west and south-west.	° °	° ′	° °	

Furrow Observations by MM. Hiriakoff and Rachette*, 1836.

On a journey from Stockholm to Abo and St. Petersburg.				
Furusund (in the Swedish Skärgarden†)........................	324	14 20	21⅔	W.
Near Abo	16	12 0	28	E.
One mile (Swed.) east of Abo...	17	12 0	29	E.
Uskila church, near Salo inn......	25	11 30	76½	E.
One mile from Lambola	35	11 30	46½	E.
At Svenskby inn	24	11 15	35¼	E.
Between Fiskar works and Kerkäla..............................	24	11 0	35	E.
Between Kerkäla and Orrejervi...	32	10 45	42¾	E.
Between Kyrksta and Bolsta......	25	10 30	35½	E.
Between Bolsta and Helsingfors	20	10 30	30½	E.
At Helsingfors	15	10 15	25¼	E.
Half a mile (Swed.) from Lovisa	10	9 20	19⅓	E.
At Fredrikshamn	4	8 45	12¾	E.

N.B. Judging from the principal direction of the furrows in the middle and south of *Sweden*, one would suppose that those in Finland would also have a somewhat westerly course; but that this is not the case is shown in the above observations, which may be depended on: these show that the furrows go in the same direction as the watercourses in Finland, both those which fall into the *Gulf of Finland* and those which fall into the *Gulf of Bothnia*, which have all the same surprising parallelism remarked before by many authors; this even extends itself to the watercourses in the northern provinces of Sweden. What has given rise to this course, it is impossible now to determine; for an accurate knowledge of the actual directions of the mountains with their branches, in the north of *Scandinavia* and *Finland*, is yet wanting.

* MM. Hiriakoff and Rachette have, with His Majesty's consent, been sent by the Russian government to the Fahlun Mining-School, and, while attending the courses of lectures, have taken part in the investigations respecting the furrows, and which, at my request, they will continue in such countries as they may visit.

† Skärgarden signifies an immense number of small islands and barren rocks, so prevalent in the inlets and on the coasts of Norway and Sweden. They are generally uninhabited; the depth of water between them is sufficient for vessels of any tonnage to pass.

NOTE.

Note.

[Whatever opinion may be entertained with regard to the reasonings of Mr. Sefström, and the hypothesis which he proposes, it has been thought that the detailed observations contained in his memoir would make a translation of it acceptable and useful, with a view to further investigations which may be in progress for the elucidation of the subject of the mountain-furrows.

Notices of polished and furrow rocks at Miami, in North America, and in the forest of Fontainebleau, have lately appeared in Silliman's Journal for January 1841, and in the *Comptes Rendus*, t. xiii. p. 69.—Ed.]

Article VI.

On a Method of Facilitating the Observations of Deflection. By Carl Friedrich Gauss.

[From the *Resultate aus den Beobachtungen des Magnetischen Vereins.* 1839.]

1. If, in addition to the magnetic force of the earth, another force be brought to act on a magnetometer statically, but in a direction inclined to the magnetic meridian, the needle will take up a new position of equilibrium, and the magnitude of the deflection may serve as a measure of the superadded force. The determination of the amount of deflection requires not only that the new position of equilibrium should lie within the limits of the scale, but, inasmuch as it is not desirable to wait until the bar is at perfect rest, the extent of its remaining vibration ought not to exceed these limits. Supposing the bar to have been stationary whilst under the influence of the earth's magnetic force alone, and the additional force to be brought at once into full action,—an oscillation will commence, having for its middle point the new position of equilibrium, and for its two extremities, the previous position on the one side, and a point equidistant from the middle of the vibration on the other. If the new position of equilibrium should be but little within the scale, it is clear that, without having recourse to some artifice, the slow decrease of the arc of vibration would make it necessary to wait a considerable time before that position could be determined. On account of the hourly change of the declination, this delay would in all cases detract from the trustworthiness and usefulness of the determination, and would vitiate it almost entirely in cases where the superadded force is liable to considerable alteration in short periods,—as in the case of galvanic currents.

2. This inconvenience may be remedied by the following simple method. Let the additional force be at first brought into action only during the third part of the time of one vibration; let it then be suspended for an equal time, and after a similar interval brought into continued action. For example, supposing the time of vibration of the magnet to be 30 seconds, and that it is desired to measure the deflection produced by a

galvanic current: let the circuit be closed at a second which is to be reckoned as 0^s, reopened at 10^s, and definitively closed at 20^s. If we desire to measure the deflection produced by a magnetic bar in a particular position, this position must be beforehand exactly and conveniently marked, and the bar being held vertically as the observer approaches, must be laid down in its place at the instant 0^s,—taken up again at 10^s, and replaced definitively at 20^s. The result will be, that during the first 10 seconds the magnetometer will move with an accelerated velocity from its original position towards that which corresponds to the deflection; at the tenth second it will have reached a point precisely midway between the two positions, and during the second 10 seconds it will pass through the remaining half of the interval with a retarded movement, so that at the 20th second it will have taken up its new position, and all motion will have ceased.

It is easy to perceive how the needle may be brought in the same way from one deflected position of rest to an opposite position, in which it shall also be at rest; namely, by causing the deflecting force first to act for the third part of a vibration in the opposite direction, then during an equal time again in the original direction, and then changing again. In galvanic currents the alternations may be made almost instantaneously by the aid of a suitable commutator;—with deflecting bars the effect may be produced by a rapid half-revolution (most conveniently, a horizontal half-revolution), bringing the north pole of the bar into the place of the south pole. It is equally evident, that after observing the deflection, the needle may at once be brought to rest in the magnetic meridian, by first suspending the action of the deflecting force for the third part of one vibration, then renewing it for a similar interval, and then finally causing it to cease.

3. The method described rests on the two following suppositions:—

First, that in the oscillations of the needle, the distance from the middle point of the vibration (so long as that point is itself stationary) is proportional to the sine of an angle which increases uniformly, and is augmented by 180° during the time of one vibration.

Second, that the time of vibration is not altered by the additional force.

Inasmuch as these suppositions are not rigorously fulfilled, and as, moreover, in practice the alternations can neither be effected instantaneously, or precisely at the prescribed intervals, it will seldom happen that the needle will be perfectly at rest after the operation is completed; however, the object will be sufficiently attained, if the remaining movement be so small that the observation of the true position of equilibrium can be at once begun in the usual manner.

Practically, therefore, the above-mentioned suppositions will deviate but very little from the truth. The applicability of the magnetometer implies that the additional force shall produce only a moderate deflection, in which case (with an exception to be hereafter noticed) the law contained in the first supposition holds good with sufficient exactness. The alteration of the time of vibration by the deflecting force is quite insensible when that force acts perpendicularly to the magnetic meridian, as is almost always the case; but even if it were to act obliquely, inasmuch as it is but a small fraction of the magnetic force of the earth, its effect in altering the time of vibration during the short time occupied by the operation, would still be quite immaterial.

There is only one case in which an exception is to be made, namely, when the bar oscillates under the influence of a damper, by which the arc of vibration is considerably lessened,—in such case the above law no longer holds good, and the object would not be attained by the method described. On the other hand, the inconvenience mentioned in Art. 1 is much less, as a powerful damper does of itself bring the bar to rest within a moderate time. As, however, in this case a modification only of the described method is required to obtain the same result, and as it must always be desirable to avoid all unnecessary loss of time, it is worth while in practice, as well as in theory, to consider the question in all its generality.

4. We have first to solve the following general problems.

A magnetic bar oscillates under the influence of repeated alterations of the acting forces, the time of vibration and the logarithmic decrement* remaining, however, unaltered, and the arc of vibration continuing sufficiently small to allow of quantities of the third order being neglected. We are required to deduce, from the circumstances of the initial movement, those which take place after the last change.

Let T be the time of vibration, ε the logarithmic decrement,

* *Resultate*, 1837, p. 68.

e the base of the hyperbolic, and m the modulus of Briggs's logarithms, π the proportion of the circumference to the diameter. Let

$$n = \frac{\pi}{T}, \quad \varepsilon = \frac{\lambda}{m\,T}.$$

Then the position x, for the time t, will be expressed by the formula

$$x = p + A\,e^{-\varepsilon t} \sin(n\,t - B),$$

to which we may also give the form

$$x = p + a\,e^{-\varepsilon t} \cos n\,t + b\,e^{-\varepsilon t} \sin n\,t,$$

where p expresses the position of equilibrium, and the coefficients a, b, continue constant as long as p is the same. The velocity of the movement is hence found

$$\frac{d\,x}{d\,t} = -e^{-\varepsilon t}\,(n\,a \sin n\,t + \varepsilon\,a \cos n\,t - n\,b \cos n\,t + \varepsilon\,b \sin n\,t);$$

or, if we introduce an auxiliary angle ϕ, so that $\frac{\varepsilon}{n} = \text{tang}\,\phi$,

$$\frac{d\,x}{d\,t} = -\frac{n\,e^{-\varepsilon t}}{\cos\phi}(a \sin(n\,t + \phi) - b \cos(n\,t + \phi)).$$

For $a\,e^{-\varepsilon t} \cos n\,t + b\,e^{-\varepsilon t} \sin n\,t$ we may write u, so that $x = p + u$.

Now let t', t'', t''', be the particular values of t, in which an alteration of the acting force has been made; further, let the particular values of p, a, b, in the different portions of time be the following:—

p^0, a^0, b^0 before t'	p'', a'', b'' from t'' to t'''
p', a', b' from t' to t''	p''', a''', b''' after t'''.

Lastly, let the general expression of u become u^0, u', u'', u''', when the particular values are substituted for a and b, so that before the first change $x = p^0 + u^0$, from thence to the second change $x = p' + u'$, and so on.

As the instant t' is at once the last of the first interval of time, and the first of the succeeding interval, x as well as $\frac{d\,x}{d\,t}$ must preserve the same value for $t = t'$; and, in the above general expression, the values p^0, a^0, b^0, or p', a', b', may be substituted for p, a, b. Thus

$$0 = p' - p^0 + (a' - a^0)\,e^{-\varepsilon t'} \cos n\,t' + (b' - b^0)\,e^{-\varepsilon t'} \sin n\,t',$$

$$0 = (a' - a^0) \sin(n\,t' + \phi) - (b' - b^0) \cos(n\,t' + \phi),$$

whence it is easy to deduce

$$a' - a^0 = -\frac{p' - p^0}{\cos\phi} e^{\epsilon t'} \cos(n t' + \phi),$$

$$b' - b^0 = -\frac{p' - p^0}{\cos\phi} e^{\epsilon t'} \sin(n t' + \phi),$$

and hence

$$u' = u^0 - \frac{p' - p^0}{\cos\phi} e^{-\epsilon(t - t')} \cos(n(t - t') - \phi).$$

In like manner we obtain

$$u'' = u' - \frac{p'' - p'}{\cos\phi} e^{-\epsilon(t - t'')} \cos(n(t - t'') - \phi),$$

$$u''' = u'' - \frac{p''' - p''}{\cos\phi} e^{-\epsilon(t - t''')} \cos(n(t - t''') - \phi),$$

and so on if there are more alterations of the moving force. Thus, from the initial movement every succeeding movement may be determined.

5. For the case of the present investigation we should write $p'' = p^0$ and $p''' = p'$. Thence

$$u''' = u^0 - \frac{p' - p^0}{\cos\phi} e^{-\epsilon t'} [e^{\epsilon t} \cos(n(t - t') - \phi)$$
$$- e^{\epsilon t''} \cos(n(t - t'') - \phi) + e^{\epsilon t'''} \cos(n(t - t''') - \phi)],$$

which formula, if we make

$$e^{-\epsilon(t'' - t')} \cos n(t'' - t') - 1 + e^{\epsilon(t''' - t'')} \cos n(t''' - t'') = f,$$
$$e^{-\epsilon(t'' - t')} \sin n(t'' - t') - e^{\epsilon(t''' - t'')} \sin n(t''' - t'') = g,$$

passes into

$$u''' = u^0 - \frac{p' - p^0}{\cos\phi} e^{-\epsilon(t - t'')} [f \cos(n(t - t'') - \phi)$$
$$- g \sin(n(t - t'') - \phi].$$

Hence it follows, that if the intervals $t'' - t'$, $t''' - t''$ are so determined that $f = 0$, and $g = 0$, then generally

$$u''' = u',$$
$$\text{or} \quad a''' = a^0,\ b''' = b^0.$$

Thus, if before the change the needle was in repose in p^0, after the change it will be in repose in p': in the opposite case the needle, after the three changes, will have at each instant precisely the same velocity and the same position relatively to the middle point of its motion p', which it would have had at the same instant relatively to p^0, if it had continued its original movement undisturbed; in a word, the middle point of the movement alone is changed; the movement itself is wholly unaltered.

6. We have still to determine the intervals so that the equations $f = 0$, $g = 0$ may be satisfied. If we make

$$t'' - t' = q\,\mathrm{T}, \quad t''' - t'' = r\,\mathrm{T},$$

and bear in mind that $e = 10^m$, these equations become

$$10^{-q\lambda} \cos q\pi + 10^{r\lambda} \cos r\pi = 1$$
$$10^{-q\lambda} \sin q\pi = 10^{r\lambda} \sin r\pi.$$

Thus, for the case of an insensible decrease of the arc of vibration, $\cos q\pi + \cos r\pi = 1$, and $\sin q\pi = \sin r\pi$, and $q\pi = r\pi = 60^0$, or $\frac{1}{3}\pi$, and $t'' - t' = t''' - t'' = \frac{1}{3}\mathrm{T}$, as has been already shown in Art. 2. For the case of a sensible logarithmic decrement, on the other hand, these equations are to be solved indirectly, to which calculation the following form may be given.

From the combination of the equations it follows that

$$\operatorname{tang} r\pi = \frac{\sin q\pi}{10^{q\lambda} - \cos q\pi}$$

$$10^{2r\lambda} = 1 - 2\,.\,10^{-q\lambda} \cos q\pi + 10^{-2q\lambda}.$$

Eliminating r we obtain the equation with one unknown quantity,

$$\frac{\pi}{2\lambda} \log\left(1 - 2\,.\,10^{-q\lambda} \cos q\pi + 10^{-2q\lambda}\right)$$
$$= \operatorname{arc}\left(\operatorname{tang} = \frac{\sin q\pi}{10^{q\lambda} - \cos q\pi}\right)$$

in Briggs's logarithms. When this has been satisfied, it is manifest that the value of r will have been obtained at the same time.

7. In order that those who desire to make use of the method here described, when a damper is employed, may be spared the calculation which has been explained in the preceding article, I subjoin a table, from which the proportion of the two intervals of time to the time of vibration can be taken out at sight for every logarithmic decrement. It will be seen that as the logarithmic decrement increases, the first interval always increases, and the second decreases. Their sum is exactly equal to two-thirds of the time of vibration when $\lambda = 0$; but it changes from this much more slowly. In making use of the table, it need

hardly be remarked that it is sufficient to take into account the first decimals of the values of q and r.

TABLE.

λ.	q.	r.	λ.	q.	r.
0	0·33333	0·33333	0·30	0·45921	0·21406
0·01	0·33757	0·32911	0·31	0·46322	0·21048
0·02	0·34181	0·32489	0·32	0·46721	0·20694
0·03	0·34606	0·32068	0·33	0·47118	0·20343
0·04	0·35031	0·31648	0·34	0·47513	0·19996
0·05	0·35456	0·31229	0·35	0·47906	0·19652
0·06	0·35882	0·30812	0·36	0·48297	0·19311
0·07	0·36308	0·30395	0·37	0·48685	0·18975
0·08	0·36734	0·29981	0·38	0·49071	0·18641
0·09	0·37160	0·29568	0·39	0·49454	0·18311
0·10	0·37585	0·29156	0·40	0·49835	0·17985
0·11	0·38011	0·28746	0·41	0·50214	0·17663
0·12	0·38436	0·28338	0·42	0·50590	0·17344
0·13	0·38861	0·27932	0·43	0·50963	0·17029
0·14	0·39285	0·27528	0·44	0·51334	0·16718
0·15	0·39708	0·27126	0·45	0·51702	0·16411
0·16	0·40131	0·26727	0·46	0·52067	0·16107
0·17	0·40552	0·26329	0·47	0·52430	0·15808
0·18	0·40973	0·25934	0·48	0·52790	0·15512
0·19	0·41393	0·25542	0·49	0·53147	0·15220
0·20	0·41892	0·25152	0·50	0·53501	0·14931
0·21	0·42230	0·24764	0·51	0.53852	0·14647
0·22	0·42646	0·24379	0·52	0·54201	0·14367
0·23	0·43061	0·23997	0·53	0·54546	0·14090
0·24	0·43474	0·23618	0·54	0·54889	0·13817
0·25	0·43886	0·23242	0·55	0·55229	0·13548
0·26	0·44297	0·22868	0·56	0·55566	0·13283
0·27	0·44705	0·22498	0·57	0·55900	0·13022
0·28	0·45112	0·22131	0·58	0·56231	0·12765
0·29	0·45517	0·21767	0·59	0·56559	0·12511
0·30	0·45921	0·21406	0·60	0·56884	0·12261

8. The fundamental assumption in our theory, *i. e.* that the three changes take place instantaneously, is never rigorously fulfilled in practice, although in the case of deflections by galvanic currents the time required for each change may be regarded as insensible. But in the case of deflections by magnetic bars, the time is always more or less considerable, in proportion to their size and weight. With bars of 25 lbs. some seconds are always required to complete the change; especially when it is a complete inversion, and not merely a change from a

vertical to a horizontal position, or *vice versâ*; and for this case, which is far the most important and most frequent in practice, it is easy to execute the operation in such manner that the result may be scarcely sensibly disturbed. It is only necessary to take care that the second and third change take place in the same manner as the first, so that they may occupy an equal interval of time; and to deduct this from the intervals which would be otherwise required. If, for example, the logarithmic decrement is 0·33570, and the time of vibration $21^s{\cdot}21439$, the table shows that the first interval $= 10^s{\cdot}04$, and the second $= 4^s{\cdot}27$. If it be found that three seconds are requisite for completing a change, *begin* the first change at 0^s; from 3^s to 10^s the bar remains in the new position; the second change commencing at 10^s brings the bar at 13^s back to its first position, in which it remains only $1\frac{1}{4}$ second, when the third change begins, so that $17\frac{1}{4}$ seconds are required to complete the whole operation. A more extended investigation, too long for insertion in this place, shows that if p^0 passes into p', not *per saltum*, but gradually,—and likewise p' into p^0, and p^0 into p'—in the second and third changes the result continues the same as is given at the conclusion of Art. 5, provided only that the three times of passage be of equal length,—that the three passages themselves proceed by a similar gradation,—and that the calculated intervals q T, r T apply to the commencing moments of change; or, which comes to the same thing, that the two first times of passage be included in the calculation.

GAUSS.

SCIENTIFIC MEMOIRS.

VOL. III.—PART X.

ARTICLE VII.

General Propositions relating to Attractive and Repulsive Forces acting in the inverse ratio of the square of the distance. By C. F. GAUSS.

[From the *Resultate aus den Beobachtungen des magnetischen Vereins im Jahre* 1839. Leipsic, 1840.]

1.

NATURE presents to us many phænomena which we explain by the assumption of forces exerted by the ultimate particles of substances upon each other, acting in inverse proportion to the squares of their distance apart.

Amongst these forces, the first to be noticed is that of universal gravitation, by virtue of which every material molecule μ exercises upon every other such molecule $\mu_{\prime}$ a moving force, which, if we call the distance r, is expressed by $\frac{\mu\mu_{\prime}}{r^2}$, and tends to produce the approximation of the molecules in the direction of the straight line connecting them.

If, in order to explain magnetic phænomena, we assume two magnetic fluids, one positive and the other negative, two magnetic elements μ, $\mu_{\prime}$, will exert, each on the other, a moving force $\frac{\mu\mu_{\prime}}{r^2}$, acting along the straight line which joins the two elements, repulsively if μ and $\mu_{\prime}$ are of the same kind of fluid, attractively if they are of different kinds.

The same is true of the mutual action of the particles of electric fluids upon each other. The linear element ds of a galvanic current exerts in like manner on an element of the magnetic fluid μ a moving force, which is inversely proportional to the square of the distance r; but there is now introduced a new and distinctive circumstance; the direction of the force is not in the connecting straight line, but is perpendicular to the plane passing through μ and the direction of ds; and the intensity of the force depends not on the distance alone, but also on the angle which r makes with ds. If this angle be called θ, then $\frac{\sin\theta \,.\, \mu\, ds}{r^2}$ is the measure of the moving force which ds exerts upon μ; and an equal force in the parallel and opposite direction is exerted by μ on the element of the current ds, or on its material conductor.

If we assume with Ampère that the elements ds, ds' of two galvanic currents act attractively or repulsively on each other in the straight line which joins them, then the phænomena require us to consider this force as acting in the inverse ratio of the square of the distance; but as having, at the same time, a somewhat less simple dependence on the direction of the elements of the currents. We shall restrict ourselves in this treatise to the three first cases, or to those forces which are exerted by one element upon another in the straight line which joins them, and which are therefore simply in the inverse ratio of the square of the distance; although several propositions will be found applicable, with slight alterations, to the other cases also, the more detailed development of which must be reserved for another treatise.

2.

We denote by a, b, c the rectangular coordinates of a material point, from which an attracting or repelling force emanates; and we denote its accelerating force, at an indeterminate point O, the coordinates of which are x, y, z, by

$$\frac{\mu}{(a-x)^2+(b-y)^2+(c-z)^2}=\frac{\mu}{r^2},$$

where μ expresses for the first case mentioned in the preceding article, the quantity of matter existing at the first point; and for the second and third cases, the quantity of magnetic or electric fluid. Resolving this force in directions parallel to the three coordinate axes, we have its components

$$\frac{\varepsilon\mu\,(a-x)}{r^3},\quad \frac{\varepsilon\mu\,(b-y)}{r^3},\quad \frac{\varepsilon\mu\,(c-z)}{r^3},$$

where $\varepsilon = +1$, or $= -1$, according as the force is attractive or repulsive, which is always decided by the qualities of the agent and the recipient. These components are represented by the partial differential coefficients

$$\frac{d\frac{\epsilon\mu}{r}}{dx}, \quad \frac{d\frac{\epsilon\mu}{r}}{dy}, \quad \frac{d\frac{\epsilon\mu}{r}}{dz}.$$

If therefore several agents μ^0, μ', μ'', &c. act on the same point O, from the distances r^0, r', r'', &c., and if we write

$$\frac{\mu^0}{r^0} + \frac{\mu'}{r'} + \frac{\mu''}{r''} + \text{\&c.} = \Sigma\frac{\mu}{r} = V,$$

then the components of the whole force acting at O will be represented by

$$\frac{\epsilon\, dV}{dx}, \quad \frac{\epsilon\, dV}{dy}, \quad \frac{\epsilon\, dV}{dz}.$$

If the agents do not act from discontinuous points, but from a continuous line, surface, or solid, then, instead of the summation Σ, we have a single, a double, or a triple integration. The last case is the only one which exists in nature; but as it is often possible, under certain limitations, to substitute forces imagined to be concentrated in points continuously distributed on lines or surfaces, we will include these cases in our investigation, and we shall not scruple to speak of *masses* distributed over a surface, or upon a line, or concentrated in a point; inasmuch as the expression "mass" will here mean merely the source from which the attracting or repelling forces are imagined to proceed.

3.

If, then, we denote by x, y, z the rectangular coordinates of any point in space, and by V the sum of all the active particles of the mass, divided respectively by their distances from that point, so that, according to the condition of the investigation on each occasion, the negative particles may be either excluded or suitably treated, V becomes a function of x, y, z; and the investigation of the properties of this function will be itself the key to the theory of the attracting or repelling forces. For convenience I will give to the function V a special denomination, calling it the *potential* of the masses to which it relates. This restricted conception of the potential suffices for our present investigation; in the case of other laws of attraction, than those which act in the inverse ratio of the square of the distance, or for that of the fourth case mentioned in Art. 1, we may, in a wider sense, understand by the potential, that function of x, y, z of which the partial differential coefficients represent the components of the resultant force.

If we denote by p the whole force exerted at the point x, y, z, and by α, β, γ the angles which the direction of the force makes with the three coordinate axes, then the three components are

$$p \cos \alpha = \epsilon \frac{d\,V}{d\,x}, \quad p \cos \beta = \epsilon \frac{d\,V}{d\,y}, \quad p \cos \gamma = \epsilon \frac{d\,V}{d\,z},$$

and

$$p = \sqrt{\left(\left(\frac{d\,V}{d\,x}\right)^2 + \left(\frac{d\,V}{d\,y}\right)^2 + \left(\frac{d\,V}{d\,z}\right)^2\right)}.$$

4.

If $d\,s$ be the element of a line, either straight or curved, $\frac{d\,x}{d\,s}, \frac{d\,y}{d\,s}, \frac{d\,z}{d\,s}$ are the cosines of the angle which that element makes with the coordinate axes; and if θ denote the angle between the direction of the element and that of the resultant force, then

$$\cos \theta = \frac{d\,x}{d\,s} \,.\, \cos \alpha + \frac{d\,y}{d\,s} \,.\, \cos \beta + \frac{d\,z}{d\,s} \,.\, \cos \gamma.$$

The force resolved in the direction of $d\,s$ becomes consequently

$$p \cos \theta = \epsilon \left(\frac{d\,V}{d\,x} \,.\, \frac{d\,x}{d\,s} + \frac{d\,V}{d\,y} \,.\, \frac{d\,y}{d\,s} + \frac{d\,V}{d\,z} \,.\, \frac{d\,z}{d\,s}\right) = \frac{\epsilon\, d\,V}{d\,s}.$$

If a surface pass through all the points in which the potential V has a constant value, it will, generally speaking, separate the portion of space in which V is greater than that value, from that in which it is less. If the line s lie in this surface, or be at least tangential to it at the element $d\,s$, then $\frac{d\,V}{d\,s} = 0$. Unless, then, the constituents of the whole force destroy each other at this point, or $p = 0$, in which case there can be no longer question of a direction of the force, $\cos \theta$ must necessarily $= 0$; whence we conclude that the direction of the resultant force at every point of such a surface must be normal to that surface, and will be directed towards that part of space where the higher values of V exist if $\epsilon = +1$, and to the opposite side if $\epsilon = -1$. We call such a surface a surface of equilibrium. As it may be drawn through any point, the line s, if not included in one surface of equilibrium, will at every point meet a different surface. If the line s intersects all the surfaces of equilibrium at right angles, a tangent to this line will everywhere represent the direction of the force, and $\frac{dV}{ds}$ will represent its intensity.

The integral $\int p \cos\theta \,.\, ds$, extended through a part of the line s, is evidently $= \epsilon\,(V' - V^0)$; V^0, V' denoting the values of the potential for the limiting points. Thus if s be a closed line, the integral extended throughout the whole line will $= 0$.

5.

It is evident that the potential must have an assignable value at every point of space lying without all the attracting or repelling particles; the same must also be true of all its differential coefficients, as well of the first as of the higher orders, as under the above supposition these must likewise assume the form of sums of assignable parts, or of integrals of differentials, of which the coefficients have throughout assignable values. Thus,

$$\frac{dV}{dx} = \Sigma\,\frac{(a-x)\,\mu}{r^3},$$

$$\frac{d^2V}{dx^2} = \Sigma\left(\frac{3\,(a-x)^2}{r^5} - \frac{1}{r^3}\right)\mu,$$

$$\frac{dV}{dy} = \Sigma\,\frac{(b-y)\,\mu}{r^3},$$

$$\frac{d^2V}{dy^2} = \Sigma\left(\frac{3\,(b-y)^2}{r^5} - \frac{1}{r^3}\right)\mu,$$

$$\frac{dV}{dz} = \Sigma\,\frac{(c-z)\,\mu}{r^3},$$

$$\frac{d^2V}{dz^2} = \Sigma\left(\frac{3\,(c-z)^2}{r^5} - \frac{1}{r^3}\right)\mu.$$

Thus the well-known equation

$$\frac{d^2V}{dx^2} + \frac{d^2V}{dy^2} + \frac{d^2V}{dz^2} = 0$$

holds good for all points of space situated outside the acting masses.

6.

Among the different cases in which the value of the potential V, or of its differential coefficients, may be sought for a point not situated outside the acting masses, we will first consider the case which occurs in nature, in which a determinate space is filled with matter of uniform or varying, but everywhere of finite, density.

Let t be the whole space containing the mass; $d\,t$ an infinitesimal element, of which the coordinates are a, b, c, and the mass $k\,d\,t$; further, let V be the potential at the point O, the coordinates of which are x, y, z; the distance of the element from the point O is therefore

$$\sqrt{((a-x)^2 + (b-y)^2 + (c-z)^2)} = r;$$

consequently

$$V = \int \frac{k\,d\,t}{r}$$

extended through the whole space t, which implies a triple integration. It will be easily perceived that a real integration may be executed, even if O is within the space, although $\frac{1}{r}$ then becomes infinitely large for those elements which are infinitely near O. For if instead of a, b, c we introduce polar coordinates, by making

$$a = x + r\cos u,\quad b = y + r\sin u\cos\lambda,\quad c = z + r\sin u\sin\lambda,$$

then $d\,t$ becomes $= r^2 \sin u\,.\,d u\,.\,d\lambda\,.\,d r$, and at the same time,

$$V = \int\int\int k\,r\sin u\,.\,d u\,.\,d\lambda\,.\,d\,r,$$

where the integration, as it relates to r, must be extended, from $r = 0$ to the values existing at the limit of t, from $\lambda = 0$ to $\lambda = 2\pi$, and from $u = 0$ to $u = \pi$. V will thus necessarily receive a determinate finite value.

Further, it is easy to perceive that we may here also say

$$\frac{d\,V}{d\,x} = \int k\,d\,t\,.\,\frac{d\,\frac{1}{r}}{d\,x} = \int \frac{k\,(a-x)\,d\,t}{r^3} = X.$$

This expression, which, by the introduction of polar coordinates, becomes

$$\int\int\int k\cos u\,.\,\sin u\,.\,d u\,.\,d\lambda\,.\,d r,$$

is susceptible of a real integration; thus X receives a determinate finite value, which varies continuously, because all the elements, situated infinitely near O, can only contribute an infinitely small addition to it. For similar reasons we may also make

$$\frac{dV}{dy} = \int \frac{k\,(b-y)\,dt}{r^3} = Y;\quad \frac{dV}{dz} = \int \frac{k\,(c-z)\,dt}{r^3} = Z,$$

and thus these functions, as well as V, will have withinside t determinate values varying continuously. The same will still hold good at the limit of t.

7.

In regard to the differential coefficients of the higher orders, a different process must be adopted for points withinside t; for example, it is not permissible to transform $\frac{dX}{dx}$ into

$$\int k\,dt\,.\,\frac{d\,\frac{a-x}{r^3}}{dx};\ \text{that is, into} \int k\left(\frac{3\,(a-x)^2-r^2}{r^5}\right)dt,$$

inasmuch as this expression, strictly considered, would be only a sign without any determinate clear signification. For, in fact, within every part of t, however small, which includes the point, portions may be determined, throughout which, if the integral be taken, it will exceed any given, positive, or negative value. Thus the essential condition fails, under which alone a definite signification might be attached to the integral, namely, the applicability of the method of exhaustion.

8.

Before undertaking this investigation in its generality, the consideration of a very simple particular case will be useful towards a clear understanding of the subject.

Let t be a sphere, of which the semi-diameter $= R$, the centre coinciding with the origin of coordinates; let the density of the mass which fills the sphere be constant $= k$, and let us denote the distance of the point O from the centre by $\varrho = \sqrt{(x^2+y^2+z^2)}$. It is well known that the potential has two different expressions, according as O is situated within or without the sphere. In the first case,

$$V = 2\pi k R^2 - \frac{2}{3}\pi k \varrho^2 = 2\pi k R^2 - \frac{2}{3}\pi k\,(x^2+y^2+z^2),$$

and in the second case,

$$V = \frac{4\pi k R^3}{3\rho}.$$

On the surface of the sphere both the expressions give the same value, and thus the potential varies continuously throughout space.

For the differential coefficients we obtain, in the internal space,

$$\frac{dV}{dx} = X = -\frac{4}{3}\pi k x; \quad \frac{dV}{dy} = Y = -\frac{4}{3}\pi k y; \quad \frac{dV}{dz} = Z = -\frac{4}{3}\pi k z;$$

and in the external space,

$$X = -\frac{4\pi k R^3 x}{3\varrho^3}; \quad Y = -\frac{4\pi k R^3 y}{3\varrho^3}; \quad Z = -\frac{4\pi k R^3 z}{3\varrho^3}.$$

Here, also, the latter formulæ give the same values on the surface as the former ones; thus X, Y, Z, also vary continuously throughout space.

But it is different with the differential coefficients of these functions. In the inner space we have

$$\frac{dX}{dx} = -\frac{4}{3}\pi k, \quad \frac{dY}{dy} = -\frac{4}{3}\pi k; \quad \frac{dZ}{dz} - = \frac{4}{3}\pi k.$$

In the outer space, on the other hand,

$$\frac{dX}{dx} = \frac{4\pi k R^3 (3x^2 - \rho^2)}{3\rho^5}; \quad \frac{dY}{dy} = \frac{4\pi k R^3 (3y^2 - \rho^2)}{3\rho^5}; \quad \frac{dZ}{dz} = \frac{4\pi k R^3 (3z^2 - \rho^2)}{3\rho^5}.$$

On the surface the latter values do not coincide with the former, but surpass them respectively by the quantities

$$\frac{4\pi k x^2}{R^2}, \quad \frac{4\pi k y^2}{R^2}, \quad \frac{4\pi k z^2}{R^2}.$$

Thus the above differential coefficients vary continuously in the whole of the internal and the whole of external space, but discontinuously in passing from one to the other; and in the separating surface itself they must be considered as having double values, according as *dx*, *dy*, *dz* are regarded as positive or as negative.

It is the same with the remaining six differential coefficients,

$$\frac{dX}{dy}, \quad \frac{dX}{dz}, \quad \frac{dY}{dx}, \quad \frac{dY}{dz}, \quad \frac{dZ}{dx}, \quad \frac{dZ}{dy},$$

which in the interior of the sphere are each $= 0$, and in their passage through the surface of the sphere suddenly undergo the alterations $\frac{4\pi k x y}{R^2}$, $\frac{4\pi k x z}{R^2}$, &c.

The sum $\frac{dX}{dx}+\frac{dY}{dy}+\frac{dZ}{dz}$, or $\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+\frac{d^2V}{dz^2}$ becomes within the sphere $= -4\pi k$, and in external space $= 0$. But on the surface itself it loses its simple signification: speaking precisely, we can only say that it is an aggregate of three parts, each of which has two different values; and thus there are properly eight combinations, one of which agrees with the values within the sphere, and another with those without the sphere, whilst the remaining six have no signification whatsoever. The analysis by which some geometers have derived for the surface of the sphere the value $-2\pi k$, or the mean between the internal and external values, cannot, I think, be regarded as satisfactory, if the idea of differential coefficients is conceived in its mathematical purity.

9.

The result obtained in the preceding example is only a particular case of the general theorem, according to which, if the point O be situated in the interior of the acting mass, the value of $\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+\frac{d^2V}{dz^2}$ is equal to the product of -4π multiplied into the density at O. The most satisfactory basis of this important theorem appears to be the following.

We assume that within t the density k nowhere varies discontinuously, or that it is a function of a, b, c represented by $f(a, b, c)$, the value of which varies continuously everywhere within t, but without t becomes $= 0$.

Let t' be the space into which t is changed if the first coordinate of each point of the bounding surface be diminished by the quantity e; or, which is the same thing, if the bounding surface be moved by that quantity towards the origin of co-ordinates in a direction parallel to the first coordinate axis; let t consist of the spaces t° and θ, t' of t° and θ', so that t° shall be the whole space common to t and t'.

We have to consider the three integrals.

$$\int \frac{f(a, b, c)\,(a-x)\,dt}{((a-x)^2+(b-y)^2+(c-z)^2)^{\frac{3}{2}}} \quad . \quad . \quad . \quad (1.)$$

$$\int \frac{f(a, b, c)\,(a-x-e)\,dt}{((a-x-e)^2+(b-y)^2+(c-z)^2)^{\frac{3}{2}}} \quad . \quad . \quad (2.)$$

$$\int \frac{f(a+e,b,c)(a-x)\,dt}{((a-x)^2+(b-y)^2+(c-z)^2)^{\frac{3}{2}}} \quad . \quad . \quad . \quad (3.)$$

where the integral (1.) extended through the whole space t is the value $\frac{d\,\mathrm{V}}{d\,x}$ or X, at the point O. The integral (2.), in like manner, extended through the whole of t, is the value of $\frac{d\,\mathrm{V}}{d\,x}$, at the point of which the coordinates are $x+e, y, z$; we will denote this value by $\mathrm{X}+\xi$. It is manifest that this integral and the integral (3.) extended through the whole space t' are perfectly identical. So if

the integral (1.) extended		through t^0 be	 l
"	"	through θ be	. . . . . λ
"	(3.) "	through t^0 be	. . . . . l'
"	"	through θ' be	. . . . . λ'

then $\mathrm{X}=l+\lambda$, $\mathrm{X}+\xi=l'+\lambda'$.

If we write $f(a+e,b,c)-f(a,b,c)=\Delta\,k$, then the integral

$$\int \frac{\frac{\Delta\,k}{e}(a-x)\,dt}{((a-x)^2+(b-y)^2+(c-z)^2)^{\frac{3}{2}}} \quad . \quad . \quad . \quad (4.)$$

extended over t^0 will be $=\frac{l'-l}{e}$.

The results hitherto obtained hold good generally for every position of O. In the further development, the case in which O is situated in the surface itself will be excluded, or O will be assumed to be at a finite distance from the surface, either within or without t.

Now if we make e infinitely small, the spaces $\theta\,\theta'$ are two infinitely thin strata of space at the surface of t. If we resolve this surface into elements $d\,s$, and denote by α the angle which a perpendicular raised outwards at $d\,s$ forms with the first coordinate, it is manifest that α will be acute everywhere where the surface of t adjoins θ, and, on the contrary, obtuse wherever it adjoins θ'. The elements of θ may thus be expressed by $e\cos\alpha\,ds$, and the elements of θ' by $-e\cos\alpha\,ds$, whence it may easily be concluded that $\frac{\lambda-\lambda'}{e}$ passes into the integral

$$\int \frac{f(a,b,c)\;(a-x)\cos\alpha\,d\,s}{((a-x)^2+(b-y)^2+(c-z)^2)^{\frac{3}{2}}},$$

or, which is the same thing, into the integral

$$\int \frac{k\;(a-x)\cos\alpha\,.\,d\,s}{r^3}$$

extended through the whole surface, where k represents the density at the element $d\,s$.

Upon the assumption of an infinitely small value of e, $\frac{\Delta\,k}{e}$ will further pass into the value of the partial differential quotient $\frac{d\,f\,(a,b,c)}{d\,a}$, or $\frac{d\,k}{d\,a}$, and the value of the integral (4.), or $\frac{l'-l}{e}$ into the integral

$$\int \frac{\frac{d\,k}{d\,a}\,.\,(a-x)\,d\,t}{r^3}$$

extended through the whole space t.

Lastly, for an infinitely small value of e, $\frac{l'-l}{e} - \frac{\lambda-\lambda'}{e}$, or $\frac{\xi}{e}$ is no other than the value of the partial differential coefficients $\frac{d\,X}{d\,x}$ or $\frac{d^2\,V}{d\,x^2}$. We have consequently the simple result

$$\frac{d^2\,V}{d\,x^2} = \frac{d\,X}{d\,x} = \int \frac{\frac{d\,k}{d\,a}\,.\,(a-x)\,d\,t}{r^3} - \int \frac{k\,(a-x)\cos\alpha\,.\,d\,s}{r^3},$$

where the first integration is to be extended through the whole space t, and the second over the whole of its surface.

This result holds good, however near O may be to the surface, either on the inside or on the outside, providing it be not actually on the surface itself, where $\frac{d\,X}{d\,x}$ would have two different values. The first integral, it is true, varies continuously in passing through the surface; but according to a theorem which will be demonstrated in the sequel, $-\int \frac{k\,(a-x)\cos\alpha\,d\,s}{r^3}$, in passing from a point infinitely near the surface on the inside, to one on the outside, varies by the finite quantity $4\,\pi\,k\cos\alpha$, where k and α refer to the point of passage, and to this same quantity the difference between the two values of $\frac{d\,X}{d\,x}$ at this place will be equal.

10.

In like manner, if β and γ have the same signification with relation to the second and third coordinate axes as α with relation to the first, and preserving the same restriction as to the situation of O, we have, as before,

$$\frac{d\,\mathrm{Y}}{d\,y}=\int\frac{\frac{d\,k}{d\,b}\,(b-y)\,d\,t}{r^3}-\int\frac{k\,(b-y)\cos\beta\,.\,d\,s}{r^3},$$

$$\frac{d\,\mathrm{Z}}{d\,z}=\int\frac{\frac{d\,k}{d\,c}\,(c-z)\,d\,t}{r^3}-\int\frac{k\,(c-z)\cos\gamma\,.\,d\,s}{r^3}.$$

Now, if we take into consideration that

$$\frac{d\,k}{d\,a}\,.\,\frac{a-x}{r}+\frac{d\,k}{d\,b}\,.\,\frac{b-y}{r}+\frac{d\,k}{d\,c}\,.\,\frac{c-z}{r}$$

is precisely the value of the differential coefficient $\frac{d\,k}{d\,r}$; inasmuch as in this differentiation the length of r only is to be considered as variable, and its direction is to be regarded as constant; and further, that

$$\frac{a-x}{r}\,.\cos\alpha+\frac{b-y}{r}\,.\cos\beta+\frac{c-z}{r}\,.\cos\gamma=\cos\psi\,;$$

where ψ denotes the angle which the perpendicular, raised outwards at $d\,s$, makes with the straight line r produced, it is manifest that if the integral

$$\int\frac{\frac{d\,k}{d\,r}}{r^2}\,.\,d\,t$$

extended through the whole space t be called M, and the integral

$$\int\frac{k\cos\psi}{r^2}\,d\,s$$

extended through the whole surface of t be called N, then . . . .

$$\frac{d^2\,\mathrm{V}}{d\,x^2}+\frac{d^2\,\mathrm{V}}{d\,y^2}+\frac{d^2\,\mathrm{V}}{d\,y^2}=\mathrm{M}-\mathrm{N}.$$

In order to effect the first integration, we describe a spherical surface with the radius 1 about the centre O, and we resolve it into elements $d\,\sigma$. Straight lines of indefinite length, proceeding from the centre O, and passing through all the points of the periphery of $d\,\sigma$, form a conical surface (in the more extended sense of the word), by which, out of the whole space t, a space, consisting, it may be, of several detached parts, is separated, of which $r^2\,d\,\sigma\,.\,d\,r$ is an indefinite element. That part of M which belongs to this space is consequently expressed by $d\sigma \int \frac{dk}{dr} \cdot dr$, if this integration be extended through all the parts within t, of a straight line r passing through O and a point of $d\sigma$, and prolonged as far as necessary. Now, if we assume that the straight line intersects the surface of t successively at O$'$, O$''$, O$'''$, O^{IV}, &c.; if we denote by r', r'', r''', r^{IV}, &c, the values of r at those points; by ds', ds'', ds''', ds^{IV}, the corresponding elements separated by the cone from the surface of t; by k', k'', k''', k^{IV}, &c., the values of k; and by ψ', ψ'', ψ''', ψ^{IV}, &c., the values of ψ for these elements; then it is easily perceived,

I. That for the case of O within t, the number of those points will be uneven, and the integration $\int \frac{dk}{dr} \cdot dr$ must be performed from $r = 0$ to $r = r'$, then from $r = r''$ to $r = r'''$, &c., whence it follows that if the density at O be designated by k^0

$$\int \frac{dk}{dr} \cdot dr = -k^0 + k' - k'' + k''' - k^{IV}, \text{ \&c.}$$

As the angles ψ', ψ'', ψ''', ψ^{IV}, are manifestly alternately acute and obtuse

$$\begin{aligned} ds' \,.\cos\psi' &= +r'^2\, d\sigma, \\ ds'' \,.\cos\psi'' &= -r''^2\, d\sigma, \\ ds''' \,.\cos\psi''' &= +r'''^2\, d\sigma, \\ ds^{IV} .\cos\psi^{IV} &= -r^{IV^2} d\sigma, \text{ \&c.} \end{aligned}$$

and consequently,

$$d\sigma \int \frac{dk}{dr} \cdot dr = -k^0 d\sigma + \frac{k' \cos\psi'}{r'^2} ds' + \frac{k'' \cos\psi''}{r''^2} ds''$$
$$\frac{k''' \cos\psi'''}{r'''^2} + \text{\&c.} = -k^0 d\sigma + \Sigma \frac{k \cos\psi}{r^2} ds,$$

as the summation is extended to all the ds which correspond to the element $d\sigma$. By the integration of all the $d\sigma$ we thus obtain

$$M = 4\pi k^0 + \int \frac{k\cos\psi}{r^2} ds,$$

where the integral must be extended over the whole surface, or $M = 4\pi k^0 + N$. Consequently

$$\frac{d^2 V}{dx^2} + \frac{d^2 V}{dy^2} + \frac{d^2 V}{dz^2} = -4\pi k^0.$$

II. For the case of O situated outside of t, we have to take into consideration only those $d\sigma$, for which the straight line drawn through O and a point of $d\sigma$ really meets the space t; the number of the points O′, O″, O‴, &c. will, in this case, always be an even number, and the angles ψ', ψ'', ψ''', &c. will be alternately acute and obtuse, thus $ds' \,.\, \cos\psi' = -r'^2\, d\sigma$, $ds'' \cos\psi'' = -r''^2\, d\sigma$, $ds''' \cos\psi''' = -r'''^2\, d\sigma$, &c. Now, as the integration $\int \frac{dk}{dr} \,.\, dr$ must be performed from $r = r'$ to $r = r''$, and then from $r = r'''$ to $r = r^{\text{IV}}$, &c., we obtain

$$d\sigma \int \frac{dk}{dr} \,.\, dr = \frac{k' \cos\psi'}{r'^2} \,.\, ds' + \frac{k'' \cos\psi''}{r''^2} \,.\, ds''$$

$$+ \frac{k''' \cos\psi'''}{r'''^2} \,.\, ds''' + \text{\&c.} = \Sigma \frac{k\cos\psi}{r^2}\, ds.$$

and by the second integration carried through all the $d\sigma$,

$$\text{M} = \int \frac{k\cos\psi}{r^2}\, ds = \text{N};$$

consequently

$$\frac{d^2 V}{dx^2} + \frac{d^2 V}{dy^2} + \frac{d^2 V}{dz^2} = 0.$$

11.

Although it has been assumed in this demonstration that the density varies continuously in the *whole* space t, yet this condition is not necessary to the validity of our results, which merely requires that at the point O the density shall vary continuously in every direction, or that O shall be situated in a space, however small, within which the conditions are satisfied. If we call the potential of the mass contained in *this* space $=$ V′, and the potential of the remaining mass situated beyond $=$ V″, we shall have the whole potential $V = V' + V''$, and, as according to the preceding article

$$\frac{d^2 V'}{dx^2} + \frac{d^2 V'}{dy^2} + \frac{d^2 V'}{dz^2} = -4\pi k^0,$$

$$\frac{d^2 V''}{dx^2} + \frac{d^2 V''}{dy^2} + \frac{d^2 V''}{dz^2} = 0,$$

then

$$\frac{d^2 V}{dx^2} + \frac{d^2 V}{dy^2} + \frac{d^2 V}{dz^2} = -4\pi k^0.$$

If, on the other hand, this condition fail at the point O, and that point be situated in the separating surface of two spaces, in each of which, taken apart, the density varies continuously, but varies discontinuously in passing from one to the other, then, generally, $\frac{d^2 V}{d x^2}$, $\frac{d^2 V}{d y^2}$, $\frac{d^2 V}{d z^2}$, have each there two different values, and what has been said at the close of Art. 8. is true of the aggregate of those quantities.

12.

We have included in our investigation, as has been already remarked, the ideal case in which the attracting or repelling forces are supposed to proceed from the parts of a *surface*, and in doing so have permitted ourselves to represent the acting mass as distributed over the surface. By density at any point of the surface we understand in this case the quotient of the mass contained in a surface element to which the point belongs, divided by that element. This density may be uniform in all the points of the surface, or not uniform, and in the latter case it may either vary in the whole surface continuously (*i. e.* differing infinitely little between any two points infinitely near to each other), or the whole surface may be resolved into two or more portions, in each of which the variation is continuous, whilst, in passing from one to another, the change is sudden. We may, moreover, imagine such a distribution, that, notwithstanding the whole mass be finite, the density in particular points or lines shall be infinitely great. The surface itself not being a plane, will generally have a continuous curvature, without, however, excluding an interruption in singular points (cusps) or lines (edges).

These suppositions being made, the potential receives at each point of the surface, where the density is not infinitely great, a determinate finite value, between which and the value at a second point infinitely near to it, either in the surface or without it, there can be only an infinitely small difference*; or, in other words, in any line, whether in the surface or cutting it, the potential varies continuously.

* It is easy to convince oneself of the finite value of the integral which expresses the potential, by resolving the surface into elements in a manner similar to that in which it is done in Art. 15., and it will at once be seen from thence that the parts of the surface infinitely near to the two points in question, contribute infinitely little to the whole integral; from whence what is said above may be easily demonstrated.

13.

If we denote by k the density at the surface element ds; by a, b, c the coordinates of a point belonging to it; by r the distance of that point from a point O, the coordinates of which are x, y, z; and by V the potential of the mass contained in the surface at the point O, then is $V = \int \frac{k\,ds}{r}$ extended through the whole surface; and lastly, if we denote by X, Y, Z the integrals

$$\int \frac{k\,(a-x)\,ds}{r^3}, \int \frac{k\,(b-y)\,ds}{r^3}, \int \frac{k\,(c-z)\,ds}{r^3};$$

it is true that X, Y, Z have thus the same signification as $\frac{dV}{dx}, \frac{dV}{dy}, \frac{dV}{dz}$, so long as O is situated without the surface; but, strictly speaking, this is no longer the case if O is a point of the surface itself; and the inequality is of different kinds, according to the nature of the angle formed by the normal to the surface and the coordinate axis which it encounters. It is obviously sufficient to give here the relation with respect to the first coordinate axis.

I. If that angle $= 0$, then the integral X has a determinate value at O; on the other hand, $\frac{dV}{dx}$ has two different values, according as dx is regarded as positive or as negative.

II. If the angle is a right angle, the expression for X does not admit a true integration (inasmuch as a remark similar to that in Art. 7. will hold good), while $\frac{dV}{dx}$ has only one determinate value.

III. If the angle is acute, X is as in the second, and $\frac{dV}{dx}$ as in the first case.

Other modifications are introduced if there be at O an interruption of continuity in respect either of density or of curvature. However, our main object does not require us to treat in detail such exceptional cases, which can only occur in particular lines or points; therefore, in pursuing the subject more closely, we shall assume that at the points in question there is a determinate finite density, and a determinate tangential plane.

14.

Before undertaking the investigation in its generality, it will be useful to consider a simple particular case. Let the surface be a portion, A, of the surface of a sphere, and let the density in it be uniform, or k be constant. Then V, X, are the values of the integral $\int \frac{k\,d s}{r}$, $\int \frac{k\,(a - x)\,d s}{r^3}$, taken through A; and let V X′ represent the value of the same integral extended through the remainder of the spherical surface B, V^o, X^o, when extended through the whole of the surface of the sphere. Then $V = V^o - V'$, $X^o = X - X'$. Further, let the radius of the sphere be called R; let the origin of coordinates be the centre of the sphere; and let $\sqrt{(x^2 + y^2 + z^2)}$, or the distance of the point O from the centre of the sphere, be called $= \rho$.

It is known that $V^0 = 4\,\pi\,k\,R$, if O is within the sphere; and if, on the other hand, O is without the sphere, $V^0 = \frac{4\,\pi\,k\,R^2}{\rho}$: at the surface of the sphere the two values coincide. Hence the differential coefficient $\frac{d\,V^0}{d\,x} = 0$ inside the sphere, and $= -\frac{4\,\pi\,k\,R^2\,x}{\rho^3}$ outside the sphere; at the surface itself both values will hold good, each according to the sign of $d\,x$: these two values are equal, only if $x = 0$, which corresponds to Case II. in the preceding Article.

The expression for X^0 having the same signification as $\frac{d\,V^0}{d\,x}$ within and without the sphere, becomes on the surface a sign without any meaning, as a real integration is impossible, except in that particular case in which for elements lying infinitely near the surface, $a - x$ becomes an infinitesimal of a higher order than r; namely, if $y = o$, $z = o$, $x = \pm R$; in which case the integration gives $X^0 = 2\,\pi\,k$, thus not agreeing with either of the values of $\frac{d\,V^0}{d\,x}$, but rather with the mean of the two: it is clear that this case belongs to Division I. in the foregoing Article.

If we now consider that when O is a point on the surface of the sphere situated within A, X′ and $\frac{d\,V'}{d\,x}$ have the same signification, and that determinate continuously varying values, it is clear that the mutual relation between $X^0 - X'$ and $\frac{d V^0}{d x} - \frac{d V'}{d x}$; *i.e.* between X and $\frac{d V}{d x}$, is exactly the same as between X^0 and $\frac{d V^0}{d x}$: whence the propositions in the preceding Article follow of themselves.

15.

For the more general investigation, it is advantageous to take the origin of coordinates at a point P, situated in the surface itself, and to make the first coordinate axis perpendicular to the tangent plane at P. If we denote by ψ the angle between the normal to the indefinite surface element $d s$ and the first coordinate axis, then $\cos \psi \, d s$ is the projection of $d s$ upon the plane of b and c; and if we call $\sqrt{(b^2 + c^2)} = \rho$, $b = \rho \cos \theta$, $c = \rho \sin \theta$, then $\rho \, d \rho . d \theta$ will represent an indeterminate element of this plane, and the corresponding surface element $d s = \frac{\rho \, d \rho . d \theta}{\cos \psi}$; the element of mass contained therein will be $= h \rho \, d \rho . d \theta$, if for the sake of brevity, we write, h for $\frac{k}{\cos \psi}$.

We will now examine how far the value of X changes discontinuously, as the point O in the first coordinate axis passes from one side of the surface to the other, or as x passes from a negative to a positive value. In this question, it is obviously indifferent, whether we take into consideration the whole surface, or only an indefinitely small part of it enclosing the point P, as the share contributed by the remaining part of the surface to the value of X changes continuously. We may therefore take ϱ only from 0 to an indefinitely small limitary value ρ', and suppose that in the surface so bounded h and $\frac{a}{\rho}$ vary everywhere continuously. If for any determinate value of θ we call Q the value of the integral $\int \frac{h (a - x) \rho \, d \rho}{r^3}$, taken from $\varrho = 0$ to $\rho = \varrho'$, then $X = \int Q \, d \theta$; where the integration is to be taken from $\theta = 0$ to $\theta = 2 \pi$.

We have now to compare the values of X for $x = 0$, for an infinitely small positive x, and for an infinitely small negative x (the other two coordinates being always assumed $= 0$); we will call these three values of X, X^0, X', X'', and the corresponding values of Q, Q^0, Q', Q''.

As $r = \sqrt{((a-x)^2 + \rho^2)}$, we obtain, considering θ as constant,

$$d\,\frac{h\,(a-x)}{r} = -\frac{h\,(a-x)\,\rho\,d\rho}{r^3} + \frac{d\,h}{d\,\rho}\cdot\frac{a-x}{r}\cdot d\rho + \frac{d\,a}{d\,\rho}\cdot\frac{h\,\rho^2}{r^3}\cdot d\,\rho;$$

and consequently Q =

$$\int\frac{d\,h}{d\,\rho}\cdot\frac{a-x}{r}\cdot d\,\rho + \int\frac{d\,a}{d\,\rho}\cdot\frac{h\,\varrho^2}{r^3}\cdot d\,\varrho - \frac{h'\,(a'-x)}{r'} + \text{const.}:$$

where both the integrations are to be extended from $\rho = 0$ to $\rho = \rho'$, and the values of h, a, r for $\rho = \rho'$ are denoted by h', a', r'. We must assume as a constant the value of $\frac{h\,(a-x)}{r}$ for $\rho=0$, which, if we designate the density at P by k^0, becomes $= -k^0$ for a positive x, and $= +k^0$ for a negative x; as it is manifest that for $\rho = 0$ we have $a = 0$, $\psi = 0$, $h = k^0$, $x = \pm r$. For the case of $x = 0$, on the other hand, we must assume as constant the limitary value of $\frac{h\,a}{r}$, as ρ decreases indefinitely; this is $=0$, because a is an infinitesimal of a higher order than r.

The value of the integral $\int\frac{d\,h}{d\,\rho}\cdot\frac{a-x}{r}\cdot d\,\varrho$ only differs by an infinitesimal, whether we make $x = 0$, or infinitely small, $= \pm\,\epsilon$. If we decompose that integral into

$$\int_0^\delta\frac{d\,h}{d\,\rho}\cdot\frac{a-x}{r}\cdot d\rho + \int_\delta^{\rho'}\frac{d\,h}{d\,\varrho}\cdot\frac{a-x}{r}\cdot d\,\rho,$$

it is clear that what has been said holds good for the first member, if δ is infinitely small; and for the second, if $\frac{\delta}{\epsilon}$ is infinitely great; and so for the whole, if δ is an infinitesimal of a lower order than ϵ.

A similar conclusion holds good also in relation to the integral $\int\frac{d\,a}{d\rho}\cdot\frac{h\,\rho^2}{r^3}\cdot d\,\varrho$, if the points of the surface which correspond to the determinate values of θ form a curve having a finite curvature at P, so that $\frac{a}{\rho^2}$ may receive in the space here contemplated a finite continuously varying value. If we put A for this value, then

$$\frac{d\,a}{d\,\rho} = 2\,\text{A}\,\varrho + \frac{d\,\text{A}}{d\,\rho}\cdot\rho^2;$$

and the integral is decomposed into the two following,

$$\int \frac{2\,\rho^3\,A\,h\,d\rho}{r^3} + \int \frac{dA}{d\rho} \cdot \frac{\rho^4}{r^3}\,h\,d\rho;$$

in both which the validity of the above-given mode of conclusion is self-evident.

Lastly, it is manifest that the values of $h' \frac{(a' - x)}{r'}$ are the same for all the three values of x within infinitely small differences.

Hence it follows that $Q' + k^0, Q^0, Q'' - k^0$, differ infinitesimally; and the same will accordingly hold good of $\int (Q' + k^0)\,d\,\theta$, $\int Q^0\,d\,\theta$, $\int (Q'' - k^0)\,d\,\theta$, or of the quantities $X' + 2\,\pi\,k^0, X^0$, $X'' - 2\,\pi\,k^0$.

This important proposition may also be expressed thus: The limiting value of X, with infinitely decreasing positive x, is $X^0 - 2\,\pi\,k^0$, and with infinitely decreasing negative x, it is $X^0 + 2\,\pi\,k^0$, or X changes twice suddenly by $-\,2\,\pi\,k^0$, as x passes from a negative to a positive value; the first time as x reaches the value 0, the second time as it passes it.

16.

In the demonstration in the foregoing Article it has been supposed that the intersections of the surface with planes passing through the first coordinate axis at P have a finite curvature: but our result will remain valid even if the curvature at P were infinitely great, with the exception of a single case. It follows from the supposition of the existence of a determinate tangent plane to the surface at P, that for an infinitely small ρ, $\frac{a}{\rho}$ must itself be infinitely small; but the two values will be of the same order only if there is a finite radius of curvature; with an infinitely small radius of curvature, $\frac{a}{\rho}$ will be of a lower order than ρ. We will now show that our results preserve their validity in the latter case also, provided only that the orders of the two latter quantities be *comparable.*

Thus if we assume $\frac{a}{\rho}$ to be of the same order as ρ^μ, where μ denotes a finite positive exponent, then $\frac{a}{\rho^{1+\mu}}$ represents a finite quantity varying continuously within the space under consideration, which quantity we will call B. Thus the integral $\int \frac{d\,a}{d\,\rho} \cdot \frac{h\,\rho^2}{r^3}\,d\,\varrho$ resolves itself into the two following:

$$\int \frac{(1+\mu)\,\varrho^{2+\mu}\,h\,\mathrm{B}\,d\,\varrho}{r^3} + \int \frac{\rho^{3+\mu}}{r^3} \cdot \frac{d\,\mathrm{B}}{d\,\rho} \cdot h\,d\,\rho.$$

The conclusions in the preceding article may be applied to the second integral immediately, and to the first after a slight transformation. If we call $\frac{1}{\mu} = m$, $\rho^{\mu} = \sigma$, or $\rho = \sigma^m$, then that integral becomes

$$= (m+1)\int \frac{\mathrm{B}\,h\,\sigma^3\,m\,d\,\sigma}{(\sigma^{2m} + (a-x)^2)^{\frac{3}{2}}}.$$

It is manifest that this integral also has an infinitely small value, only so long as the integration is extended no further than from 0 to an infinitely small value of σ; whereas for each finite value of σ the coefficient of $d\sigma$ receives the same value, within an infinitely small difference, whether x be taken $= 0$, or as infinitely small. This holds good likewise of the whole integral if extended from $\sigma = 0$ to $\sigma = \sqrt[m]{\rho'}$.

There is only one case in which our conclusions lose their validity, if $\frac{a}{\varrho}$ is not of the same order as any power of ϱ, as, for example, if $\frac{a}{\varrho}$ is of the same order as $\frac{1}{\log\frac{1}{\varrho}}$: in this case, as the point O approximates infinitely to the surface, Q increases beyond all limits; and the same would be true for X also, if this relation existed not merely for some particular values of θ, but for all. It is, however, unnecessary at present to proceed further in this development, as we may exclude this particular case from our investigation without any disadvantage.

17.

Employing the same suppositions and notations as in Art. 15, we proceed to the consideration of the quantity Y, of which $\frac{h\,b\,d\,b\,\,d\,c}{r^3}$ is an indefinite element. As $r = \sqrt{(b^2 + c^2 + (a-x)^2)}$, and consequently

$$\frac{d\,\frac{h}{r}}{d\,b} = -\frac{h\,b}{r^3} + \frac{1}{r} \cdot \frac{d\,h}{d\,b} - \frac{h\,(a-x)}{r^3} \cdot \frac{d\,a}{d\,b}$$

c being considered as constant. The first integration gives upon this hypothesis,

$$\int \frac{h\,b\,d\,b}{r^3} = \frac{h^*}{r^*} - \frac{h^{**}}{r^{**}} + \int \frac{1}{r} \cdot \frac{d\,h}{d\,b} \cdot d\,b - \int \frac{h\,(a-x)}{r^3} \cdot \frac{d\,a}{d\,b} . d\,b,$$

when the integrations extend from the least to the greatest value of b for every given value of c, and h^*, r^*, h^{**}, r^{**}, denote the values of h and r, corresponding to those limiting values. If, for the sake of brevity, we write

$$\frac{h^*}{r^*} - \frac{h^{**}}{r^{**}} = \mathrm{T}, \quad \frac{\varrho}{r} \cdot \frac{d\,h}{d\,b} - \frac{h\,(a-x)\,\varrho}{r^3} \cdot \frac{d\,a}{d\,b} = \mathrm{U}$$

then

$$\mathrm{Y} = \int \mathrm{T}\,d\,c + \iint \frac{\mathrm{U}}{\varrho}\,d\,b\,d\,c,$$

where the integration with respect to c must be extended from the least value of this coordinate at the surface to its greatest value. In the double integral $d\,b \;\; d\,c$ represents the projection of an indefinite element of the surface on the plane of b, c, and therefore $\varrho\,d\,\varrho\;d\,\theta$ may be written for it; in which case

$$\mathrm{Y} = \int \mathrm{T}\,d\,c + \iint \mathrm{U}\,d\,\varrho\,.\,d\,\theta,$$

where in the double integral we must integrate from $\varrho = 0$ to $\varrho = \varrho'$, and from $\theta = 0$ to $\theta = 2\,\pi$.

By conclusions similar to those in Art. 15, it is easy to perceive that the values of this expression differ by an infinitely small quantity, whether x be taken as $=0$, or as infinitely small; or, in other words, the value of Y has one and the same limit, with positive and with negative infinitely decreasing values of x, and this limit is no other than the value of the above formula, if x is made $= 0$. By analogy, we will denote this value by Y^0, in which, however, it must be remarked that we cannot call this THE value of $\int \frac{k\,b\,d\,s}{r^3}$ for $x = 0$ (inasmuch as this expression for $x = 0$ does not admit of a real integration), but only ONE value of that integral, namely, that value which is obtained if we integrate in the order which has been followed above.

This result, moreover, requires (as in Art. 16) a restriction in the particular case, in which the radius of curvature at the point P is infinitely small, and likewise if $\frac{d\,h}{d\,b}$ at this point is infinitely great; it is, however, unnecessary for our object to dwell on such exceptional cases, which can only occur in singular points or lines, and thus not in parts of the surface, but only at the limits of parts.

Lastly, it is self-evident, that it is in all respects with the quantity Z, or the integral $\int \frac{k\, c\, d\, s}{r^3}$, as with Y; that is to say, if in the first coordinate axis the point O approaches infinitely near to the point P, this integral has the same limiting value Z^0, whether the approximation be on the positive or on the negative side, and this limiting value is at the same time the value of $\int\int \frac{h\, c\, d c\, a b}{r^3}$ for $x = 0$, if we integrate first with respect to c.

18.

Now, if we remember that the quantities $\frac{d\,V}{d\,x}$, $\frac{d\,V}{d\,y}$, $\frac{d\,V}{d\,z}$ in all points of space not situated in the surface itself are unconditionally the same as X, Y, Z, and that V varies everywhere continuously, it is easy to deduce from the results obtained in the last article, that at an infinitely small distance from P, or for infinitely small values of x, y, z, the value of V is expressed, within infinitesimals of a higher order, by

$$V^0 + x\,(X^0 - 2\,\pi\,k^0) + y\,Y^0 + z\,Z^0$$

if x is positive, or by

$$V^0 + x\,(X^0 + 2\,\pi\,k^0) + y\,Y^0 + z\,Z^0$$

if x is negative, where V^0 denotes the value of V at the point P itself, or for $x = 0$, $y = 0$, $z = 0$. Thus, if we consider the values of V in a straight line drawn through P, making with the three coordinate axes the angles A, B, C, and denote by t an indefinite portion of this line, and by t^0 the value of t at the point P; then if $t - t^0$ be infinitely small, we shall have, within an infinitesimal of a higher order,

$$V = V^0 + (t - t^0)\,(X^0 \cos A + Y^0 \cos B + Z^0 \cos C \mp 2\,\pi\,k^0 \cos A),$$

the lower sign holding good for positive, and the upper one for negative values of $(t - t^0) \cos A$; or $\frac{d\,V}{d\,t}$ has at the point P two different values when A is acute, viz.—

$$X^0 \cos A + Y^0 \cos B + Z^0 \cos C - 2\,\pi\,k^0 \cos A, \text{ and}$$
$$X^0 \cos A + Y^0 \cos B + Z^0 \cos C + 2\,\pi\,k^0 \cos A,$$

according as $d\,t$ is considered positive or negative. For the case of A being a right angle, so that the straight line only touches the surface, the two expressions coincide, and we have

$$\frac{dV}{dt} = Y^0 \cos B + Z^0 \cos C$$

* * * *

As far as we have hitherto gone, the propositions contain no essential novelty; but it was necessary to state them afresh in connexion with, and preparatory to, the following investigation, in which a series of new theorems will be developed.

19.

Let V be the potential of a system of masses M', M'', M'''.... at the points P', P'', P'''....; v the potential of a second system of masses m', m'', m''' .... at the points p', p'', p''' .....; further, let V', V'', V''' be the values of V at the latter points, and v', v'', v''' the values of v at the points P', P'', P''' ...., we then have the equation

$$M' v' + M'' v'' + M''' v''' +, \&c. = m' V' + m'' V'' + m''' V''' +, \&c.,$$

which may also be expressed by $\Sigma M v = \Sigma m V$, if M represents generally each mass of the first, and m each mass of the second system. In fact $\Sigma M v$, as well as $\Sigma m V$, is no other than the aggregate of all the combinations $\frac{Mm}{\varrho}$, ϱ denoting the mutual distance of the points to which the masses M, m belong.

If the masses of one or both systems are distributed, not in discontinuous points, but continuously on lines, surfaces, or solids, the above equation retains its validity, if, instead of the sum, the corresponding integral is substituted.

If, for example, the second system of masses is distributed in a surface, so that the mass $k\,ds$ corresponds to the surface element ds, then $\Sigma M v = \int k V ds$; or, when the same is the case with the first system, so that the surface element dS contains the mass $K dS$, $\int K v dS = \int k V ds$. It is important, in relation to the latter case, to remark that this equation still holds good if the two surfaces coincide; for the sake of brevity, however, we will here only indicate the principal points of the mode in which this extension of the proposition can be rigorously justified. It is not difficult to show that these two integrals, in so far as they relate to one and the same surface, are the limiting values of those which relate to two separate surfaces, by causing their distance from each other to decrease infinitely; for which purpose it is only necessary to assume these two surfaces to be equal and parallel with each other. It is true that this kind of demonstration is immediately applicable only when the surface is such that the normals at all its points form acute angles with a determined straight line. A surface, in which this condition fails (as is always the case with a closed surface), must first be decomposed into two or more parts, which, taken singly, satisfy that condition, so that it is easy to reduce this case to the preceding one.

20.

If we apply the theorem of the preceding article to the case when the second system of masses, with a uniform density $k = 1$, is distributed over the surface of a sphere, the radius of which $= R$, then the potential v arising therefrom is constant within the sphere and $= 4\pi R$; at any point without the sphere, of which the distance from the centre is r, $v = \frac{4\pi R^2}{r}$, or it is just as great as the potential of a mass $4\pi R^2$, placed at that point, is at the centre; on the surface of the sphere the two values of v coincide. Thus, if the first system of masses be altogether within the sphere, then $\Sigma M v$ will be equal to the product of the whole mass of this system multiplied into $4\pi R$; but if this system of masses be altogether without the sphere, then $\Sigma M v$ will be equal to the product of the potential of this mass at the centre of the sphere multiplied into $4\pi R^2$; lastly, if the first system of masses is distributed continuously on the surface of the sphere, then for $\int k v\, ds$ the two expressions give the same result. Hence follows the

THEOREM.—If V denote the potential of a mass, howsoever distributed, in the element ds of the surface of a sphere described with the radius R, then if we integrate through the whole surface of the sphere

$$\int V\, ds = 4\pi\,(R\,M^0 + R^2\,V^0),$$

if we denote by M^0 the whole of the mass within the sphere, by V^0 the potential at the centre of the sphere of the external mass, and class at pleasure with the internal, or with the external masses, the masses which may be distributed continuously upon the surface of the sphere.

21.

THEOREM.—The potential V of masses, which are all situated externally in reference to a coherent space, cannot have a constant value in one part of this space, and a different value in another part of the same space.

Demonstration.—Let us assume, in every point of the space A, the potential constant to be $= a$, and in every point of another space B adjoining A, and not containing any mass, to be greater algebraically than a. Construct a sphere partly in B, and the remainder with the centre in A, which construction will always be possible. Now R being the radius of this sphere, and $d s$ an indeterminate element of its surface, then, according to the theorem in the preceding article, $\int V\, d s = 4 \pi R^2 a$, and $\int (V - a)\, d s = 0$, which is impossible, as for the part of the surface which is in A, $V - a = 0$, and for the remaining part is by hypothesis not $= 0$, but is positive.

In like manner the impossibility of V being less than a at all points of a space adjoining A will be clearly seen. But it is manifest that one at least of these two cases must exist if our theorem were false.

This theorem includes the two following propositions:—

I. If the space containing the masses include a space devoid of mass, and the potential in a part of this space have a constant value, this value will be the same for all points of the whole included space.

II. If the potential of the mass included in a finite space have a constant value in any part of external space, this value will be the same for all infinite space.

At the same time it is easy to perceive that in the second case the constant value of the potential can be no other than 0. For if we designate by M the aggregate of all the masses if they have all one sign, or if they have different signs, the aggregate, either of all the positive, or of all the negative masses, taken exclusively according as the first or the last-named preponderate, then the potential, at a point of which the distance from the nearest element $= r$, is always taken absolutely less than $\frac{M}{r}$, which fraction may manifestly be less in external space than any assignable quantity.

22.

THEOREM.—If ds be the element of a surface bounding a coherent finite space, P the force which masses, distributed in any manner, exert at ds in a direction normal to the surface, a force directed towards the interior or towards the exterior being considered as positive, according as attracting or repelling forces are deemed positive; then the integral $\int P\, ds$, extended through the whole surface $= 4\pi M + 2\pi M'$, M denoting the aggregate of the masses in the interior, and M′ that of those on the surface distributed continuously.

Demonstration.—If we denote by $U\, d\mu$ that part of P which is derived from the element of mass $d\mu$, by r the distance of the element $d\mu$ from ds, and by u the angle which the normal directed towards the interior makes with r at ds, then $U = \frac{\cos u}{r^2}$. But with respect to any given $d\mu$, by virtue of a theorem demonstrated in Art. VI. of the *Theoria Attractionis Corporum sphæroidicorum ellipticorum*, $\int \frac{\cos u}{r^2}\, ds = 0,\ 2\pi$, or 4π, according as $d\mu$ is without the space bounded by the surface, in the surface itself, or within the space in question. Now, as $\int P\, ds$ is equivalent to the amount of all the $d\mu . \int U\, ds$, our theorem follows immediately.

With respect to the auxiliary proposition which has been used here, it must be remarked, that in the form in which it has been enunciated in the work referred to, a modification is necessary for a particular case. The distance of a *given point* from the element ds being denoted by r, in case the point should be in the surface itself, the formula $\int \frac{\cos u}{r^2}\, ds = 2\pi$ is true only when the continuity of the curvature of the surface is uninterrupted at the point. But such an interruption occurs if the point be situated in an edge or in a cusp, and, in such case, for 2π we must substitute the area of the figure which the system of straight lines, touching the surface at the point in question, cuts off from a spherical surface described with radius $= 1$ about that point as centre.

But as such exceptional cases respect only lines or points, and thus not *parts* of the surface, but only boundaries between parts, it is evident that this has no influence on the use here made of the auxiliary proposition.

23.

Drawing through each point of the surface a normal, and denoting by p the distance of an indeterminate point upon it from the origin situated in the surface itself, the distance being considered as positive on the inner side of the surface, the potential V of the masses may be regarded as a function of p and of two other variable quantities, which in some way or other distinguish the several points of the surface from each other; and it is the same with the partial differential coefficient $\frac{dV}{dp}$, the value of which is to be taken into account here only for the points which lie in the surface itself, or for $p = 0$. As this has exactly the same signification as P, if there are masses distributed only in the internal space, or in external space, or in both, but none in the surface itself, then we have in this case

$$\int \frac{dV}{dp} \,.\, ds = 4\pi M.$$

In the contrary case of the whole mass being distributed solely in the surface itself, so that the element ds contains the mass $k\,ds$, $\frac{dV}{dp}$ and P have no longer the same signification; it is manifest that the latter quantity is here in relation to p what X^0 is in relation to x in Art. 15; $\frac{dV}{dp}$, on the other hand, has two different values, namely, $P - 2\pi k$, and $P + 2\pi k$, according as dp is regarded as positive or negative. Now, as $\int k\,ds$ is evidently equal to the whole mass M′ distributed over the surface, and agreeably to the theorem of the preceding article $\int P\,ds = 2\pi M'$, we have either

$$\int \frac{dV}{dp}\, ds = 0, \text{ or } \int \frac{dV}{dp}\, ds = 4\pi M',$$

according as we understand by $\frac{dV}{dp}$ the value which obtains everywhere without, or within the surface, and thus in the first case the integral is exactly the same as if the mass M′ belonged to the space without, and in the second case to the space within.

Hence, in any distribution of the masses, the equation $\int \frac{dV}{dp}\, ds = 4\pi M$ is universally true in the sense that M denotes the masses contained in the internal space, it being well understood that even if there be on the surface itself masses distributed continuously, they must be regarded as belonging to those in the interior, or excluded from them, according as the values which apply to external or to internal space have been chosen for $\frac{dV}{dp}$.

Consequently, if there are no masses in the internal space, then, if we understand in every case by $\frac{dV}{dp}$ the values belonging to the interior, $\int \frac{dV}{dp}\, ds = 0$.

24.

Under the same suppositions as those at the close of the last article, and denoting by T the space in question, and by q the whole resultant force at the element $d\,T$ of the masses, either outside the space, or distributed continuously in the surface, we have the following important

$$\text{THEOREM: } \int V \frac{dV}{dp} \,.\, ds = -\int q^2\, dT,$$

if the first integral be extended through the whole surface, and the second throughout the whole space T.

Demonstration.—Introducing rectangular coordinates x, y, z, let us consider in the first place a straight line, cutting the space T parallel to the axis of x, so that y and z have constant values. From the identical equation

$$\frac{d}{dx}\left(V \frac{dV}{dx}\right) = \left(\frac{dV}{dx}\right)^2 + V \frac{d^2 V}{dx^2},$$

it follows that the integral

$$\int \left(\left(\frac{dV}{dx}\right)^2 + V \frac{d^2 V}{dx^2}\right) dx,$$

being extended throughout that portion of the straight line which falls within T will be equal to the difference between the two values of $V \frac{dV}{dx}$ at the extreme points, inasmuch as the straight line cuts the bounding surface only twice; or, generally, it will be $= \Sigma\, \epsilon\, V \frac{dV}{dx}$, putting for $V \frac{dV}{dx}$ the respective values at the different points of intersection, and ϵ being $= -1$ for the uneven points of intersection (the first, third, &c.), and $= +1$ for the even ones. Further, if we consider along this straight line the prismatic space, of which the rectangle $dy\ dz$ is a section, $dx \cdot dy \cdot dz$ being an element of it, then the integral

$$\int \left(\left(\frac{dV}{dx} \right)^2 + V \frac{d^2 V}{dx^2} \right) dT$$

being extended throughout that part of T which falls within this prismatic space, $= \Sigma\, \epsilon\, V \frac{dV}{dx}, dy.dz$. This prism separates out of the bounding surface two, or, generally, an even number of portions, and if each be denoted by ds, and the angle between the axis of x and the normal to ds directed inwards by ξ, then $dy \cdot dz = \pm \cos \xi \cdot ds$, the sign $+$ being taken for the uneven, and $-$ for the even points of intersection. Consequently the above integral will be

$$= -\Sigma\, V \frac{dV}{dx} \cos \xi\, ds,$$

where the summation applies to all the elements of surface which are met. Now if the whole space T be entirely resolved into such prismatic elements, then all the corresponding parts of the surface will exhaust these completely, and we shall have

$$\int \left(\left(\frac{dV}{dx} \right)^2 + V \frac{d^2 V}{dx^2} \right) dT = -\int V \frac{dV}{dx} \cos \xi\, ds,$$

the first integration being extended throughout the whole space T, and the second over the whole surface. Now it is evident that $\cos \xi$ may be considered equal to the partial differential coefficients $\frac{dx}{dp}$, p having the signification established in Art. 23, and that x may be regarded as a function of p and two other variable quantities which distinguish the several points of the surface from each other, consequently

$$\int \left(\left(\frac{dV}{dx} \right)^2 + V \frac{d^2 V}{dx^2} \right) dT = -\int T \frac{dV}{dx}\ \frac{dx}{dp}\, ds.$$

It is, moreover, self-evident, that in the case of the surface itself containing masses, so that $\frac{dV}{dx}$ shall have two different values, we should always understand here the value belonging to the internal space.

By conclusions exactly similar we find

$$\int\left(\left(\frac{d\,V}{d\,y}\right)^2 + V\frac{d^2\,V}{d\,y^2}\right)d\,T = -\int V\frac{d\,V}{d\,y}\,\frac{d\,y}{d\,p}\,d\,s$$

$$\int\left(\left(\frac{d\,V}{d\,z}\right)^2 + V\frac{d^2\,V}{d\,z^2}\right)d\,T = -\int V\frac{d\,V}{d\,z}\,\frac{d\,z}{d\,p}\,d\,s.$$

If we add these three equations together, and remember that in the space T

$$\frac{d^2\,V}{d\,x^2} + \frac{d^2\,V}{d\,y^2} + \frac{d^2\,V}{d\,z^2} = 0$$

$$\left(\frac{d\,V}{d\,x}\right)^2 + \left(\frac{d\,V}{d\,y}\right)^2 + \left(\frac{d\,V}{d\,z}\right)^2 = q^2,$$

and at the surface

$$\frac{d\,V}{d\,x}\,\frac{d\,x}{d\,p} + \frac{d\,V}{d\,y}\,\frac{d\,y}{d\,p} + \frac{d\,V}{d\,z}\,\frac{d\,z}{d\,p} = \frac{d\,V}{d\,p},$$

we obtain $\int q^2\,d\,T = -\int V\frac{d\,V}{d\,p}\,d\,s$, which is our theorem; and by combining the last proposition of the preceding article, we may express it still more generally thus:

$$\int q^2\,d\,T = \int (A - V)\,\frac{d\,V}{d\,p}\,d\,s,$$

where A represents an arbitrary constant.

25.

THEOREM.—If upon the same suppositions as in the preceding article, the potential V have at all the points of the limiting surface of the space T the same value, this value will obtain also for all points of the space itself, and there is in the whole space a complete destruction of forces.

Demonstration.—If in the more general theorem of the preceding article, the constant limiting value of the potential be taken for A, it is evident that $\int q^2\,d\,T = 0$, so that necessarily $q = 0$ at every point of the space T, also $\frac{d\,V}{d\,x} = 0$, $\frac{d\,V}{d\,y} = 0$, $\frac{d\,V}{d\,z} = 0$, and consequently V is constant in the whole space T.

26.

THEOREM.—If the potential of masses which are distributed entirely within the finite space T, or which are also distributed either continuously, wholly, or partially over its surface S, have a constant value $= A$ at all points of S, then at every point O of external infinite space T' the potential will be,

1. If $A = 0$, likewise $= 0$.

2. If A is not $= 0$, it will be less than A, and will have the same sign as A.

Demonstration.—I. We must demonstrate that the potential can have no value at O beyond the limits 0 and A. Let us assume such a value B for the potential at O, and let us denote by C an arbitrary quantity between B and 0, and also between B and A. Straight lines being drawn in every direction from O there will be on each of them a point O', at which the potential will be $= C$, so that the whole line $O\,O'$ belongs to the space T'. This follows directly from the continuous variation of the potential, which, if the straight line be sufficiently prolonged, must either pass from B to A, or must decrease infinitely, according as the straight line meets or does not meet the surface S (compare the remark at the close of Art. 21). The locus of all the points O' forms then a closed surface; and as the potential is constant in it and $= C$, so, according to the theorem in the preceding article, it must have the same value at all points of the space enclosed by the surface, and yet it has at O the value B, which is different from C. Thus the assumption necessarily leads to a contradiction.

Thus, for the case of $A = 0$, our proposition is completely proved; for the second case of A not being $= 0$, so far is evident that the potential can at no point of T' be greater than A, or have a contrary sign.

II. To make our demonstration complete for the second case, let us describe round O, as a centre, a spherical surface, having a radius R less than the least distance of the point O from S; let the spherical surface so described be resolved into elements $d\,s$, and let the potential in each element be denoted by V; and the potential at O be again called B. According to the theorem in Art. 20, the integral extended over the whole spherical surface, will then be

$$\int V\,d\,s = 4\,\pi\,R^2\,B, \text{ and consequently } \int (V - B)\,d\,s = 0.$$

But this equality can only subsist, either if V is constant $= B$ at all points of the spherical surface, or if at different points of the spherical surface, V differs from B in opposite directions. In the first supposition, according to Art. 25, the potential would be constant in all the space within the sphere, and hence, according to Art. 21, it would also be constant in all the infinite space T^V, and in both $=0$, in contradiction to the supposition that at the limit of this space at the surface S it is different from 0, and the impossibility of its changing discontinuously from thence. The second supposition, on the other hand, would be in contradiction with that demonstrated under I., if B were either $=0$ or $=A$, thence B must necessarily fall *between* 0 and A.

27.

THEOREM.—In the theorem of the preceding article, the first case, or the zero value of the constant potential A, can only obtain if the sum of all the masses is itself $=0$, and the second only if this sum is not $=0$.

Demonstration.—Let ds be the element of surface of any sphere enclosing the space T, R its radius, M the sum of all the masses, and V their potential at ds. As according to the theorem in Art. 20 the integral $\int V\,ds = 4\pi R M$; but according to the theorem immediately preceding, in the first case, or for $A=0$, the potential V at all points of the spherical surface will be $=0$; and in the second case, on the other hand, it will be less than A, and will have the same sign; then in the first case $4\pi R M = 0$, so that $M=0$; in the second case $4\pi R M$ and therefore M likewise must have the same sign as A. It is evident that in the second case $4\pi R M$ will be less than $\int A\,ds$, or $4\pi R^2 A$, and M will be less than R A, or A will be greater than $\frac{M}{R}$.

The second part of this theorem, in combination with the one in the preceding article, may evidently be also expressed in the following manner:—

If the algebraical sum of masses, contained in a space bounded by a closed surface, or being also in part distributed continuously in the surface itself, be $=0$, and their potential have a constant value in all points of the surface, this value will necessarily be $=0$, and will at the same time apply to all external space, where consequently the action of the forces exerted by these masses completely destroy each other.

28.

It is easy to convince ourselves that all the conclusions of the two last articles preserve their validity if S is not a closed surface, and if the masses are entirely contained in it only. Here the space T no longer exists; all the points not in the surface itself belong to external infinite space; and if the potential have everywhere in the surface the constant value A, which differs from 0, it will have everywhere without the surface a smaller value with the same sign.

What refers to the first case, $A=0$, obtains here also, but has no import, as in this case the potential V becomes $=0$ in all points of space, so that $\frac{dV}{dt}$ is everywhere $=0$, t signifying any straight line, whence it may easily be inferred, according to Art. 18, that the density in the surface is everywhere $=0$, so that the surface cannot contain any masses.

Moreover, the above remark obtains generally, if the masses be contained only in the surface, even if it be a closed one, as it is evident that, according to the proposition in Art. 25, the value of the potential in this case will be $=0$ in the whole interior space also.

29.

Before we proceed to the remaining investigations, in which masses, distributed continuously throughout a surface, form a principal feature, an important existing difference in their distribution must be attended to, by admitting either only masses having the same sign (which, for brevity, we will always consider as positive), or masses having contrary signs also. Let a mass M be distributed over a surface in such manner, that to each element of surface ds the mass $m\,ds$ may correspond, m denoting, as heretofore, the density, and $\int m\,ds$ extended over the whole surface $=$ M; we shall call this a *homogeneous* distribution when m is everywhere positive, or at least nowhere negative; and a *heterogeneous* distribution, when m is positive in some places and negative in others; so that M will be only the algebraic sum of the molecules of mass, or the absolute difference of the positive and negative masses. A very special case of heterogeneous distribution is where $M=0$, when it may appear improper to speak of the mass 0 being distributed over the surface.

30.

It is self-evident, that however a mass M may be distributed homogeneously over a surface, the positive potential V everywhere resulting will, at every point of the surface, be greater than $\frac{M}{r}$, r denoting the greatest distance between any two points of the surface; the potential could only have this value at one extremity of the line r, if the whole mass were concentrated in the point at the other extremity, a case which cannot come into question here, as we are treating solely of continuous distribution, where to each element of surface $d\,s$ only an infinitely small mass $m d s$ corresponds. The integral $\int V\, m\, d\,s$, extended over the whole surface, is thus in every case greater than $\int \frac{M}{r}\, m\, d\,s$, or $\frac{M^2}{r}$, so that there must necessarily be one mode of homogeneous distribution for which that integral has a minimum value. It may here be premised, that one of the objects of the following investigations is to demonstrate that with such a distribution, in which $\int V\, m\, d\,s$ has its minimum value, the potential V will have one and the same value at every point of the surface, that no parts of the surface can remain vacant, and that there is only one such mode of distribution. For brevity, however, the investigation will be conducted from the commencement in a more comprehensive form.

31.

Let U denote a quantity, having at every point of the surface a given finite value varying continuously. The integral

$$\Omega = \int (V - 2\,U)\, m\, d\,s$$

extended over the whole surface may, it is true, have very unequal values, according to the diversity of the homogeneous distribution of the mass M; but it is evident that for one such mode of distribution, there must be a minimum value of this integral. We propose to demonstrate the

THEOREM, that for such a mode of distribution

1. The difference $V - U = W$ will have a constant value at every part of the surface which is occupied by parts of M.

2. That if parts of the surface are not so occupied, W must in those parts be greater, or at least cannot be less, than that constant value.

I. We must first demonstrate that if, instead of one mode of distribution, another differing infinitely little from it be taken, by substituting $m + \mu$ for m, the resulting variation of Ω will be expressed by $2\int \mathrm{W}\, \mu\, d s$.

In fact, if we denote by $\delta\,\Omega$ and $\delta\,\mathrm{V}$, the variations of Ω and V,

$$\delta\,\Omega = \int \delta\,\mathrm{V}\,.\,m\, d s + \int (\mathrm{V} - 2\,\mathrm{U})\, \mu\, d s.$$

But at the same time $\int \delta\,\mathrm{V}\,.\,m\, d s = \int \mathrm{V}\, \mu\, d s$, as may easily be seen from the theorem, Art. 19, $\delta\,\mathrm{V}$ being no other than the potential of that mode of distribution in which μ represents the density in each element of surface, so that what is here V, m, $\delta\,\mathrm{V}$, μ, may there be substituted for V, K, v, k, and $d\,s$ may be taken both for $d\,\mathrm{S}$ and $d\,s$. Consequently

$$\delta\,\Omega = \int (2\,\mathrm{V} - 2\,\mathrm{U})\, \mu\, d s = 2\int \mathrm{W}\, \mu\, d s.$$

II. The variations μ are obviously connected generally with a condition requiring that $\int \mu\, d s = 0$; but in the present investigation a second condition is also necessary; μ must not be negative in the unoccupied portions of the surface, if such exist, for otherwise the distribution would cease to be homogeneous.

III. Let us assume for a given distribution of M the existence of unequal values of the quantity W at different parts of the surface. Let A be a quantity lying between the unequal values of W; P the portion of the surface where the values of W are greater, and Q that where they are less than A; further, let p, q be equal portions of the surface, the first belonging to P, and the second to Q. This being supposed, let us give to the variation of m everywhere in p the constant negative value $\mu = -\nu$, everywhere in q the positive value $\mu = \nu$, and in all the remaining portions of the surface the value 0. It is plain that the first condition in II. will thus be satisfied; but the second condition will still require that p shall have no unoccupied portions, which may always be effected, providing only that the whole of the portion P be not unoccupied.

The result will be that $\delta\Omega$ receives a negative value, as may easily be seen by putting this variation into the form $2\int(\mathrm{W}-\mathrm{A})\,\mu\,d\,s$.

Hence it is evident, that if, with a given distribution,—either unequal values of W obtain in the occupied portions of the surface,—or, with existing equality of the values in the occupied portions, lesser values are met with in the unoccupied portions,—a diminution of Ω may be obtained by a change in the distribution, and that consequently with the minimum value the conditions enunciated in the above theorem must necessarily be fulfilled.

32.

If, for our special case (Art. 30), where $\mathrm{U}=0$, so that W denotes simply the potential of the mass distributed through the surface, and Ω the integral $\int \mathrm{V}\,m\,d\,s$, we combine the theorem of the preceding article with that given in Art. 28, it follows immediately, that with the minimum value of $\int \mathrm{V}\,m\,d\,s$, the surface can have no unoccupied portions whatever; for otherwise, even if the whole surface be a closed one, the occupied portion must be considered as an unclosed surface, and in respect of it the unoccupied portion must be regarded as belonging to external space; and in it, according to Art. 28, the potential must have a less value than in the occupied surface, whereas the theorem of the preceding article excludes a less value.

It is thus demonstrated that there is a homogeneous distribution of a given mass over the whole surface, in which no portion remains vacant, and from which there results an equal potential in all points of the surface. There is yet wanting to complete the demonstration of the theorem in Art. 30, to show that there can be only one mode of distribution which can satisfy these conditions; the proof of this will appear in the sequel, as part of a more general theorem.

That the minimum value of $\int \mathrm{V}\,m\,d\,s$ requires that there shall be no part of the surface unoccupied, may obviously be also expressed thus: for any distribution, in which a finite portion of the surface remains vacant, the integral $\int \mathrm{V}\,m\,d\,s$ receives a value which exceeds the minimum value by a finite difference.

33.

The leading feature in the mode of demonstration developed in the 31st Article, rests on the immediate recognition of the existence of a minimum value for Ω, so long as we restrict ourselves to the homogeneous distributions of a given mass. If it were equally evident without this restriction, the conclusions in Art. 31 would at once lead to the result, *that there is always, if not a homogeneous, yet a heterogeneous distribution of the given mass, for which* $W = V - U$ *has at all points of the surface an equal value,* as then the second condition (in Art. 31, II.) drops. But as the self-evidence is lost as soon as we dispense with the restriction to homogeneous distribution, we are compelled to seek for the rigorous demonstration of this, the most important proposition of our whole investigation, by a somewhat more artificial path: and the following appears the most simple which will conduct us to the end.

Let us consider three different distributions of mass, in which, instead of the indefinite signs m for the density and V for the potential, we employ the following:

$$\begin{array}{rll} \text{I.} & m = m^0, & V = V^0 \\ \text{II.} & m = m', & V = V' \\ \text{III.} & m = \mu & V = v. \end{array}$$

I. is that homogeneous distribution of the positive mass M, for which $\int V\, m\, d\, s$ has a minimum value.

II. is that homogeneous distribution of the same mass M, for which $\int (V - 2\, \epsilon\, U)\, m\, d\, s$ has a minimum value, ϵ signifying an indeterminate constant coefficient.

III. depends on I. and II., by making $\mu = \dfrac{m' - m^0}{\epsilon}$, so that it is a heterogeneous distribution in which the total mass $= 0$.

In Art. 31, V^0 was shown to be constant in the whole surface; $V' - \epsilon\, U$ in the surface, so far as it is occupied in distribution II., and thence in the same portion of the surface $v - U$ because $v = \dfrac{V' - V^0}{\epsilon}$.

Whether in the second distribution the whole surface be occupied, or whether a greater or less portion remains unoccupied, will depend on the coefficient ϵ. As the second distribution passes into the first if $\epsilon = 0$, so, generally, the portion of the surface remaining unoccupied for a determinate value of ε, will diminish as ε decreases, and will be quite filled up before $\varepsilon = 0$. In particular cases a portion may remain unoccupied so long as ε differs from 0, and has not changed its sign. It is sufficient for our purpose to take ε as infinitely small; when it is easy to show that in any case no finite portion of surface can remain unoccupied; for otherwise, according to the concluding remark in Art. 32, the integral $\int V' m' ds$ must exceed the integral $\int V^0 m^0 ds$ by a finite difference; if this be denoted by e, then the difference of the two integrals is

$$\int (V' - 2\varepsilon U)\, m' ds - \int (V^0 - 2\varepsilon U)\, m^0 ds = e$$
$$- 2\varepsilon \int U\, (m' - m^0)\, ds,$$

which for an infinitely small ε preserves a positive value, in contradiction to the supposition that $\int (V - 2\varepsilon U)\, m\, ds$ has its minimum value in distribution II.

Hence, we conclude, that if in the third distribution we take for μ the limiting value of $\frac{m' - m^0}{\varepsilon}$ with an infinite decrease of ε, then $v - U$ has a constant value in the whole surface.

If now we imagine a fourth distribution in which $m = m^0 + \mu$, so that the whole mass $= M$, the resulting potential will be $= V^0 + v$; so that in the whole surface it will exceed U by the constant difference $V^0 + v - U$, whereby the theorem enunciated above is demonstrated.

34.

We have still to demonstrate that there is only one mode of distribution possible with a given mass M, in which $V - U$ shall be constant throughout the whole surface. If there were two modes of distribution fulfilling this condition, then m and v being denoted by m' and v' in the first, and by m'' and v'' in the second mode of distribution, then in a third mode, in which m is taken $= m' - m''$, the potential would be $= V' - V''$, and consequently constant; and the total mass would be $= 0$. According to Art. 27, the potential must then necessarily $= 0$; and therefore, according to Art. 28, $m' - m''$ would also $= 0$, or the two distributions would be identical.

Lastly, it must be noticed that there is always a distribution of mass in which the difference $V - U$ has a *given* constant value. Let α denote an indefinite constant coefficient; preserving for the first and third distribution the notation of the preceding article, the potential of that distribution in which $m = \alpha\, m^0 + \mu$ will be $= \alpha\, V^0 + v$, and the constant difference $\alpha\, V^0 + v - U$ may be given any desired value by a suitable determination of the coefficient α. The total mass in this distribution is, then, no longer arbitrary, but $= \alpha\, M$. It is evident, as before, that this condition of distribution also can be fulfilled in only one manner.

35.

The actual determination of the distribution of the mass on a given surface for each assigned form of U surpasses in most cases the power of analysis in its present state. The simplest case, where it is within our power, is that of the whole surface of sphere; but we will forthwith treat the more general case, in which the surface deviates very little from that of a sphere, and quantities of a higher order than the difference itself may be neglected.

Let R be the radius of the sphere, r the distance of any point in space from its centre, u the angle between r and a fixed straight line, λ the angle between the plane passing through this straight line and r, and a fixed plane. Let the distance of an indefinite point in the given closed surface, from the centre of the sphere be $= R\,(1 + \gamma\, z)$, where γ is a very small constant factor, whose higher powers may be neglected, z as well as U being functions of u and λ.

The potential V of the mass distributed through the surface will be expressed at any point of external space by a series descending according to the powers of r, to which we give the form

$$A^0 \frac{R}{r} + A' \left(\frac{R}{r}\right)^2 + A'' \left(\frac{R}{r}\right)^3 + \&c.$$

At any point of the internal space, on the other hand, by the ascending series

$$B^0 + B' \frac{r}{R} + B'' \left(\frac{r}{R}\right)^2 + B''' \left(\frac{r}{R}\right)^3 + \&c.$$

The coefficients A^0, A', A'', &c. are functions of u and λ, which satisfy known partial differential equations. (*Resultate*, 1838, p. 22.)* The same is the case with B^0, B', B'', &c. On the given surface the potential must be equal to a given function of u and λ, namely, $V = U$, thus

$$\left(\frac{r}{R}\right)^{\frac{1}{2}} V = (1 + \gamma z)^{\frac{1}{2}} U.$$

Now if we assume $(1 + \gamma z)^{\frac{1}{2}} U$ to be developed in a series

$$P^0 + P' + P'' + P''' + \&c.,$$

in such manner that the several members P^0, P', P'', P''', &c. may likewise satisfy the differential equations in question; and if we bear in mind that both the above series for the potential remain valid up to the surface, it is evident that

$$P^0 + P' + P'' + P''' + \&c.$$
$$= A^0 (1 + \gamma z)^{-\frac{1}{2}} + A' (1 + \gamma z)^{-\frac{3}{2}} + A'' (1 + \gamma z)^{-\frac{5}{2}} + \&c.$$
$$= B^0 (1 + \gamma z)^{-\frac{1}{2}} + B' (1 + \gamma z)^{\frac{1}{2}} + B'' (1 + \gamma z)^{\frac{3}{2}} + \&c.$$

We conclude hence, that if quantities of the order γ be neglected, $P^0 + P' + P'' + \&c. = A^0 + A' + A'' + \&c.$; and thus (as a function of u, λ can only be developed in one series, whose members satisfy the above-mentioned differential equations) $P^0 = A^0$, $P' = A'$, $P'' = A''$, &c. In like manner quantities of the order γ being neglected, $P^0 = B^0$, $P' = B'$, $P'' = B''$, &c.

Thus if we write (I.),

$$\begin{aligned} A^0 &= P^0 + \gamma a^0, & B^0 &= P^0 - \gamma b^0 \\ A' &= P' + \gamma a', & B' &= P' - \gamma b' \\ A'' &= P'' + \gamma a'', & B'' &= P'' - \gamma b'' \\ A''' &= P''' + \gamma a''', & B''' &= P''' - \gamma b''', \ \&c., \end{aligned}$$

where it is obvious that a^0, a', a'', a''', &c., and likewise b^0, b', b'', b''', &c., will satisfy the differential equations in question; and if we substitute these values in the above equations, neglecting in so doing quantities of the order γ^2, then after dividing by γ, we shall have within errors of the order γ,

$$a^0 + a' + a'' + a''' + \&c. = \tfrac{1}{2} z (P^0 + 3 P' + 5 P'' + 7 P''' + \&c.)$$
$$b^0 + b' + b'' + b''' + \&c. = \tfrac{1}{2} z (P^0 + 3 P' + 5 P'' + 7 P''' + \&c.)$$

Therefore within errors of the order γ,

$$b^0 = a^0, b' = a', b'' = a'', \&c.$$

and consequently, within errors of the order γ^2, (II.)

$$B^0 = P^0 - \gamma a^0, B' = P - \gamma a', B'' = P'' - \gamma a'', \&c.$$

* Scientific Memoirs, vol. ii. p. 203.

The differential coefficient $\frac{d\,\mathrm{V}}{d\,r}$ has at the surface itself two different values, and the value which applies to a negative $d\,r$, or to the inner side, surpasses that which applies to the outer side by $4\,\pi\,m \cos\theta$, m denoting the density at the place of intersection, and θ the angle between r and the normal. (Art. 13, where t, A, k^0 correspond to r, θ, m in the present article.) These two values are found by differentiating with respect to r the two expressions for V which apply to the internal and external space, and then putting $r = \mathrm{R}\,(1 + \gamma z)$. The first is

$$\frac{1}{\mathrm{R}}\,(\mathrm{B}' + 2\,\mathrm{B}''\,(1+\gamma z) + 3\,\mathrm{B}'''\,(1+\gamma z)^2 +, \&c.),$$

and the second

$$-\frac{1}{\mathrm{R}}\,(\mathrm{A}^0\,(1+\gamma z)^{-2} + \mathrm{A}'\,(1+\gamma z)^{-3} + \mathrm{A}''\,(1+\gamma z)^{-4} + \&c.)$$

Therefore, if we multiply the difference by $\mathrm{R}\,(1+\gamma z)^{\frac{3}{2}}$, we have $4\,\pi\,m\,\mathrm{R}\cos\theta\,.\,(1+\gamma z)^{\frac{3}{2}} =$

$$\mathrm{A}^0\,(1+\gamma z)^{-\frac{1}{2}} + \mathrm{A}'\,(1+\gamma z)^{-\frac{3}{2}} + \mathrm{A}''\,(1+\gamma z)^{-\frac{5}{2}} + \&c.$$
$$+\,\mathrm{B}'\,(1+\gamma z)^{\frac{3}{2}} + 2\,\mathrm{B}''\,(1+\gamma z)^{\frac{5}{2}} + 3\,\mathrm{B}'''\,(1+\gamma z)^{\frac{7}{2}} + \&c.$$

If we substitute for A^0 A', &c., the values from I., and for B^0, B, &c., the values from II., and if we neglect quantities of the order γ^2, we obtain

$$4\,\pi\,m\,\mathrm{R}\cos\theta\,.\,(1+\gamma z)^{\frac{3}{2}} = \mathrm{P}^0 + 3\,\mathrm{P}' + 5\,\mathrm{P}'' + 7\,\mathrm{P}''' +, \&c.$$
$$+\,\gamma\,(a^0 + a' + a'' + a''' +, \&c.)$$
$$-\,\tfrac{1}{2}\,\gamma z\,(\mathrm{P}^0 + 3\,\mathrm{P}' + 5\,\mathrm{P}'' +, \&c.);$$

consequently, as the two last series destroy each other within quantities of the order γ^2,

$$m = \frac{(1+\gamma z)^{-\frac{3}{2}}}{4\,\pi\,\mathrm{R}\cos\theta}\,.\,(\mathrm{P}^0 + 3\,\mathrm{P}' + 5\,\mathrm{P}''' + 7\,\mathrm{P}''' + \&c.),$$

whereby the problem is solved. Instead of $(1+\gamma z)^{-\frac{3}{2}}$, we may write $1 - \frac{3}{2}\,\gamma z$, and omit the divisor $\cos\theta$, since, generally speaking, θ is of the order γ, so that $\cos\theta$ differs from 1 only by a quantity of the order γ^2.

For the case of a sphere, where $\gamma = 0$, we have rigorously

$$m = \frac{1}{4\,\pi\,\mathrm{R}}\,(\mathrm{P}^0 + 3\,\mathrm{P}' + 5\,\mathrm{P}'' + 7\,\mathrm{P}''' + \&c.,)$$

as $\mathrm{P}^0 + \mathrm{P}' + \mathrm{P}'' + \mathrm{P}''' + \&c.$ is the development of U itself.

36.

In the investigation hitherto the quantity U has been left indefinite; its application to the case, for which the potential of a given mass has been taken for U, prepares the way for the following important

THEOREM.—For a given arbitrary distribution of mass D, which is restricted either entirely to the internal space, bounded by the closed surface S, or entirely to external space, we may substitute a distribution E solely on the surface itself, with this result, that the effect of E will be equal to the effect of D, at all points of external space for the first case, and at all points of the internal space for the second case.

For this it is only requisite that as the potential of D is denoted at any point of S by U, and the potential of E by V, $V-U$ be $=0$ through the whole surface for the first case, and that it be constant only for the second case. Of course $-U$ will be the potential of a distribution D′, which is opposed to D (so that each molecule of mass is replaced by a contrary one), and thus $V-U$ becomes the potential of the distribution D′ and E subsisting at once; their effects will consequently destroy each other in the first case, in all external space; in the second, in the whole internal space (Articles 27 and 25), or the effect of D and E will be equal in the spaces in question. Moreover, the whole mass in E will, for the first case, be equal to the mass in D, but for the second case will remain arbitrary.

The theorem, which was announced in the *Intensitas vis Magneticæ*, p. 10, and which was referred to in different parts of the *Allgemeine Theorie des Erdmagnetismus*, now appears as a particular case of what has been here demonstrated.

37.

Although, as has been already noticed in Art. 35, the actual complete deduction of the distribution E presents, in most cases, insuperable difficulties, yet there is one case in which it can be effected with more facility, which deserves to be particularly mentioned. It is when U is constant, so that S is a surface of equilibrium for the given system of mass D. It is easy to perceive that we need here only speak of the case in which D is taken in the internal space, and the total mass is not $=0$, as otherwise there would be no effect at all there to be represented by a distribution of mass through S.

Let O be a point of the surface S, and r a straight line cutting the surface at O at right angles, and which is to be regarded increasing in the outward direction; further, let $-$C be the value of the differential coefficient $\frac{d\,U}{d\,r}$ at O, and m the density of the distribution of mass E at O. The differential coefficients $\frac{d\,V}{d\,r}$ will have two different values at O; the one relating to the exterior, will be equal to the differential coefficient $\frac{d\,U}{d\,r}$, or $= -$C, because $V = U$ in the surface, and in all external space; and the one relating to the interior will be $= 0$, because V is constant in the surface and in the whole internal space. But as the second value is greater than the first by $4\,\pi\,m$, we have $4\,\pi\,m = C$, or $m = \frac{C}{4\,\pi}$. It is obvious that C is no other than the force resulting from the distribution of mass D, and that it has the same sign as the total mass.

GAUSS.

Article VIII.

On the Law of Storms. *By* H. W. Dove.

[From Poggendorff's *Annalen der Physik und Chemie*, 1841.]

THAT a considerable decrease of atmospheric pressure should be an effect of any unusual disturbance of the atmosphere is a supposition so natural, that it at once occurred to those who first remarked that the weight of air surrounding us is not always the same. For the purpose of measuring these changes, Otto von Guerike attached a scale to the water barometer which he had invented, and in the 21st chapter of the *Mirabilia Magdeburgica*, in Schott's *Technica Curiosa*, he records the following remarkable observation:—"In the year 1660 the air was once so uncommonly light, that the index pointed below the lowest mark on the glass tube; on seeing this, I said to the persons who were present, that doubtless there was a great storm somewhere; two hours afterwards the tempest was raging in our district, though with less violence than it had done over the ocean." To mention only one more recent example, I may recall the storm of the 17th January 1818, of which the ravages are still visible in the forests of Prussian Lithuania, after the lapse of nearly a quarter of a century. This storm extended from the coasts of England to Memel, and was felt throughout a region 240 German miles in length, and forty-one German miles in breadth. On the 18th of January the barometer fell at Königsberg eight lines in eight hours. The whole fall between the 3rd and the 17th of January was twenty-one lines. In Edinburgh the fall of the barometer and the violence of the storm were also both remarkable.

The experience of the last two centuries has so far confirmed the remark of Otto von Guerike, that the scales attached to our common barometers usually terminate with "very stormy." But its applicability is not confined to the temperate zone: in lat. 70° N., long. 70° W., the warning afforded by a fall of $9'''.29$ in the marine barometer, enabled Scoresby to avoid the dangers of a tempest which lasted two days uninterruptedly, and he consequently strongly recommends the use of these instruments to whalers when in high latitudes. In the regions of the trade winds, and of the monsoons, the numerous examples of greatly diminished pressure ushering in the typhoons and West India hurricanes are well known.

[M. Dove then proceeds to recount several such instances. In 1837 the Harbour Master at Porto Rico warned the shipping in the port to prepare against a storm, as the barometer was falling in an unusual manner; at 8 P.M. the preceding evening it had stood at $333'''\cdot28$, and had sunk to $315'''\cdot27$. The precautions taken were unavailing; thirty-three vessels at anchor were all destroyed, and at St. Bartholomew alone 250 buildings were overthrown. At the same time the barometer fell at St. Thomas from $337'''$ to $316'''$, and the ravages of the hurricane were even greater than at Porto Rico. He describes the violent effects of the wind on this and other occasions, in the destruction of vessels in harbour, forts, houses, and larger buildings; in dragging large guns (twenty-four pounders) along the ground; driving boards through trees and walls several inches in thickness, &c. &c. He refers also to General Baudrant's account of the destruction of Basse Terre in Guadaloupe by a hurricane in 1825; and to hurricanes in 1828 and 1836 in the island of Mauritius, where in 1828 the barometer fell to $316'''$, and in 1836, having stood at $337'''\cdot00$ at 5 A.M. on the 6th, it had fallen to $317'''\cdot85$ at 8 A.M. on the 8th. M. Dove says further in a note, "A striking instance of the great mechanical power, even of smaller hurricanes, occurred near Calcutta in April 1833, when a revolving storm, not above half an English mile in breadth, passed between Calcutta and the great salt water lake three miles to the east of that city, and in the space of four hours, on a track of sixteen miles in length, caused the death of 215 human beings, and injured 223. It overthrew 1239 fishermen's huts; a bamboo was driven quite through a wall of five feet thick, piercing the covering of masonry on both sides, so that the Editor of the Indian Review says a six-pounder would scarcely have had the same effect."]

If two phænomena frequently occur together, we may surmise, with some degree of probability, that they have a causal connexion; but it may remain quite undecided which is the conditional, and which the contingent phænomenon; or both may be effects of a third phænomenon, which is itself their common cause. Further, if one of the phænomena be really an immediate consequence of the other, we may not be able to infer with certainty that the same effect might not have been produced in some other way.

If barometric minima almost always occur when the atmosphere is agitated by tempests, on the other hand we frequently see a very low barometer, when mild vernal breezes interrupt the severe cold of winter and appear to introduce the temperature of a more genial season. As it seemed however difficult to believe that such gentle winds could cause any considerable disturbance in the equilibrium of the atmosphere, the great diminution of pressure on such occasions has been attributed to other causes. The idea that the convulsions of the surface in earthquakes could not be unconnected with the atmosphere, was one of such natural occurrence, that the barometer was always looked to in the expectation of its indicating these phænomena at great distances. This idea appeared to be confirmed, when, four days after the destruction of Messina in 1783, the barometer in Europe fell unusually low. Van Swinden accordingly inferred a connexion between the two phænomena; but on a comparison of the meteorological observations made at the time, and recorded in the Manheim *Ephemerides*, Brande found that on the 9th of February the barometer fell below its average height by 14 lines in Lyndon, in Rutlandshire; $13\frac{1}{2}$ in Amsterdam and Franeker; $12\frac{3}{4}$ in Dunkirk; $12\frac{1}{2}$ in Middelburg; $12\frac{1}{4}$ in Paris; $11\frac{1}{2}$ in Laon, Nantes, and Cambray; $10\frac{1}{2}$ in Brussels, Chartres, Poictiers, and Rochelle; 10 in Troyes and Montmorenci; 9 at Göttingen, Mayence, Metz, Limoges, and Bordeaux; 8 at Copenhagen, Erfurt, Würzburg, Lyons, Mezier in Guyenne, and Oleron; 7 at Spydberga in Norway, Stockholm, Berlin, Vienna, Manheim, Geneva, and Vienne; 6 at Sagan, Prague, Regensburg, on the St. Gothard, and at Montpelier; 5 at Marseilles and Montlouis; 4 at Ofen and Padua; 3 at Petersburg, Mafra, Bologna, and Rome. Thus it appeared that the barometer was lowest in England and Holland, and that in approaching Italy it differed less and less from its mean height, so that the independence of the two phænomena became highly probable.

If, as in this instance, such simultaneous observations sometimes serve to show, that what had been regarded as evidence of essential connexion was merely an accidental coincidence between two independent phænomena, we have much reason to hope that a careful examination of such observations may lead also to the actual discovery of the true causes of the phænomena. On Christmas eve 1821, after a long continuance of stormy weather, the barometer sank so low in Europe, that the attention of all meteorologists was strongly drawn to the circumstance. Brande requested, in the scientific journals, that all the observations made at that time might be sent to him, and published his conclusions from their intercomparison in his *Dissertatio physica de repentinis variationibus in pressione Atmosphæræ observatis*, 1826. The conclusion he arrived at was, that some unknown cause of diminished pressure was moving over the surface, and that the air flowed in on all sides towards that part; therefore, that the storm so produced was *centripetal* (*vergere procellarum directionem ad idem illud centrum*), arising from the tendency of the surrounding air to restore the equilibrium deranged at any particular part.

Brande had previously tried to support the same view by an examination of some analogous barometric minima in his 'History of the Weather in 1783,' published in 1820; but it is remarkable how little the observations adduced by him correspond to that view. In the storm which on the night of the 11th and 12th of March, according to Toaldo, advanced from Naples to Venice in three hours, or 140 feet in a second of time the distance being 276 Italian miles, it appears so little probable that this was a flowing in towards Switzerland, which was the centre of least pressure, that Brande himself is forced to suppose that the current of air flowing with extraordinary force towards Venice, had produced a kind of enormous whirlwind, causing the air to flow from Marseilles to Corsica, in order, adds he, "then to join the great current." When he says further on, "but these are only conjectures; it is certain, however, that as the wind was east at Copenhagen, and south-east at Ofen, there is a flowing in almost completely round the circumference," (for which, however, we have only the evidence of the North in Berlin,) we might with more reason regard the directions named as tangents to circles round that centre rather than as radii.

According to the view which I had taken, that the mean atmospheric variations are produced by the conflict of two currents above the place of observation, it necessarily follows that the absolute extremes of these variations must arise from the exclusive prevalence of one of the two currents over the other. Thus a barometric minimum would be a phænomenon of the *south* current; when viewed as occurring simultaneously at several places, the south current itself; when viewed locally, a stormy passage through the minimum of the wind circle; or comprehending both views, a whirl or whirlwind advancing in the direction of the south current, *i. e.* from S.W. to N.E. In confirmation of this view, I subjected the observations collected by Brande and others to a new examination, and in a treatise, entitled 'On Barometric Minima,' which appeared in Poggendorff's 'Annals' in 1828, vol. xiii. p. 596, I pointed out, that a simple explanation of all the phænomena could be afforded, on the assumption of one or more great rotatory currents, or whirlwinds, advancing from S.W. to N.E.; and I remarked at the same time, that in all the hurricanes of the southern hemisphere which I had examined, the rotatory movement was in the opposite sense to that which took place in the northern hemisphere. As the example discussed in that paper contained a complete refutation of the idea of a flowing in towards a centre, I will here repeat the principal quantitative determinations.

On the 24th December, 1821, at 6 P.M., the barometer stood below its average height, as follows:—22 lines at Brest; 19 at Helston and Nantes; 17 at Gosport; 16½ at Dieppe; 15 in London, Haarlem and Paris; 11 at Strasburg, Geneva and Bremen; 10 at Zurich, Göttingen and Bergen; 9 at Joyeuse and Augsburg; 8½ at Wurzburg; 8 at Regensburg and Leipsic; 7 at Prague, Breslau and Christiania; 6½ at Cracow, Apenrade and Abo; 5 at Turin and Modena; 3½ at Florence; 3 at Tilsit and Petersburg; 1½ at Rome; 1 at Molfetta.

On the 25th December, at 3 A.M.—22 lines in London; 21½ at Dieppe; 20 at Gosport and Boston; 19 at Helston; 18½ at Paris; 18 at Haarlem; 18½ at Kinfauns Castle; 16½ at Strasburg; 15 at Heidelberg; 14 at Cologne, Regensburg and Göttingen; 13 at Geneva, Zurich, Augsburg, Berlin and Bergen; 12½ at Joyeuse; 12 at Regensburg, Gotha and Leipsic; 11 at Prague and Breslau; 9 at Turin; 8 at Milan and Cracow; 7½ at Christiania; 6 at Abo; 5 at Florence, Rome and Tilsit; 3 at Molfetta; 2½ at Petersburg.

On the 25th December, at 10 A.M.—23 lines at Middelburg; 21 at Gosport; 20½ at Haarlem; 18 at London; 17 at Helston; 16 at Dieppe, Göttingen and Bremen; 15 in Paris, Strasburg and Bergen; 14 at Heidelberg, Gotha and Leipsic; 13 at Zurich, Augsburg, Vienna, Prague and Breslau; 12½ at Joyeuse and Inspruck; $11\frac{1}{2}$ at Cracow and Dantzic; 11 at Padua; $9\frac{1}{2}$ at Christiania; 8 at Florence; 7 at Tilsit; 6 at Rome, Molfetta and Abo; 3 at Petersburg.

On the 25th December, at 8 P.M.—17 lines at London; $16\frac{1}{2}$ at Helston and Apenrade; 16 at Haarlem and Bergen; 15 at Bremen; 14 at Dieppe, Göttingen and Dantzic; 13 at Paris, Gotha, Breslau and Christiania; 12 at Strasburg, Berlin and Cracow; 11 at Turin, Zurich and Augsburg; $10\frac{1}{2}$ at Padua; 10 at Prague; 9 at Tilsit; 8 at Florence; 7 at Molfetta; 4 at Petersburg.

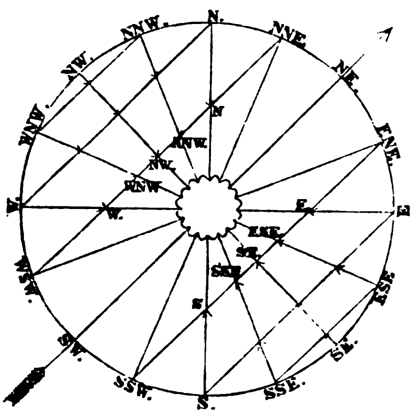
According to the one view,—in which it is considered that the atmospheric pressure at a given place being from some cause unusually diminished, an inflowing takes place from all sides,—there will be equilibrium between the several particles on a line in which the pressure is equally diminished, and the general direction of the wind will be perpendicular to that line. According to the other view,—in which the complex phænomenon is regarded as the consequence of a rotatory movement,—the general direction of the wind will be that of the above-named line itself. Thus the two assumptions lead to two directions of the wind at right angles to each other. We have therefore next to inquire to which assumption the observations correspond.

From the abovementioned observations, it follows that the march of the minimum was from the French coast towards the south-west point of Norway, or nearly from Brest to Cape Lindenaes. What was the direction of the wind at the different stations in reference to this moving minimum? Was the direction *towards* the minimum? or was it tangential to circles having the place of minimum as their common but constantly moving centre? This may be directly tested in the most simple manner, by laying down on four maps the place of the minimum for the four epochs, 6 P.M. 24th December, and 3 A.M., 10 A.M., and 8 P.M. 25th December, and then marking on these maps the directions of the wind simultaneously observed at the several stations. If the arrows on the maps are found to be tangents to concentric circles, the actual existence of these circles may be assumed, and the directions of the wind which follow from such a supposition may be compared with the observations.

As the march of the minimum is from Brest to Cape Lindenaes, France, Italy, Germany, Denmark and Russia are on the south-eastern side of the main path of the storm, Ireland, Scotland and Iceland are on the north-western side, and England is nearly in the middle of its course.

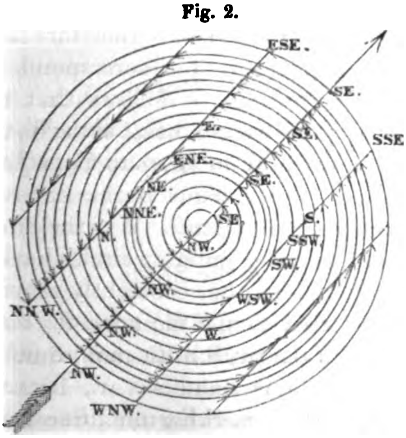
On the supposition of a centripetal inflowing according to Brande's view, as the phænomenon passes over a station on the south-eastern side, a wind vane at that station should be seen to pass successively from E.N.E. through E., E.S.E., S.E., S.S.E. and S., to S.S.W.; and at a station on the north-western side the vane should pass from N.N.E. through N., N.N.W., N.W., W. N.W. and W., to W.S.W. (Fig. 1.)

Fig. 1.



If, on the other hand, the storm is rotatory and turning in the opposite direction to that of the hands of a watch, the wind at a south-eastern station will pass from S.S.E. through S., S.S.W., S.W., W.S.W., and W., to W.N.W.; and at a station on the north-western side, from E.S.E. through E., E.N.E., N.E., N.N.E., N. and N.N.W. (Fig. 2.) At places situated in the middle of the phænomenon the wind should change suddenly, according to the first view, from N.E. to S.W.; and, according to the second view, from S.E. to N.W.

Fig. 2.



Both suppositions agree in leading us to expect a rotation *with* the sun on the south-eastern side of the storm, and *against* the sun on the north-western side; and in the middle a calm, dividing winds blowing in opposite directions; but they disagree in making a difference of 90° in the two opposite positions of the wind vane at the commencement and at the termination of the rotation.

The observations in the memoir referred to are decidedly in favour of the second view, and adverse to the first; for the rotation of the vane never begins with N.N.E., N., or N.N.W., and ceases with W.S.W., W., or W.N.W., but always begins with E.S.E. and S.E., and ceases with S.W. and W. It was so in Germany, Italy, Denmark and Russia. In England the wind before the minimum was east, not north; in France it was principally S.W., and in Iceland it was first N.E. and afterwards N., just as the rotatory movement would require at a point so distant from the centre.

[In a map accompanying the original memoir, M. Dove has marked the successive circles of the rotatory storm as it advanced from the beginning to the end of the observations, in order to facilitate the comparison of the observations with the theoretical assumption. Where the advancing circles are stopped by the Spanish mountains and the Maritime Alps, those points are assumed as centres of new circles or whirlwinds.]

The observations give—

1. On the North-west side of the Storm.

Naes in Iceland, N.E., N.

2. Nearly in the middle of the Storm.

Helston, E., min., W.
London, S.E., min., N.W.
Owen's Row at Islington, S.E., min., N.W.
Cambridge, S.E., min., W.
New Malton, southerly storm.

3. On the South-east side of the Storm.

Boulogne-sur-Mer, S.S.E., S., E.N.E., min., W.N.W.
Paris, S., min., W.S.W.
Joyeuse, southerly storm, min.
Nismes, S., min., S.W., N.W.
Vivarais, S.E., min., S.E.
Strasburg, S.E., E., S., min.
Haarlem, S.E., E.S.E., S.S.E., min., S.S.W., S.W.
Schwelm, S., min., S.W.
Cologne, S.S.E., S.E., min., S., W.S.W., S.W.
Coblentz, S.W., min., S., S.W.
Salzuflen, S.E., min., S.
Wetzlar, S.S.E., min., S.S.W., S.W.
Minden, S.E., min., S.
Carlsruhe, S., min., S.W.
Göttingen, S.E., S.S.E., min., S.W.
Regensburg, E., S.E., min.
Augsburg, S.W., min., W.
Quedlinburg, E., min., S.W.
Zellerfeld, S., min., W.
Leipsic, S.W., min., S.

Zschoppau, min., at S.W.
Annaberg, S.E., min., S.W., W.
Prague, W., min., S.W., W.
Breslau, S.W., min., S.
} Disturbing influence of the mountains to the S.
Leobschutz, S.
Dantzic, S., min., S.

Konigsberg, S.E., min., W.
Tilsit, S.W., S.E., min., W.
Petersburg, S.E., E., S.E., S.S.E., min.
Geneva, S.E., min.
Zurich, E., min., S.E., W.
St. Gall, S.E., S.S.E., min., S.E.

4. Modified, on the South side of the Alps.

Milan, W., S.W., min., W., S.E.
Pavia, S.E., min., S.W.
Modena, S.E., min., S.W., W.
Padua, W., S., min., N.

Florence, S., S.S.W., min., S.W.
Rome, S.S.E., S., min., S.S.E., S., S.S.E.
Molfetta, S.E., S., min., S.S.W.

The two views which I have thus contrasted have recently formed the subject of a very animated discussion. On the one side Mr. Redfield, of New York, has been led, by a most careful examination of the phænomena accompanying the very frequent storms on the coasts of the United States, to the same conclusion as that which I had arrived at for Europe. On the other hand, the view enounced by Brande has also found an American supporter in Mr. Espy of Philadelphia. The tornado of the 19th of June, 1835, gave occasion to Mr. Espy to assume the hypothesis of centripetal storms. After the tornado* Mr. Bache and Mr. Espy visited the site of a wood over which it had passed, for the purpose of examining the direction in which the trees had been overthrown, and they found that the tops of all the trees pointed to a centre, the most western trunks lying with their heads towards the east, those to the north with their heads towards the south, the eastern ones towards the west, and the southern ones towards the north. An eye-witness of this storm, Lewis Back, maintains, on the contrary, that it was a decided whirlwind, and asserts that no one who beheld it could think otherwise, unless they brought with them previously-embraced theoretical views. Mr. Espy's account of the cause of the inflowing towards a centre is the following:—He considers that when aqueous vapour is condensed into the form of a cloud, it disengages heat, which heat causes the air which contained the vapour to expand six times the loss of volume from the con-

* Notes and diagrams illustrative of the directions of the forces acting at and near the surface of the earth, in different parts of the Brunswick tornado of June 19th, 1835.

densation of the vapour. This air he supposes to ascend therefore with a velocity of 364 feet in a second, and at the height of hail clouds to exert on a square foot of surface a pressure of 120 cwt., capable of carrying up a cubic block of ice of a foot and a half dimension, or even of lifting an elephant. These conclusions, which are termed by Mr. Espy himself "extraordinary and unexpected," are to be found in a memoir consisting of sixteen pages, and bearing the modest title of, 'Theory of Rain, Hail and Snow, Waterspouts, Landspouts, Variable Winds and Barometric Fluctuations, and examination of Hutton's, Redfield's, and Olmstedt's Theories.' We are indebted to the repeated attacks of this author for having given occasion to some excellent memoirs from Mr. Redfield*. The collection of observations, to serve as materials, formed by Mr. Redfield with the greatest care, has further received a highly important augmentation, by the magnificent work which the present Governor of the Bermudas, Lieut.-Colonel Reid, has published on the subject†. Colonel Reid has arrived at precisely the same result as Mr. Redfield, and I know by written communications, that both these gentlemen have done so quite independently of my earlier researches. But Redfield and Reid, besides placing on a wider basis the rotatory movement which takes place in opposite senses in the two hemispheres, have added further some very material observations, whose empirical establishment is entirely their own; these I shall now attempt to connect theoretically with the rotation movement.

* Remarks on the prevailing Storms of the Atlantic Coast. (Silliman's American Journal, 20, No. 1.)

Hurricane of August 1831. (To the Editor of the Journal of Commerce.)

Observations on the Hurricanes and Storms of the West Indies, and of the Coast of the United States. (Blunt's American Coast Pilot, 12th edit.)

On the Gales and Hurricanes of the Western Atlantic. (Sill. Amer. Journ. 31, No. 1.)

Meteorological Sketches, by an Observer. (Sill. Amer. Journ. 33, No. 1.)

Remarks on Mr. Espy's Theory of Centripetal Storms, including a Refutation of his Positions relative to the storm of 3rd of September, 1821, with some notices of the fallacies which appear in his examinations of other Storms. (Journal of the Franklin Institute.)

On the Courses of Hurricanes, with Notices of the Typhoons of the China Sea and other Storms. (Sill. Amer. Journ. 35, No. 5.)

The Law of Storms. (New York Observer, 18th January, 1840.)

Whirlwinds excited by Fires, with further Notices of the Typhoons of the China Sea. (Sill. Amer. Journ. 36, No. 1.)

† An attempt to develop the law of storms by means of facts arranged according to place and time, and hence to point out a cause for the variable winds, with a view to practical use in navigation; illustrated by charts and woodcuts. London, 1838.

In my first researches on the subject of the winds, I had referred both the law of rotation and the rotatory movement of storms to the mutual action of two currents of air, each tending to press aside the other; but a more close investigation of the phænomena has taught me to regard the law of rotation as resting on more general conditions, and as being a simple and necessary consequence of the rotation of the earth. The principle of Hadley's theory of the trade winds thus generalized, explained fully all the rules which had been found for the non-periodic variations of the meteorological instruments in the northern hemisphere, and permitted the prediction of rules for the southern hemisphere; but it did not explain the rotatory movement of storms, and consequently when I published my *Meteorologische Untersuchungen*, Berlin, 1837, which were made to embrace all that I had previously written on the subject, I was obliged to retain the earlier theoretical representation, since that which had been thus empirically deduced had been fully confirmed, but without its connexion with the principle of the general theory being shown. The object of the present memoir is to supply this deficiency. From the researches of Redfield and Reid we have the following facts:—

1. Storms which originate within the tropics preserve the first direction of their path almost unaltered, until they enter either of the temperate zones, when their course becomes deflected into one almost at right angles to the former. Thus the storms of the northern hemisphere move from S.E. to N.W., until they have past to the north of the tropic of Cancer, when their course becomes from S.W. to N.E.; and, on the other hand, the storms of the southern hemisphere, whose progress within the tropics is from N.E. to S.W., take a new direction on entering the southern temperate zone, and then move from N.W. to S.E.

2. The breadth of the whirlwind, which increases very gradually within the tropics, becomes suddenly greatly augmented at the time when the path undergoes the above-described flexure on passing those limits. The chart of the West India hurricane of the middle of August 1837, in Colonel Reid's work, and that of the Mauritius storm of March 1809, in Berghaus's atlas, are examples of these phænomena in either hemisphere. The course of storms is further illustrated by a chart of Redfield's, in which the tracts of ten are laid down. The paths of two of these

storms, which did not extend beyond the tropics, are rectilinear; that of the 23rd of June, 1831, passes from Trinidad by Tobago and Granada, through the middle of Yucatan to the neighbourhood of Vera Cruz; that of the 12th of August, 1835, passing from Antigua by Nevis, St. Thomas, St. Croix, Porto Rico, Hayti, Matanzas and Cuba, and thence to Texas.

The courses of the eight storms which passed the boundaries of the tropics were as follows:—

The storm which ravaged Barbadoes on the night of the 10th of August, 1831, reached Porto Rico on the 12th, the Keys, St. Jago de Cuba on the 13th, Matanzas on the 14th, the Tortugas on the 15th, the Gulf of Mexico on the 16th, and finally, Mobile, Pensacola and New Orleans on the 17th, so that it had passed over a space of 2000 nautical miles in about 150 hours, or at the rate of 13½ miles in one hour. Its direction before reaching the tropic was N. 64° W.

The storm which began on the 17th of August, 1827, in the neighbourhood of Martinique, reached St. Martin and St. Thomas on the 18th, passed to the north-east of Hayti on the 19th, reached Turk Islands on the 20th, the Bahamas on the 21st and 22nd, the coast of Florida and South Carolina on the 23rd and 24th, Cape Hatteras on the 25th, Delaware on the 26th, Nantucket on the 27th, Sable Island and Porpoise Bank on the 28th, having passed over 3000 nautical miles in eleven days. Within the tropics the direction of its path was N. 61° W.; in the latitude of 40° it was N. 58° E.

The storm which began in the neighbourhood of Guadaloupe on the 3rd of September, 1804, reached the Virgin Islands and Porto Rico on the 4th, the Turk Islands on the 5th, the Bahamas and the Gulf of Florida on the 6th, the coasts of Georgia and of the Carolinas on the 7th, Chesapeake Bay, the mouth of the Delaware and the neighbouring parts of Virginia, Maryland and New Jersey on the 8th, Massachusetts, New Hampshire and Maine on the 9th. Its curved path from Guadaloupe had extended over 2200 nautical miles in six days, or at the rate of 15½ miles an hour.

The storm which prevailed at St. Thomas on the 12th of August, 1830, passed near Turk Islands on the 13th, the Bahamas on the 14th, the Gulf and coast of Florida on the 15th, along the coasts of Georgia and the Carolinas on the 16th, those of Virginia, Maryland, New Jersey and New York on the 17th, George's Bank and Cape Sable on the 18th, and the banks of Newfoundland on the 19th: it advanced therefore eighteen miles an hour. Now if we take the actual velocity of the wind in its rotatory direction as five times greater than the progressive movement of the storm, we have the air moving through 18,000 miles in seven days.

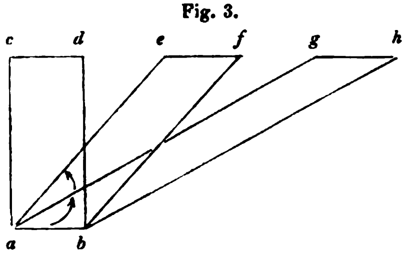
The most eastern storm was that of the 29th of September, 1830. Beginning to the north of Barbadoes in the 20th degree of latitude, in long. 68° lat. 30°, its course became northerly, and subsequently, after passing to the west of the Bermudas, north-easterly until on the 2nd of October it reached the east end of the banks of Newfoundland.

A very violent storm, but of much less diameter than the preceding, prevailed at Turk Islands on the 1st of September, 1821. On the following day it was felt north of the Bahamas, early on the 3rd it reached the coast of the Carolinas, later in the day New York and Long Island, and in the following night it passed over the states of Connecticut, Massachusetts, New Hampshire and Maine, a course of 1800 miles in sixty hours: its mean velocity was therefore thirty miles an hour.

The course of the storm of the 28th of September, 1838, was similar to the one last described. The storm of the 22nd of August, 1830, was much slower in its progress. It began in lat. 20° to the north of Porto Rico, and preserving about the same distance from the coast of North America, reached the banks of Newfoundland on the 27th*.

The explanation of these phænomena appears to be the following:—Let *a b* (Fig. 3.) be considered to represent a series of material points parallel to the equator; supposing these points to receive from any cause whatever an impulse in the direction *a c* towards the north, then, inasmuch as they would be transferred from greater to lesser

Fig. 3.



* As the direction in which the storm is advancing is quite a different thing from the direction in which the rotating current may be blowing at a given place, it is easy to see what incorrect conclusions might be arrived at by an exclusive consideration of observations merely local. Thus Raynal in his *Histoire Philosophique et Politique des deux Indes*, vol. v. p. 72, says that recent observers had noticed that the storms which had ravaged the Antilles from

parallels of latitude, they would move towards *g h* if the space *d b h* were a vacuum; but if there be in this space air not in movement, then the particles in *b*, as they move towards *d*, will be continually coming in contact, in the space *d b h*, with particles of air of inferior rotatory velocity, and thus their own eastward velocity will be diminished. Thus the point *b* will move towards *f* instead of towards *h*; but the particles at *a* have near them, on the side of *b*, particles of originally equal rotatory velocity with their own; they move therefore as they would do *in vacuo*, *i. e.* towards *g*. Therefore if *a b* be a mass of air impelled from the south towards the north, then the direction in which the storm is blowing will be much more southerly on its eastern side than on its western side, where it will be more westerly, and thus there will arise a tendency to rotate in the sense S. E. N. W. This rotatory tendency would not be produced at all if there were no resisting mass in the space *d b h*; it will therefore be proportionate to the resistance thus opposed to the westerly deflection of the storm, and the force of the rotatory movement of the storm will be the greater the more the original direction of its path is preserved. In the zone of the northern trade winds, the space *d b h* is filled with air which is flowing from N.E. to S.W.; the resistance will here be the greatest, so that the air in *b* may be so checked in its westward tendency, that it may preserve its direction towards *d* almost unaltered, whilst *a* tends toward *g*; the rotatory motion of the storm will therefore be the most violent, whilst at the same time its course will be rectilinear, and its breadth unaltered. But when it reaches the temperate zone there will be in the space *d b h* air which is already moving from S.W. to N.E.; the resistance hitherto encountered at *b* will therefore be suddenly greatly diminished, or altogether removed; and hence the direction *b d* is suddenly changed into the direction *b h*, so that the storm is suddenly deflected almost at right angles, and at the same time its breadth increases rapidly from the cessation of the difference between the movements of the points in *a* and the

time to time had always come from the north-west, from which he concludes that they came from the mountains of Santa Martha, although all that is really indicated is that the islands are situated on the southern side of a rotating storm, in which the movement is in the opposite sense to that of the hands of a watch, or from east to west, which agrees perfectly with the observations referred to above. The frontispiece of the sixth volume of Raynal's work is a vivid picture of a West Indian hurricane.

points in *b*. The phænomena in the southern hemisphere may be derived in the same manner; the rotation is in the opposite sense, and the change of direction on passing the boundary of the tropics is analogous*.

As the West India hurricanes originate at the inner boundary of the trade winds, where at the limit of the so-called region of calms the air ascends and flows over the trade in an opposite direction, it is probable that portions of this upper current penetrating through the lower one, give the first occasion of those storms. The high mountains of several of the islands, by offering a mechanical impediment, may be one cause of this effect, as the air flows with redoubled velocity between two mountains. The reason of the course of the storm being in the first instance from S.E. to N.W. may be explained by the circumstance, that according to the theoretical deduction which has been given, this direction is the one most favourable to the origination of a rotatory movement. If, as may happen, the first impulse should be from S.W. to N.E., the north-east trade blowing in the opposite direction would equally check all the points of the advancing line, so that no tendency to rotate would be produced.

It is evident that if the above deduction of these phænomena be the true one, a similar whirlwind must be produced whenever, owing to any other mechanical cause, a current flowing towards higher northern latitudes is more southerly on its eastern side than on its western, where the direction is more towards the east. Observations collected by Piddington† show this to have been the case in the storm in the bay of Bengal on the 3rd, 4th and 5th of June, 1838. It was one of those storms which usually accompany the change of the north-east to the south-west monsoon, which change takes place in the bay of Bengal between

* The above derivation of the rotatory movement only applies when great masses of air, of a considerable extension in breadth, are set in motion; lesser whirls of wind or water, as water-spouts, &c., are produced by other causes, and therefore probably do not show either regular direction or definite opposition in reference to the two hemispheres. Mr. Redfield observed a small tornado rotating like the greater storms; but Colonel Reid saw from the Government-house, Bermuda, a water-spout rotating in the opposite sense. The observations of Akin at Greenbush near Albany, of Dwight at Stockbridge in Massachusetts, and of Dr. Cowles at Amherst, of whirlwinds of great force taking place in forest fires in calm weather, show that a strong ascending current may also produce a rotatory motion.

† Researches on the Gale and Hurricane in the Bay of Bengal on the 3rd, 4th and 5th of June, 1838, being a first Memoir with reference to the Theory of Storms in India. Journal of the Asiatic Society of Bengal, No. 91, p. 550; Second Part, No. 52, p. 631.

the 15th of May and the 15th of June*. Throughout the greater part of its course "it was a gale or strong wind blowing with tolerable steadiness from one quarter of the compass," and it was only at a particular part that it was a hurricane or violent wind blowing in a circle or vortex of greater or less diameter†. It blew as a violent south-west monsoon in the space between the east coast of Ceylon and Masulipatam, and across the bay of Bengal towards the mountain range of Arracan, where it turned completely at right angles, advancing into the interior, up the Ganges, and blowing as a south-eastern current over Calcutta and Benares to Cawnpoor, Lucknow and Agra. At the place of flexure near Arracan,—in the focus, as Piddington expresses it, of the parabolic course of the storm,—a whirlwind originated and advanced parallel to the coast, and passing off the mouths of the Ganges, moved in a direction from between E.N.E. and E. towards W.S.W. and W., from Shapooree Island towards Vizagapatam, Gangam, Juggernaut, and the mouths of the Mahanuddy and Bramnee, rotating like the West India hurricanes in the sense S. E. N. W.

Here we have a whirlwind rotating in a precisely similar manner to those before described, arising (under circumstances originally quite different) when the direction of the storm on its eastern side was more towards the north than on its western side; and possibly the typhoons of the Chinese sea may owe their origin to similar causes. In the complex phænomenon of the south-west monsoon the conditions are analogous. The wind, from being south-west in the Indian sea and the bay of Bengal, becomes more nearly south in the Chinese sea; more extended observations are required to show whether this deflection is caused by the chain of the Philippines, or whether it is an im-

* According to the observations of Brown it commenced at Anjarakandy on the Malabar coast in 1820–1833, on May 20, 31, 31, 27, June 15, May 21, June 18, May 26, June 5, May 9, 26, June 16, 2, 6: at Canton it set in, according to the Canton Register, from the 20th to 28th of April in 1830, from the 7th to 17th of April in 1831, from the 4th to 7th of April in 1832, from the 9th to 14th of April in 1833, from the 3rd of April to 8th of May in 1834, and from the 8th to the 21st of April in 1835.

† This seems to be also the case with the storms which accompany the change from the south-west to the north-east monsoon. These storms, which the Spaniards in Manilla call "los temporales," are not accompanied by rain, but the air is everywhere darkened by the salt spray from the sea. On the coast of Coromandel these storms are termed "the breaking out of the monsoon." On the Malabar coast the Portuguese call those which are peculiarly violent, "Elephanta."

mediate consequence of the bordering of the monsoon and the trade. Horsburgh says expressly*, that on the south coast of China the typhoons from July to September make the wind vanes when near the coast point successively N.W., N., N.E., E., S.E. and S., and that further off the coast they point instead to N., N.W., W., S.W., S. In other words, the typhoons are storms rotating S. E. N. W., passing along the coast from east to west, so that the northern half of the whirlwind impinges on the coast, and the southern half covers remoter portions of the sea. The Raleigh typhoon of the 5th of August, 1835, which passed from Bashee Island, between Luconia and Formosa towards Macao, in the direction from E.S.E. to W.N.W., is a recent example of these storms, and corresponds perfectly to the description which has just been given.

But if these rotatory storms arise from the south-west monsoon being more southerly on its eastern than on its western side, and if for this very reason they move from east to west, they will prevail by preference in the eastern part of the Indian ocean; and in fact it had been remarked by Dampier that on the coast of Coromandel storms are looked for in April and September, which are the months of the change of the monsoons, whilst on the Malabar coast they are frequent during the whole westerly monsoon.

Having thus found in the typhoons a confirmation of the principles which were applied to the West India hurricanes, we may proceed to consider in greater detail the phænomena which accompany these great disturbances of the atmosphère.

When in the storms of the regions of the trades the rotating cylinder from the lower trade impinges on the upper current, it is evident that inasmuch as a south-westerly direction of the wind prevails above, the reasoning which has been made use of for the lower part of the same cylinder when it passed beyond the external limit of the trades, becomes applicable to its upper part, which will immediately spread, and will advance in a different direction from that of the lower part of the whirlwind. Thus will arise the secondary phænomenon of suction in the middle of the whirlwind, producing a diminution of pressure on the surface of the earth, and this for two reasons, viz. inasmuch as the rotation causes the air to fly from the centre, and as moreover the whirlwind widens conically in ascending, and conse-

* India Directory, vol. ii. p. 233.

quently the upper strata are more distant from the axis of the cylinder than the lower, which have therefore a tendency to ascend in order to compensate the diminished density above.

But that the storm itself does not originate from this kind of suction, will be evident on a closer consideration of the observations. I will take as an example the hurricane of the 2nd of August, 1837, for which we have simultaneous observations at St. Thomas and at Porto Rico, which are shown in comparison in the following table:—

	St. Thomas.			Porto Rico.		
Mean Time.	Barom.	Wind.		Barom.		Wind.
h m	‴					
Aug. 1. 18 0	337					
2. 2 10	335	N.W.	Increasing tempest.			
3 20	334	N.				
3 45	334	N.				
4 45	332	N.				
5 40	331·5	N.E.				
5 45	330	N.E.	Hurricane.			
6 30	328	N.W.				
6 35	325·5	N.W.				
6 45	324	N.W.				
7 0	324	N.W.				
7 10	322	N.W.				
7 22	318·5	N.W.				
7 30	317	N.W.				
7 35	316·5	Dead calm.				
7 52	316			h	‴	
8 10	316			8	333·28	N.N.E.
8 20	316					
8 23	320	S.S.E.	Hurricane.			
8 33	321	S.E.				
8 38	322	S.E.				
8 45	323	S.E.				
8 50	324	S.E.				
9 0	326	S.E.			332·16	
9 10	328	S.E.				
9 25	329	S.E.				
9 35	330	S.E.				
9 50	331	S.E.				
10 10	332	S.E.		10	331·03	
10 35	333	S.E.				
11 10	333·25	S.E.		11	329·90	E.
11 30	333·5	S.E.		12	315·27	Hurricane.
14 45	335	S.E.		15¼	328·43	S.
20 0	336·5	S.W.		16	332·16	
21 0	336·75	E.				

The dead calm suddenly interrupting the fiercest raging of the storm from opposite directions, which is shown in the register of observations at St. Thomas,—that dreadful pause which fills the heart of the bravest sailor with awe and fearful expectation,—receives a simple explanation on the rotatory theory, which requires that at the centre of the whirlwind the air should be in repose; but appears irreconcileable with the supposition of a centripetal inflowing, because two winds blowing towards each other from opposite directions must *gradually* neutralize each other, and thus their intensity must diminish more and more in approaching their place of meeting. This takes place on the great scale in the trade winds; and if the centripetal view of hurricanes were the just one, the same effect would necessarily be seen as the centre of the storm passed over the station of observation. But the phænomena shown by observation are widely different. At St. Thomas the violence of the tempest was constantly increasing up to $7^h 30^m$ A.M., when a dead calm succeeded, and at $8^h 10^m$ A.M. the hurricane recommenced as suddenly as it had intermitted. How can this be reconciled with the meeting of two winds? Besides, the air at Porto Rico should have been flowing towards St. Thomas at that time, and therefore should have been west, whereas it was N.N.E., just as is required by a whirlwind of which St. Thomas was then the centre.

A remark of the St. Thomas Observer, Hoskiaer, to the effect, that at each gust the mercury in the barometer sunk two lines and then immediately rose again to the same height as before, shows the diminution of atmospheric pressure to be not the cause, but rather a consequence attendant on the violent movement of the air.

In considering the progressive advance of the whirlwind, we have not hitherto taken into account the resistance opposed to the motion of the air by the surface of the earth. This resistance, as Redfield justly remarks, causes the rotating cylinder to incline forwards in the direction of its advance, so that at any station the whirlwind begins in the higher regions of the atmosphere before it is felt on the surface of the earth, where therefore the sinking of the barometer indicates its near approach. The inclined position of the axis causes a continual intermixture of the lower and warmer strata of air with the upper and colder ones, thereby occasioning heavy falls of rain, and proportionably violent electric explosions. The cold air appears to precipitate itself from the cloud, and the storm to assume the form called by the Greeks ἐκνεφίας. This may also explain the phænomenon known to the navigators of the torrid zone under the name of bulls'-eyes, *i. e.* a small black cloud appearing suddenly in the sky in violent motion, which becoming apparently self-developed, soon covers the face of the heavens, and is followed by an uproar of the elements, rendered doubly striking by the previous untroubled serenity of the sky.

[M. Dove then quotes from Colonel Reid's work, a very vivid description given by eye-witnesses of the Barbadoes hurricane of the 10th of August, 1831, in which violent electric explosions (both lightning and meteors), the heavy rain, the tremendous force of the wind, its changes of direction, and its interruption by lulls, all form part of the picture.]

Let us now consider these storms on their entrance into the temperate zone. Their change of direction from S.E. to S.W., on passing the outer boundary of the trade winds, has been explained on the assumption of the storm meeting with S.W. winds, instead of the N.E. wind which had till then opposed its advance. It must however be remembered, that the direction of the wind in the temperate zone is not constant but varying. Such phænomena as those which have been described require for their occurrence, that the S.W. winds do actually predominate previously in the temperate zone: barometric minima accompanied by storms are therefore only observed when those conditions are fulfilled. They were so in a high degree previous to the time of the minimum on the 24th of December, 1821; for in November and December the mean direction of the wind had been south-west in Penzance, London, Bushey, Cambridge, Lancaster, Manchester, Paris, Brest, Dantzic, Königsberg, &c.; and it appears from the *Bibliothèque Universelle*, that a more or less stormy south-west wind prevailed throughout the middle region of Western Europe.

We have before assigned reasons for the sudden increase of breadth and diminution of intensity which accompany the change of direction of the course of the storm. It will be seen by the converse of the same reasoning, that the intensity increases again, when smaller whirlwinds are, from any cause, developed from the larger one. Such was the case in the Mediterranean at the time of the minimum of the 21st of December, already noticed, when the advancing masses of air, arrested in their progress by the Spanish mountains and by the Maritime Alps, were set into violent rotatory motion around these points as fresh centres; and we find accordingly that the force of the storm was particularly great there no less than at the primary centre. In regard to the latter, the Brest accounts say, on the 26th of December, "We have been living for fourteen days in the midst of storms, which have not ceased to rage with unparalleled fury." In London there was the highest flood which had been seen since 1809. At Portsmouth one gust from the S.S.E. is spoken of as almost unprecedented, and the sea rose to an enormous height. In regard to the secondary centres, the ravages of the storm in and around the Mediterranean were very great. From Leghorn to Barcelona it was terribly destructive. On the southern declivity of the Alps enormous masses of rain fell, and Venice, Genoa and Nice were overflowed. In Appenzell the tempest was such as the oldest inhabitants had never witnessed; it raged with peculiar force in the valleys; the mountains presented such an obstacle to the pressure of the stream of air, that the barometer stood much higher on their southern than on their northern declivity.

We see that those barometric minima of the temperate zone, which are caused by the entrance of tropical whirlwinds, are distinguished from the same phænomena in the torrid zone by their greater extension as well as by the different direction of their course. In the case of the minimum of the 2nd of August, 1837, the difference of barometric pressure at St. Thomas and Porto Rico, places scarcely twenty miles apart, was 15 lines. On the 24th of December, 1821, the difference of pressure at Brest and Bergen was only 12 lines, the amount of the absolute minimum being the same in both instances. On the 21st of May, 1823, on the Hidgelee coast, the barometer fell on board the Duke of York, between 8 A.M. and 11 A.M., from 325''' to below 298''', or 27 lines in three hours, as shown both by the barometer and sympiesometer (the fluid in both instruments having sunk for the space of half an hour below the visible part of the tubes, which began at 298'''), the simultaneous fall at Calcutta having been only 8 lines. We see thus, that the fall of the barometer previous to the minimum, and its subsequent rising, take place much more rapidly within the tropics than in the temperate zone; but if we consider the total diminution of pressure, we shall find that it is much greater in temperate than in tropical regions. In the former it may be compared to an extensive valley with gentle declivities, in the latter to a deep ravine with precipitous sides. Besides the causes of diminished atmospheric pressure in the tropical regions, an additional cause comes into play in the temperate zone, viz. the high temperature brought from lower latitudes by the rapid movement of the air from that direction. On the 24th of December this was very considerable. In Tolmezzo the thermometer rose to 25° Reaumur in the shade; at Geneva it rose suddenly 5° in the night of the 24th and 25th; at 1 A.M. on the 25th it reached 12°·5, which was its highest point. In Boulogne, Paris, and Hamburg the temperature was unusually high. It seems evident that when so warm a current of air was flowing towards the Pole over Europe, the cold air displaced by it must flow southward in some other quarter, and according to the rotation of the whirlwind, this might be expected to be in America. In effect, the thermometer at Salem in Massachusetts, in the latitude of Rome, stood − 10°·2 R. on the 24th of December, and a few days later at − 14°·2 R., and all accounts from America speak of an unusual degree of cold.

But these phænomena are not peculiar to the winter months. The storm which ravaged St. Thomas and Porto Rico on the 2nd of August, was followed in the middle and on the 21st of the same month by two very violent storms, which are described in detail in Colonel Reid's work; at the same time unusual heat, accompanied by most violent storms of wind and heavy rain, prevailed in Europe. From the 10th to the 20th of August the thermometer stood at + 30° R. in Messina, and between + 28° and + 30° at Naples; on the 12th it stood at + 30° at Rome, whilst at Rothen and in the Emmethal the torrents, swollen by the violent rains, swept along rocks of 60 cwt. In Silesia the heat was oppressive. In Gallicia and Prussia this unusual heat was followed near the end of the month by remarkable cold. This had prevailed in America during the great heats in Europe, for at Rochester, in the state of New York, on the 4th of August, the extraordinary phænomenon of a night frost had been witnessed.

If in these meteorological phænomena of the temperate zone we recognise the manifest influence of the quickly succeeding disturbances of the atmosphere within the tropics, we shall at once see the reason why deviations from the order of change in the direction of the wind, which results from the law of rotation, namely, S. W. N. E., are a sure sign of very unsettled weather;

a remark which has been made by almost all observers who have carefully examined the connexion of the direction of the wind with the accompanying phænomena of the weather. On the north-western side of a rotatory storm the wind vane turns N.W., W., S.W.; the usual order, according to the law of rotation, being exactly opposite, *i. e.* S.W., W., N.W.

We thus see that the rotation of the earth on its axis causes three different phænomena:—1. The constant direction of the trade winds, and the regular alternation of the monsoons. 2. The regular order in the change of direction of the wind, which in both hemispheres is with the sun. 3. The rotatory movement of storms in a determinate order.

The course of the phænomena which we have been considering becomes very much complicated when the advancing storm meets another wind, or when it has successively to press aside currents of air from different directions. In treating of the minimum of February 2nd and 3rd, 1823 (Pogg. *Annal.*, vol. xiii.), I have considered in detail a case of this kind, in which a north wind blowing directly against the south-west current, the meeting of the two produced a calm, which appeared to bear no sort of relation to the alteration of the atmospheric pressure. The minimum would seem to be divided into two portions by the current flowing towards its centre, so that there are two places of least pressure. The elucidation of these phænomena requires the comparison of observations made over a very extended surface; but as the view which I then took has been since confirmed by more complete data, I refer to it here. This case leads us on to the consideration of the phænomena which follow these great agitations of the atmosphere, when the equilibrium which had been violently disturbed re-establishes itself after the disturbing cause has ceased to act; but these secondary phænomena must not be confounded with the primary ones. North of the minimum there may often be found an unusually high barometer, accompanied by severe cold and by heavy falls of snow at the limit of contact between the warm and the cold air. The falls of snow do not enter far within the precincts of the cold, but rather form a border along its limits. As long as the minimum repels the cold air, and causes it to accumulate, the falls of snow, succeeded by thaw, recede likewise towards the north; but when the polar current forces its way underneath, the falls of snow are immediately followed by fresh cold advancing from N.E. to S.W.

When navigators are overtaken by a rotatory storm, the following are practical rules for escaping from its influence as soon as possible:—

1. In the northern temperate zone:—If the gale begin from the S.E. and veer by S. to W., the ship should steer to the S.E., for she is on the south-eastern part of the storm. If, on the contrary, it begin from the N.E. and change through N. to N.W., the vessel should steer north-westward, for she is in the north-western half of the storm.

2. In the northern part of the torrid zone:—If the storm set in from the N.E. and the wind change through E. to S.E., the ship should steer N.E., for she is in the north-eastern part of the storm; if it begin from the N.W. and change by W. to S.W., steer towards the S.W., for the vessel is on the south-western side of the storm.

3. In the southern part of the torrid zone:—If the wind set in from the S.E. and alter by S. to S.W., the ship must steer N.W., for she is on the north-western side of the storm; but if the gale begin from E. and pass through N. to N.W., steer S.E., for the vessel is on the south-eastern side of the storm.

4. In the southern temperate zone:—If the gale set in from the N.E. and veer by N. to N.W., steer towards the N.E.; but if it begin from the S.E. and change through S. towards S.W., steer to the S.W., for in the first case the ship is on the north-eastern, and in the second on the south-western side of the storm.

If our examination of storms of the temperate zone has pointed to the hurricanes of the tropics as their source, we do not therefore conclude that causes originating these phænomena may not also exist in the middle latitudes. The violent tempests of the Black Sea and the Levant, which usually mark the beginning of the rainy season in those regions, and which, on that account, are called "Temporales," appear to owe their intensity to local conditions. But we possess no detailed account of the direction in which these storms move, and of the order of change in the indications of the wind vane during their prevalence. The absence of such information in regard to seas so much frequented is a remarkable circumstance, and is much to be regretted.

[M. Dove concludes his memoir with a very interesting description of the effects of the storm of the 10th of October, 1780, which it seems unnecessary to repeat to the English reader who has access to Colonel Reid's work, from which it is taken.]

Article IX.

***On the Non-periodic Variations in the Distribution of Temperature on the Surface of the Earth, between the years* 1782 *and* 1839. *By Professor* H. W. Dove, *of Berlin**.**

THE mean temperature of the atmosphere is as little to be ascertained by immediate observation as the mean moisture or the mean pressure. It is concealed under manifold changes, of which we distinguish two kinds, periodic and non-periodic. By the non-periodic, those changes are meant whose regular or periodic return cannot be ascertained with any certainty, and is not even probable. Meteorology has therefore three questions to solve; —first, the determination of the *means*; secondly, the establishment of the laws of the periodic variations; and thirdly, the assignment of rules for the irregular or non-periodic variations: none of these problems have been as yet brought near a complete solution.

The mean distribution of temperature on the surface of the earth was first and most naturally represented by Alexander von Humboldt in his isothermal lines; the importance of this method of representation, in regard to vegetation, has been shown by Von Buch, Schouw, Wahlenberg, and Richardson. Brewster and Kämtz have since employed more recent observations to produce a more accurate representation of the isothermals; Brewster by the advantageous substitution of the equatorial for the polar projection, and Kämtz by drawing up special maps of single localities for which observations existed, and subsequently combining these in a general view.

That the atmosphere does not exert an equal pressure on all parts of the earth's surface at the level of the sea, was first shown by Von Buch at the two limits of the temperate zones: he noticed the low barometer on the coasts of Norway, and the high barometer at the Canary Islands.

Humboldt had already shown that at the equator, where the air ascends, the pressure is diminished. Schouw has given a general view of what has been empirically deduced for the different zones. Daniell first attempted to show what part of the

* Abstracted by Mr. Henry Croft, Teacher of Chemistry, London.

entire atmospheric pressure might be attributed to the elasticity of the aqueous vapour in different latitudes. The facts of observation, extending over several years, are still too isolated to permit more than an approximate connexion to be traced.

In searching for the laws which regulate the periodic variations, two modes have been pursued, a practical and a theoretical one; by the first, methods are sought by which the mean state of the atmosphere may be calculated; and by the second, the causes of the changes are examined.

In considering the daily variations of temperature, the first alone has been employed, while it is allowed that the second method is very important as regards the barometrical and hygrometrical variations (*Vide* Dove on the Daily Oscillations of the Barometer). I have endeavoured to show that the whole of the non-periodic variations may be referred to, and explained by, one primary principle, which I have called the "Law of Rotation" of the winds. Aristotle observed the regular passage of the different winds into each other: Von Buch proved the influence of the direction of the wind on the barometer, thermometer, and hygrometer. Now, as the greater number of the so-called irregular variations of the instruments are only the passage of the barometric, thermic and hygrometric states of the winds into each other, it is clear that the laws of the irregular changes can only be recognised when the mean variations of the direction of the wind are combined with the mean distribution of pressure, temperature and moisture in the wind-rose (*Wind-rose*). By comparing the direction of the wind with the state of the three instruments for Paris and London, I obtained exactly the same results as I had formerly at Königsberg; the circle of the wind-rose may be divided into halves; if the instruments rise on one side, they fall on the other,—the two parts exhibit exactly opposite phænomena. It was then easy to refer the whole of the phænomena of the weather to the conflict of two winds, whose direction agrees with the dividing line of the wind-rose, and which, when acting singly, produce the extremes; and when only partially prevailing, either over the other, cause those changes which distinguish our climate. Howard calls these winds the true monsoons of our regions.

If, as Hadley has done, we consider the difference in the rapidity of rotation of the earth in different degrees of latitude as a cause affecting the currents of air, and if we moreover introduce as a second cause the fact that the masses of air previously in a state of rest are set in motion *one after the other*,—and that thus the point of commencement of the current, which in Hadley's theory is invariable, here becomes continuously variable,—and if we extend this view to two currents of wind in continual conflict, we deduce immediately the 'Law of Rotation,' and at the same time the laws for the motion of the instruments, as I have shown in the Fourth Part of my 'Meteorological Researches.' That, however, which is seen in the movements of our instruments at one place will, of course, become more strikingly apparent when the simultaneous diffusion of the phænomena over a greater extent of the terrestrial surface is laid before our eyes: the force, direction, and conflict of the two opposing currents will then become more evident.

Supposing that, at a certain time, a modification in the mean distribution of temperature is caused by these currents, if we proceed in the direction of their course we shall everywhere find either a rise or a fall relatively to the normal temperature; if, however, we proceed more or less perpendicularly to this direction, we shall somewhere find their line of separation.

In this work the thermal phænomena for the last fifty years have been compared. Observations at one place, or for single years, can never be of any very great importance; the results are often erroneous when obtained from such sources.

Observations from fifty-nine places have been employed, twenty-two in Germany, twelve in England, six in North America, five in Italy, three in Switzerland, three in Sweden, two in Holland, two in Russia, one in Hindoostan, one in Iceland, one in France, and one in Siberia.

(Then follow tables showing the mean temperature of the twelve months in these fifty-nine places, calculated for several years. As example, the temperature of the year 1836 is here drawn from the several tables.)

Boston (*Fahrenheit*), 1826-1839.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1836.	37·3	37·5	42·7	45·8	54·8	62·4	62·1	59·6	54·9	48·0	41·4	38·9
Mean	36·17	38·69	42·96	48·5	57·09	61·23	63·53	61·00	56·6	50·96	42·88	40·23

Dresden (*Celsius*), 1812–1826, *and* 1827–1837.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1836	– 0·17	2·26	9·45	9·76	12·18	18·81	18·97	17·66	14·22	12·13	4·27	2·45	10·17
Mean	– 1·65	0·72	4·40	9·78	14·62	18·43	19·71	18·52	14·59	10·02	3·80	0·94	9·46

London (*Fahrenheit*), 1787–1838.

1836.	39·15	38·35	45·35	45·90	52·65	62·15	64·50	61·17	55·75	49·40	43·15	41·20	
Mean	34·7	36·6	37·1	41·3	46·4	52·4	55·6	55·1	51·7	45·5	40·0	37·3	

Paris (*Celsius*), 1806–1838.

1836.	2·6	2·9	8·7	8·6	12·4	18·4	19·4	18·9	14·1	11·2	7·6	4·1	
Mean	1·92	4·48	6·67	10·02	14·70	16·85	18·79	18·36	15·61	11·47	6·70	3·97	10·822

Petersburg (*Reaumur*), 1822–1836.

1836.	– 7·50	– 4·38	1·21	4·78	6·34	10·97							
Mean	– 7·67	– 5·92	– 2·89	2·30	7·34	12·42	14·18	12·96	8·58	4·17	– 0·72	– 4·19	3·38

Reikiavig (*Reaumur*) 1823–1837.

1836.	– 2·86	– 4·84	– 3·07	–1·17	5·17	7·73	10·19	6·59	4·62	0·52	– 2·12	– 3·10	1·51
Mean	0·04	– 1·55	– 0·50	2·21	5·34	8·47	10·08	8·92	6·28	2·59	– 0·37	– 0·53	3·42

Stromness (*Fahrenheit*), 1828–1834.

1836.	39·14	37·46	39·64	41·39	47·77	52·03	52·51	51·83	48·35	45·91	40·93	38·57	44·62
Mean	38·36	39·07	41·23	43·90	48·89	54·51	56·63	55·57	53·09	49·65	43·03	41·29	

Stuttgard (*Reaumur*), 1828–1837.

1836.	– 0·72	0·19	7·11	6·68	9·12	14·2	15·23	15·25	10·76	8·55	3·87	2·14	7·70
Mean	– 1·51	1·02	4·18	7·48	11·80	13·76	15·56	14·11	11·03	7·82	3·52	0·91	7·47

From the above observations we see that the temperature of the month of March in 1836 was, at Reikiavig, – 3·07, at Stromness 3·38, at Boston 4·75, at London 5·93, at Paris 6·96, at Stuttgard 7·11, at Dresden 7·64, and at Petersburg 1·17; but, by merely comparing these numbers with one another, we cannot arrive at any conclusion as to the distribution of temperature at this time; we cannot tell in which places the temperature was too high, and in which too low. But when we know that the mean temperature of March during many years was at these places respectively − 0·50, 4·10, 4·87, 5·29, 5·60, 4·18, 3·38, −4·54, we see instantly that at Reikiavig in 1836 the temperature of March was 2°·57 too low, at Boston normal, at London a little too high, and that this excess extended through Germany up to Petersburg, where it reached its maximum, the temperature being there almost 6° too high. Such differences alone can be quantitatively compared, and they have the advantage that the faults arising from imperfect instruments are eliminated.

We must not assume that the true mean monthly temperatures are obtained from the above observations; but we shall make a nearer approach to truth if we compare the means of each place drawn from the same period of time. From 1807 to 1824 we have observations for twenty places, extending from Madras to Torneo, and from Salem to Dantzic; for a few places some years are wanting. From 1820 to 1830 we have them for twenty-three places; from 1828 to 1834 for thirty-five, &c. &c. (Tables here follow in which the observations are arranged in the above manner.)

From these tables we may draw the conclusion "that there at times exist certain causes which have considerable duration, and extend over a considerable space, which either raise or depress the temperature." The following table of the yearly means will show this more plainly:—

	1807 to 1824.	1820 to 1830.	Difference.
Palermo	13·37	13·63	+ 0·26
Milan	10·10	10·36	+ 0·26
Geneva	7·61	7·52	− 0·09
Paris	8·41	8·73	+ 0·32
London	7·56	7·97	+ 0·41
Munich	7·26	7·51	+ 0·25
Regensburg	6·52	6·84	+ 0·32
Stuttgard	7·61	7·83	+ 0·22
Carlsruhe	8·30	8·65	+ 0·35
Berlin	6·81	7·46	+ 0·65
Torneo	− 0·43	− 0·72	− 0·29
Salem	7·25	7·73	+ 0·48

	1807 to 1824.	1797 to 1804.	Difference.
Madras	22·23	22·41	+ 0·18
Palermo	13·37	13·81	+ 0·44
Milan	10·10	10·58	+ 0·48
Innspruck	7·39	7·51	+ 0·12
Stuttgard	7·61	8·36	+ 0·75
Regensburg	6·52	7·26	+ 0·74
Berlin	6·81	6·93	+ 0·22
London	7·56	8·12	+ 0·56
Salem	7·25	7·76	+ 0·41

	1820 to 1830.	1828 to 1834.	Difference.
Milan	10·36	9·95	— 0·41
St. Bernard	— 0·82	— 0·80	+ 0·02
Geneva	7·52	7·89	+ 0·37
Munich	7·51	7·56	+ 0·05
Augsburg	6·76	6·38	— 0·38
Stuttgard	7·83	7·47	— 0·36
Regensburg	6·84	6·80	— 0·04
Berlin	7·46	7·08	— 0·38
Paris	8·73	8·67	— 0·07

In these tables we observe so striking an agreement between the signs of the differences, that the foregoing conclusion appears fully justified. The exceptions, of which that of Geneva is the most marked, are probably owing to the disturbing influence of the Alps.

In the next tables, the extent of the variation of temperature of each month, as deduced from the observations of many years, is exhibited. The places of observation are divided into groups according to their climate: this maximum of change may be called the *absolute variability*. The greatest variation of the yearly mean for each place is also shown.

From the attentive examination of these tables we may conclude:

First, the absolute variability of temperature is least in the tropics, but more considerable in the region of the monsoons than in that of the trade winds.

Secondly, in the temperate zone, particularly at places which have not a thorough sea climate, the absolute variability increases as we approach the frigid zone, as shown in the following comparison.

	Italy.	Alps.	Germany.	Northern Europe.
January	5·47	8·89	9·44	10·51
February	5·38	7·09	7·83	10·29
March	5·32	6·66	5·97	8·17
April	4·67	6·45	4·74	6·96
May	4·88	5·26	5·45	5·99
June	5·17	5·06	3·95	5·76
July	3·68	4·99	4·71	5·54
August	4·45	5·36	5·01	5·82
September	4·26	4·65	3·41	5·35
October	4·25	5·10	4·45	6·76
November	4·77	5·99	5·23	7·43
December	5·29	9·10	9·72	9·66
Year	2·21	2·59	2·57	2·64

Thirdly, the neighbourhood of mountains seems to increase the absolute variability, particularly in the summer months.

Fourthly, on passing from the coast into the interior of continents, the absolute variability, which, in a sea climate, is small, gradually increases, but then decreases as we advance still further inland: the greatest absolute variability is therefore neither in the true sea climate, nor in the thorough continental climate, but rather at the point where they pass into each other, as seen in the following table:—

	England.	Coasts of the Continent.	Germany.	Northern Asia.
January	5·88	8·66	9·44	5·17
February	4·86	6·80	7·83	6·51
March	4·44	5·74	5·97	5·07
April	3·97	4·98	4·74	2·72
May	3·88	5·16	5·45	4·13
June	3·98	5·00	3·95	4·03
July	3·51	4·54	4·71	3·03
August	3·63	5·30	5·01	3·25
September	3·63	3·79	3·41	1·82
October	4·20	4·56	4·45	3·21
November	4·40	5·48	5·23	5·14
December	5·04	9·11	9·72	7·75
Year	2·28	2·69	2·57	

Fifthly, the month of September is the steadiest, particularly in the region of the summer rains in the temperate zone. The variability is greatest in the winter months, and decreases rapidly towards spring, in which, at many places, it reaches its minimum.

When we consider that the differences of the yearly means in our latitude are $2\frac{1}{2}°$ or $3\frac{1}{2}°$, and in the winter months may be as high as 14°, we immediately see how little worth can be laid upon climatological conclusions, drawn from the observations of one or even of a few years.

In the next tables the *mean* variability is represented, regarding as mean variability the differences, taken without reference to sign, of each month of several years from the general mean of the same month in the same period of years. For instance, let t be the temperature of January in Berlin, determined by the observations from 1807 to 1824, $t_7, t_8, \ldots\ldots$, the temperature of the month in the single years in that period; and if we suppose

$$\begin{aligned} d_7 &= t_7 - t, \\ d_8 &= t_8 - t, \\ \ldots &= \ldots\ldots, \\ d_{18} &= t_{18} - t, \end{aligned}$$

then the mean variability $d = \dfrac{d_1 + d_2 + \ldots\ldots d_{18}}{18}$, where d_1, d_2, d_3, are all positive values.

[Here follow four tables, showing the mean variability in the years 1807 to 1824 for twenty places, 1820 to 1830 for twenty-three places, 1828 to 1834 for thirty-six places, and 1797 to 1804 for fourteen places.]

These tables confirm generally the conclusions which have been drawn from the preceding ones. The mean variability is greatest in January, decreases until April, increases in our latitudes during summer, and is at a minimum in September. These relations are not so evident in southern Italy and England as in those places which have summer rains; a considerable variation of temperature between single years is caused by the late appearance of the rainy period, or by its total absence. The extent of variability in May is but small, and this explains why vegetation generally commences in this month. In winter the earth loses more heat during the night than it receives during the day, and therefore a clear sky generally causes a diminution of temperature. In summer the exact reverse is observed; the clear days are the warmest. This agrees exactly with the thermic "wind-roses" of the different seasons, and shows the dependence of the temperature on the winds which prevail during each of them. In winter the highest temperature appears with winds which produce cloudiness; in summer those winds are the warmest which, according to the principle of rotation, blow after the clearing up of the weather.

[In the next tables the rise or fall of the temperature of each month in each year above or below the mean temperature of that month for several years is shown. As an example, we will select the tables for the very cold year, 1816, and the very hot one of 1834, which is celebrated for its good wine.]

1816.—Means taken from 1807 to 1824.

	Madras.	Palermo.	Nice.	Milan.	Geneva.	Munich.	Innspruck.
January	−1·07	−0·20	0·51	−0·31	1·37	1·29	0·54
February	−1·53	−0·11	0·19	−3·12	−1·25	−1·23	−3·49
March	−1·45	−0·52	0·26	−1·10	−0·31	0·09	−1·89
April	−1·26	−0·50	0·40	−0·74	0·07	−0·02	−0·22
May	−0·16	−0·30	−0·01	−0·85	−0·63	−0·49	−2·41
June	0·20	−2·02	−0·45	−1·14	−1·35	−0·47	−1·56
July	−0·74	−0·59	−1·27	−1·77	−2·17	−1·11	−2·46
August	−0·87	−0·44	−1·78	−3·36	−1·72	−1·58	−5·22
September	−0·68	−0·85	−0·73	−0·57	−0·38	−0·82	−1·36
October	−0·13	−1·05	1·15	0·26	0·77	0·91	0·03
November	0·24	−0·54	−0·13	−1·56	−0·70	−1·51	−0·72
December	−0·64	−0·90	0·05	−1·78	0·03	0·11	−1·09

	Regensburg.	Stuttgard.	Carlsruhe.	Paris.	London.	Penzance.	Carlisle.
January	1·58	0·80	1·22	0·49	0·42	0·21	0·35
February	−1·74	−2·52	−2·72	−2·07	−2·58	−0·79	−1·46
March	−0·75	−0·41	−0·50	−0·55	−1·13	−0·76	−1·27
April	−0·06	−0·11	0·20	0·15	−0·82	−0·33	−1·24
May	−2·21	−2·05	−2·33	−1·42	−1·53	−0·58	−1·57
June	−1·57	−2·16	−2·08	−1·34	−0·72	−0·33	−1·57
July	−1·85	−1·91	−2·18	−2·24	−1·22	−1·53	−1·61
August	−2·09	−1·97	−1·95	−2·18	−1·22	−0·83	−1·05
September	−1·55	−0·54	−0·79	−1·16	−1·17	−0·08	−1·15
October	−0·49	0·08	−0·30	0·32	0·11	0·65	0·33
November	−1·36	−2·40	−2·42	−2·20	−2·37	−1·25	−1·41
December	−0·29	−0·60	−0·06	0·23	−0·80	−0·48	−0·31

	Dunfermline.	Salem.	Berlin.	Dantzic.	Stockholm.	Torneo.
January	−0·20	0·29	1·31	1·51	0·84	2·73
February	−1·05	0·48	−2·31	−3·00	−3·54	−8·07
March	−1·09	−1·23	−0·75	0·02	−1·42	−4·10
April	−1·63	−0·11	0·35	0·41	0·02	0·85
May	−0·23	−0·85	−2·35	−1·30	−2·32	−2·57
June	−0·64	−1·97	−1·09	0·03	1·18	−0·21
July	−1·29	−2·20	−0·71	−0·57	0·94	−0·30
August	−1·16	−0·92	−2·27	−1·84	−2·94	−0·66
September	−0·69	−1·67	−1·17	−0·77	−0·29	1·77
October	−0·04	−0·01	−1·18	−0·76	−1·06	0·42
November	−0·98	1·52	−2·26	−1·10	−1·07	1·60
December	−0·64	0·44	−0·38	0·21	1·36	3·00

1834.—Means taken from 1828 to 1834.

	Milan.	Bernard.	Geneva.	Basle.	Munich.	I. Augsburg.	II. Augsburg.
January	0·44	1·57	4·68	5·91	−0·26	5·73	5·23
February	0·09	0·40	0·96	0·99	−0·06	1·26	1·53
March	−0·61	0·23	0·30	0·64	−0·61	−0·62	−0·51
April	−1·92	−1·53	−1·28	−1·31	−0·45	−0·67	−1·66
May	0·47	1·03	1·31	1·51	1·91	1·69	2·20
June............	−0·22	1·55	1·32	1·36	1·79	1·62	2·10
July	−0·37	1·28	1·72	2·46	2·35	2·30	2·60
August.........	−0·53	0·90	1·03	1·81	2·26	1·34	2·09
September	1·30	3·86	2·64	2·94	3·89	2·64	4·25
October	−0·55	−0·55	0·39	0·56	−0·18	0·48	0·86
November	0·02	1·22	0·52	0·70	0·72	1·28	0·95
December	−0·85	−0·30	−1·16	−0·39	1·54	−0·14	−0·26

	Stuttgard.	Regensburg.	Hohenfurth.	Neu Bistritz.	Deutschbrod.	Landskron	Prague.
January	5·64	6·03	5·45	5·36	5·08	5·16	5·86
February	0·30	0·02	0·15	0·75	1·21	0·87	1·20
March	−0·36	−0·30	−0·50	−0·55	−0·43	−1·11	0·03
April	−1·83	−1·97	−2·06	−1·86	−1·67	−2·65	−1·25
May	1·47	1·23	1·85	1·97	1·91	1·82	1·64
June............	1·09	0·78	1·72	1·67	0·96	1·10	1·41
July	2·03	4·45	2·12	2·56	2·06	4·58	2·63
August.........	1·61	3·37	2·06	1·92	1·69	3·22	2·35
September	2·80	2·31	2·05	1·75	1·42	1·98	1·79
October	0·27	0·55	−0·21	0·34	−0·19	−0·01	0·50
November	0·58	0·31	−0·20	0·63	0·08	−1·02	0·24
December	−0·22	0·98	1·00	1·57	1·22	0·94	1·51

	Smetschna	Rotenhaus	Hohenelb.	Tetschen.	Marietta.	Concord.	Montreal.
January	5·59	5·39	4·45	6·79	−2·20	1·47	−1·51
February	0·79	1·14	0·68	1·86	3·30	1·55	3·22
March	0·18	0·24	−0·73	0·17	0·03	0·59	−0·91
April	−1·24	1·68	−1·54	−0·93	0·47	0·87	0·95
May	2·14	1·70	1·12	2·37	−1·52	−1·63	−1·87
June............	2·10	0·65	0·61	1·27	0·23	−0·71	−1·79
July	2·98	2·58	2·79	3·07	1·23	2·00	1·20
August.........	2·37	2·33	2·78	3·05	0·43	−0·63	−1·01
September	1·97	2·24	1·41	1·69	−0·28	0·91	0·66
October	0·35	0·25	−0·50	0·38	−1·40	−0·38	−1·50
November	0·28	0·39	−0·21	0·68	−0·10	−1·38	−0·11
December	1·50	1·02	0·25	1·92	0·44	−1·42	−3·20

	Reikiavig.	Stromness.	Clunie Manse.	Applegarth Manse.	Boston.	Bedford.	London.
January	−1·47	0·24	1·45	2·62	3·61	4·05	3·71
February	0·05	0·51	0·51	0·75	0·80	0·89	0·44
March	0·16	−0·05	0·48	0·17	0·86	1·30	0·86
April	−0·23	−0·38	0·20	0·02	0·09	−0·04	−0·70
May	−1·15	−0·06	0·56	−0·50	0·67	0·24	1·13
June............	−1·79	1·56	−0·03	−0·19	0·96	0·62	0·75

	Reikiavig.	Stromness.	Clunie Manse.	Applegarth Manse.	Boston.	Bedford.	London.
July	−1·11	0·62	0·64	0·49	1·01	0·88	0·86
August.........	−2·06	0·59	0·38	0·01	1·16	1·11	1·04
September	−1·30	−0·08	0·05	−0·38	1·02	1·23	1·14
October	−1·53	−0·70	0·01	−0·52	−0·16	−0·03	−0·22
November	−0·01	0·44	0·35	−0·37	1·03	0·60	0·35
December	2·14	1·72	0·48	0·29	0·54	0·19	0·07

	Paris.	Haarlem.	Salzuflen.	Zittau.	Dresden.	Berlin.	Petersburg.	Kasan.
January	4·46	4·83	5·18	4·78	5·38	5·33	−2·40	−2·59
February	−0·43	1·23	0·60	1·29	1·05	1·20	0·25	−4·65
March	0·41	1·00	0·57	−0·22	0·04	0·81	1·81	3·57
April	−1·13	−0·33	−1·27	−1·21	−1·01	−1·11	−0·14	1·74
May	1·06	0·93	1·09	1·75	1·61	1·54	0·19	−0·05
June............	0·50	0·52	0·20	1·49	0·94	0·99	−1·67	0·21
July	0·82	1·45	2·23	3·92	2·78	3·26	−0·34	−2·00
August.........	1·01	1·67	1·70	3·02	2·42	2·64	2·01	1·30
September.....	1·67	1·42	0·53	1·92	1·68	1·17	−0·10	−0·07
October	0·00	−0·13	−0·68	−0·08	0·59	0·09	−0·08	1·88
November	0·12	0·19	0·10	0·34	1·28	0·76	−0·35	1·54
December	0·01	1·30	1·49	0·95	1·65	1·33	1·29	2·37

Before we proced to draw any conclusions from the above tables we must make some remarks as to the degree of confidence to be placed in the numerical data contained therein. On a simple inspection we at once see that the more considerable variations from the mean distribution of temperature do not appear as local phænomena, but extend over a large space. The quantitative agreement in places which are not too far apart, the regular increase or decrease of the differences when we pass over the surface of the earth in any particular direction, must certainly convince us that at all times of the year these variations are produced by general causes, and that (at least for so long a space of time as a month), in opposition to these the local perturbations, such as are caused by cloudy skies or precipitation, may be regarded as entirely subordinate phænomena. We may therefore be allowed to use these differences as "correcting elements;" and if we have the mean monthly temperature of a place from observations of *only a few years*, by which it is rendered unsafe, we may correct it by means of the monthly temperature of some adjoining locality, and thus we may be enabled to construct monthly isothermal lines, which in the present state of Meteorology can no longer be dispensed with.

When we find that the observations of any one place differ considerably and continually (and always on the same side) from those of adjoining places, we may assume that there have been errors in the observations; such errors may be caused by a change of instruments, a change of their situation, or of the time of observing. From these tables then the following conclusions may be drawn:—

First: "The tropical atmosphere of the Indian basin does not appear to exert any visible influence on the weather of Europe." It cannot be denied that the earlier or later setting-in of the monsoons in particular years stands in connexion with the preceding and following movements of the atmosphere; but from the observations at Madras we are not able to trace any direct connexion with the thermal relations.

Secondly: "The temperature of the zone of the trade winds in the Atlantic Ocean is in evident connexion with the weather of the neighbouring temperate zone." As proof of this may be adduced the warm winter of 1827–28, and the low temperature of 1829. The uncommonly high temperature of January and February, 1828, in Havanna, is still seen very plainly at Marietta, and it is also very evident at Salem, Boston, and Concord, up to Montreal, and is also visible in the observations at New Haven, N. Bedford, Cambridge, Clinton, Lowville, Washington, Hudson, Albany, Middleburg, Onondaga, Auburn, Cherry Valley, and Canandaigua; and it is remarkable that in the more southern localities, January was relatively the warmest;—in the more northern on the contrary, February. The extreme cold in Europe from November 1829 to February 1830, first makes its appearance at Havanna in January 1830, and remains there until April, at which time a milder weather has commenced in the temperate zone.

Thirdly: "The cold of winter generally extends itself from north to south, but an unusual warmth moves in an opposite direction. By north and south, we mean these directions as modified by the rotation of the earth." Regarding the more accurate direction in which an unusual extreme extends itself; this will depend in the first place upon the spot where it first appears in its greatest power, and upon the relative position of the places of observation to this spot. In January 1814 this locality appears to have been situated to the north of Sweden, for the January was coldest in Stockholm, Torneo, Carlisle, Dunfermline, and London; these places being named in the order of the intensity of the cold. This cold was very evident in northern Germany, but vanished at the Alps; indeed in Italy there was a mild temperature increasing southwards. In February the cold had decreased at the above-named places; but was intense from Dantzic to Milan, and was inconsiderable even in Palermo; the maximum fell on Berlin. In March the maximum was in southern Germany and northern Italy, and in April this cold was replaced by a generally-distributed warmth.

The propagation of an unusual warmth in winter is generally attended by stormy south winds, and is therefore too rapid to be very distinctly shown in the monthly means. Frequently a very warm south-west wind meets a very cold north-east one; in the localities situated to the south there is at such times an exceedingly mild temperature, and in the northern parts an intense cold, and on the borders we find very variable weather. As recent examples, we may mention December 1839, when Berlin was several times included in the cold stream, in southern Germany it was very warm, and in Petersburg on the contrary, intensely cold. But even in the monthly means the same phænomena are evidently visible. In December 1838 the cold which prevailed in Europe extended from Torneo to Palermo, decreasing towards the west and not sensible in America. In January 1809 the whole of southern Europe enjoyed very mild weather, while the cold, which was probably prevented from extending southward by south winds, was concentrated in northern Europe, and increased from Berlin to Dantzic, Stockholm and Torneo; it then passed off westward, for it increased considerably in Scotland, and was felt in America. In February the warmth extended up to Dantzic, the cold remained only in Stockholm and Torneo, but was considerable in America. In March it forced its way again southwards, for it again appeared in Dantzic and Berlin. In April it spread over all Europe, but had left America.

A diminution of temperature in winter is principally caused by north winds; but in spring it is often a secondary phænomenon. The dispersion of a south by a north wind is in general accompanied by a fall of snow, which on lofty mountains takes place at a great height even during the continuance of the south stream. The masses of snow which are thus collected during the winter exert their refrigerating influence on the spring warmth which appears on the plain. Perhaps from this reason the "second winters" of 1807 and 1808 reached their maximum near the Alps. If we consider more closely the march of the differences in the tables, we see that the amount of deviation is greatest at one particular spot, from which it decreases in every direction. If, for instance, we find the maximum of heat or cold in the middle part of Germany, we may expect smaller differences in the N. S. E. and W.; but if we pass certain limits we find great deviations in the opposite sense. We find however that the decrease in the amount of deviation does not take place with the same rapidity in all directions, it proceeds slower in the north and south than in the east and west direction.

Fourthly: "Similar relations of the weather are more frequent from south to north than from east to west." In cases of very great deviations we have in the latter direction a double opposition between Europe on the one hand and America and Asia on the other. The temperatures of the winter of 1821–22, and of the January of 1834, in Europe, were probably so high in consequence of the severe cold in Asia and America. In December 1829 the maximum of cold was at Berlin; it was still very visible at Kasan; but in Irkutzk it was mild, while America enjoyed very warm weather. But generally Europe partakes of the same distribution with one of its neighbours. Sometimes Europe, Asia and America have all the same kind of weather, and in this case the opposites are found either in the north or the south.

If these phænomena are due to conflicting and alternately prevailing currents, it is evident that we may determine the breadth of these currents; if the chain of observations be extended over a larger space than the currents occupy, we shall find somewhere or other the opposite extremes. Egede Saabye says of Greenland, "The Danes have remarked that when there is a severe winter in Denmark there is a mild one in Greenland, and *vice versa.*" The same applies to Iceland; and indeed so much so, that the transport of goods thither from Denmark partly depends upon it. If this distinction were one of north and south there could not be so considerable a disagreement between Petersburg and Reikiavig, as is shown in the following tables:—

Petersburg.

	1822.	1823.	1824.	1825.	1826.	1827.
January	2·80	−2·48	2·79	3·80	−1·26	2·52
February	4·68	−2·19	0·93	1·11	0·75	−1·30
March	4·09	2·80	1·03	0·08	2·08	1·38
April	2·62	−1·42	0·36	−0·94	0·95	3·85
May	0·29	−0·71	−0·69	−1·86	3·34	2·17
June	−2·02	1·02	−2·34	−0·30	2·10	1·39
July	−0·12	−0·29	−2·00	−2·14	2·82	−0·58
August	−0·27	0·61	−1·72	−0·47	2·17	0·37
September	0·33	−0·04	2·05	−1·94	0·25	0·76
October	0·35	1·43	−0·84	0·40	1·87	−0·98
November	0·99	−1·87	0·63	2·31	2·34	−1·43
December	2·57	2·17	1·80	−0·29	3·99	1·07

Reikiavig.

	1822.	1823.	1824.	1825.	1826.	1827.
January	....	1·77	−0·35	1·10	−0·22	−0·75
February	....	−0·52	0·65	0·36	−2·88	0·97
March	....	0·28	−0·17	2·92	−2·03	−3·92
April	....	1·95	2·02	0·84	−0·93	−1·00
May	....	−0·75	2·80	0·35	1·43	0·52
June	....	0·03	4·60	0·30	−1·13	0·83
July	....	2·47	3·15	1·73	−0·72	0·17
August	....	1·76	1·53	0·66	−0·18	1·73
September	....	0·84	−0·73	2·34	1·24	0·64
October	....	−1·49	−2·36	1·69	1·13	2·30
November	....	0·27	−3·55	−0·72	0·45	2·35
December	....	−0·86	−3·99	−0·92	1·17	0·88

In a treatise published in Poggendorff's Annals (xxiii. p. 54), Professor Dove has endeavoured to prove that the form of the isothermal lines does not depend entirely upon the direction of the wind, and he finds in the above results a new proof for his assertion. As the temporary thermal relations depend upon temporary currents, so, if the same were the case with the mean relation, our results ought to show an agreement between places which are isothermally connected. As regards the mean distribution of temperature, it must be allowed that Iceland is much more closely connected with Europe than with North America. But as in the variations from the mean it agrees more with North America, we see that the cause of this variation cannot also be the principal cause for its isothermal position in the general distribution of temperature, or in other words, "the mean direction of the wind exerts a smaller influence on the mean temperature of a place than the temporary direction does on its modifications." The different distribution of land and sea may make a great difference in the thermic effect of the same mean direction of the wind at places which are far apart.

If we consider the isothermals as fixed curves, constructed on the surface of the earth, we do so erroneously. The lines in reality wander between two extremes, which we call the isotheral and isochimenal lines; they have moreover various curvatures, which in different years are not always the same at the same season. These non-periodic changes of form are caused by winds which extend over large surfaces, and therefore at certain places the change of form in the curves which follow each other takes place in one direction, while at other places the direction of change is exactly opposite. Out of the several directions of the wind, which are continually varying at any particular spot, we may deduce one mean direction which is distributed over a large surface, which, if not exactly everywhere identical, at least changes uniformly. Within its range the variations from the mean take place generally in the same sense, and this range will form a "weather system." The determination of these systems would be the same for Meteorology as that of the isothermals is for Climatology.

The continual movement of the atmosphere prevents the existence of a distinct line of separation between two neighbouring systems, and the more distinctly separated they have existed at one time, with so much the more violence will they afterwards modify each other. If in winter severe cold and mild weather border on each other in a direction from west to east, the spring will appear early in that system in which the winter has been mild, at the same time that in the other system there is a temperature perhaps but little above zero, because all the heat that is produced is employed in melting the large masses of ice which have been formed. The warm air of the one system will not always be able to resist the pressure of the cold air of the other. The more rapidly the heat has risen the more sudden will be the entrance of the cold air. On this account the spring will be unpleasant from frequent changes between warm and rough weather. These changes are usually repeated several times with diminished force, and at length cease altogether suddenly, for then the summer weather has obtained the ascendency.

As an instance of this may be mentioned the rough spring of 1835, for remarks on which *vide* Dove, Pogg. Ann. xxxvi. p. 318. In the tables, which embrace a period of fifty years, we find several instances of a variation from the mean distribution, which was of considerable duration. The cold which prevailed from June 1815 to December 1816, produced a fearful sterility in western Europe, while Odessa, on account of the mild temperature of eastern Europe, owes to it its rise in the scale of commercial towns, inasmuch as its export of corn in the years 1815 to 1817 rose from eleven to thirty-eight millions of rubles. The maximum of cold in this period fell on England, and America participated in it. There appears no foundation for the opinion that a hot summer succeeds a cold winter, and a cool summer a mild winter. In the hot summer of 1822, in Berlin, no ices could be obtained at the confectioners, for the winter had been so mild that no ice had been collected. The hot summer of 1834 also followed a very mild winter, and was moreover succeeded by one. The very severe winter of 1829–30 followed a very cool summer. To make a good wine year it seems that the summer must be preceded by a very mild winter or spring, so it was at least in 1811, 1819, 1822, and 1834.

As in the distribution of the positive and negative signs, in the space of fifty years, we do not perceive any gradually increasing preponderance of one over the other, we may conclude that there has been no change in the climate during that period.

It cannot be asserted that in any particular direction an agreement or variation is more frequently seen at one period of the year than at another. The agreement in the direction of east and west seems to be of more frequent occurrence in summer than in winter. Deviations, in the same sense, from the normal state, appear to be of longer duration in the frigid than in the temperate zone. The rare change of signs is a proof that the mass of air of these regions resists the invasion of the atmosphere from lower latitudes: it either allows no entrance to the warm air, or else gives way for a length of time; the direction in which this mass then flows off must exert a material influence on the adjoining parts of the temperate zone. At the end of 1824 the cold in Torneo resisted for a long time the attacks of the heat from southern Europe; at last, in January, 1825, it gave way, and the weather became mild, while in central Europe the temperature sank considerably. In 1810 the cold of the frigid zone had a decided influence on the north of Europe. Often, on the other hand, the extremes are confined to the frigid zone. The winter of 1798–1799 was very severe in Europe, but mild in Umeo; while, in 1803–1804, there was a severe winter in Umeo, but mild weather in the rest of Europe. If we compare the description of the Arctic regions of North America by British navigators, with Wrangel's account of north Siberia, we perceive that here also important differences in the atmospheric relations arise from the configuration of the land.

It appears therefore, from the above researches, that there is always an equal quantity of heat existing over the surface of the earth, but differently distributed at different times. Every extreme in one place is counterpoised by a contrary extreme in another; we have, therefore, no right to assume any other external sources of heat than that of the sun. When the chain of observations shall be extended over a greater surface, more exact conclusions may be drawn, and the secondary phænomena distinguished from those that are primary. The tables may also be used to settle other questions: for instance, "Whether there is any connexion between the appearance of comets, and the meteorological phænomena of the period? Whether volcanic eruptions or earthquakes are independent of atmospheric changes? or whether they reciprocally produce each other? Observations at only one place are not sufficient to solve these questions; we must know whether the relations on the earth were at the time of a comet, or of a widely extended earthquake, normal or abnormal; whether the variations followed or preceded the phænomena. A certain connexion between volcanic occurrences, and great extremes of temperature, seems indeed to exist.

(*End of the First Part.*)

The volume, of which the preceding is an abstract, was published in May 1840; in 1841 Prófessor Dove published a second part, containing many additional tables, particularly those containing observations from North America, by which more light is thrown upon the opposite meteorological relations which frequently prevail in Europe and America. It has been shown above that the variation from the mean distribution of temperature, as deduced from the observations of many years, is not a local phænomenon, but extends over a large surface, and therefore the variations are produced at all times of the year by causes which exert a general influence, and that for so long a space of time as a month, the local perturbations, as caused by a clouded sky or a precipitate, may be regarded as subordinate elements.

In constructing monthly isothermals, however, it is necessary to determine this subordinate element, in order to correct the observations for a few years of one place by the long-continued ones of a neighbouring station. To determine the magnitude of the local perturbations, it is not advisable to choose places which are distributed over a large extent of country, but rather groups of stations situated close together; the places of observation in the state of New York, on the one hand, and in Suabia, Bavaria, Belgium, Bohemia, and Saxony, on the other, furnish two groups well suited for this purpose.

A second question, which was only partially answered in the first part is, In what direction does any abnormal temperature propagate itself over the surface of the earth? In examining this point the monthly means could not be employed, because extremes of short duration are frequently obliterated during that time by deviations in an opposite sense. The extension of unusual warmth in winter, by the action of south-west winds, generally takes place too rapidly for its advance from station to station, to be traced by the monthly means; it was, therefore, advisable to employ *five-day* means, and for this purpose the calculations of Brandes at Leipzig, and Suckow at Jena, and also those of Schrönn were taken. Brandes' calculations are made from the observations of 9 years at Petersburg,

6	„	„	Sagan,
9	„	„	Rochelle,
12	„	„	Manheim,
10	„	„	St. Gothard,
10	„	„	Rome,
5	„	„	Zwanenburg.

and also for 27 years at Jena.

Here follow the tables, of which the annexed is a specimen, containing the five-day means at Petersburg during the months of August and September.

August.							
	1.	6.	11.	16.	21.	26.	31.
1783	13·00	11·29	14·11	18·63	15·47	14·32	7·04
1784	18·10	17·18	15·22	15·50	13·09	13·40	10·73
1785	11·98	10·95	14·76	14·90	16·43	12·44	7·47
1786	15·93	15·50	14·22	13·30	12·09	12·94	10·84
1787	...	...	...	...	...	...	...
1788	13·42	11·80	11·93	12·73	12·52	13·69	11·94
1789	15·97	14·51	13·89	13·65	13·51	15·77	13·48
1790	14·45	13·11	11·63	9·97	8·01	9·38	8·31
1791	15·62	13·20	10·84	11·51	11·22	8·97	10·90
1792	14·10	11·19	10·39	12·09	12·09	13·20	8·45
Means	14·730	13·192	12·999	13·304	12·714	12·679	9·907

September.						
	5.	10.	15.	20.	25.	30.
1783	7·65	8·57	10·06	8·99	9·67	7·48
1784	9·33	7·64	3·66	6·79	5·95	1·67
1785	10·06	9·74	6·76	3·58	5·97	3·56
1786	10·45	9·85	10·24	8·18	4·66	6·11
1787	...	...	...	...	...	...
1788	10·99	11·49	9·68	5·42	9·27	7·70
1789	11·41	10·31	8·67	10·20	6·12	6·72
1790	7·54	7·33	8·23	7·17	5·16	2·89
1791	10·98	8·29	8·74	5·37	3·17	3·31
1792	9·85	11·38	8·51	10·48	12·48	6·87
Means...	9·807	9·400	8·283	7·353	6·939	5·15

After these follow tables of monthly means for 117 stations in North America and Newfoundland, as also tables of observations made by the North Pole expeditions at various places.

Next follow tables of monthly means for 43 stations in Bohemia and Wurtemburg, for 27 stations in England and Scotland, and for 30 stations in Germany, Switzerland, and the Austrian dominions, making in all, in the two parts of the work, 291 stations.

Then follow tables, showing, first, the mean temperature of every five days in the year, for the 8 stations mentioned above, drawn from the whole number of years of observation; secondly, the deviations in each single year from the general means.

In order to determine the magnitude of the local deviations, two systems of stations are formed; the first contains 32 stations, of which 23 are in North America, and 9 on the western coasts of Europe. The second system comprehends 25 stations, from the southern declivity of the Alps, to Helsingfors in Finland; the means for each system for the same epochs are first given, and then the simultaneous deviations from those normal temperatures*.

Professor Dove remarks that the inspection of the tables gives a clearer view of the distribution of temperature than any verbal description can do, as in the vertical columns the eye traces in one direction the succession of the phænomena, and in the horizontal ones their simultaneous existence. The following principal results, however, may be mentioned:—

In considering the results of the five-day means, we observe that the deviations with negative signs make their appearance in two different manners; either they extend over the whole ground of observation, and their relative maximum is found in the northern stations, more particularly at St. Petersburg; or they are distributed over the middle and southern stations, in which case the maximum is at St. Gothard, while at Petersburg we find large *positive* deviations. The diminution of temperature of the first kind appears to be generally more lasting than that of the second, and it is frequently very evident that the absolute extreme appears first in the northern regions. During the continuance of such low temperatures, the deviations on the St. Gothard do not appear to be very large, but even smaller than on the plains. There are therefore two kinds of diminution of temperature,—one of very extensive action, which seems to have its source in high latitudes, and one more local in its effects, which comes down from the mountains. The fact of the cold of the first kind being more intense on the plains than on the mountains, proves that the cooling polar current is principally in the lower regions of the atmosphere, while the warm equatorial current flows over it in an opposite direction. The abnormal effect of the mountain cold appears from the tables to

* The extent of the tables in question precludes the possibility of their repetition in these Memoirs: the reader who desires to study them adequately and fully, is referred to the original work. It is hoped that the present abstract contains all that is essential towards enabling the *English* reader to derive the full benefit from the inspection of the valuable numerical data and results, which form by far the larger portion of the work, besides placing before him the principal inferences which are drawn in the text.

be nearly equal at Rome and at Manheim; the polar cold is, on the other hand, much less at the former than at the latter station. The end of July, middle of September, and beginning of October 1789, offer good examples of the mountain cold, as do also February, March, and June 1791, and September 1792. In the middle of January of the last-named year, an increased temperature, spreading itself northwards from Rome, gradually repelled the cold which had previously prevailed to beyond Petersburg; in February the cold polar current gained the mastery, and advanced towards the south. In the beginning of 1786 the maximum of polar cold was in Petersburg; by the end of February the cold had spread over the whole field of observation, being inconsiderable only in Rome, as was the case in November of the same year. In December 1788, both causes probably acted together.

We see that the cooling influence of even such mountains as the Alps is inferior to that of extensive currents of air, and that they do not form such a complete separating barrier to the weather as might have been imagined *à priori*; the influence of the inequalities of the terrestrial surface, on the general meteorological relation, can therefore only be considered as subordinate in character.

Quantitative equality, in the abnormal deviations at neighbouring places during the same epoch, points to the undisturbed action of general causes; considerable differences in the amount of such deviations indicate local influences, which modify the action of these general causes differently at different places. A consideration of the Tables I. and II. appears to show that such local influences are less in England and Germany than in America, which is probably owing to the more partial distribution of cultivation in the latter; however, in both continents local influences are very inferior to general ones.

Table III. contains some striking confirmations of the conclusions already drawn in the first part as to the occurrence of different extremes adjoining each other in an east and west direction. The severe cold of January 1838, and November 1829, in Europe, was fully compensated by the simultaneous high temperatures in North America, whither the cold did not make its way until February. In January 1837 the reverse took place, the weather being cold in America, and mild in Europe.

In February 1839 the temperature was high in America, and normal in Europe, probably the negative extreme was further to the east.

The *mean variability* increases in America as well as in Europe, in proceeding from the coasts towards the interior; the American stations are not yet sufficient to show whether it is greater on the borders of the sea and continental climates, than still further in the interior.

Under the same parallels of latitude the mean variability is greater in America than in Europe, probably because in America the isothermals are nearer together, and deviate more from the direction of the parallels of latitude, so that corresponding changes in the direction of the winds produce greater thermal effects.

The comparison of the mean temperatures of 1828–1834, with the much lower ones of 1835–1839, shows plainly that there are causes of either a warming or a cooling nature, which often prevail uninterruptedly for a considerable space of time.

H. C.

December 17th, 1841.
35 Upper Gower Street.

Article X.

On the Azotized Nutritive Principles of Plants. By Professor Liebig*.

[From the *Annalen der Chemie und Pharmacie*, August, 1841.]

THE vegetable kingdom contains many azotized compounds of various characters, existing as component parts of plants. Many of these compounds are peculiar to certain genera of plants; some are found in species only, and not in every individual species of the same family; others in two or more species of different families. They are in general remarkable for their peculiar action on the animal organization; this action is poisonous, or, what is commonly called medicinal; but they are found only in minute quantity in the fruit, leaves, or roots of the plants in which they exist. All organic bases, such as caffein, asparagin, and piperin, belong to this class of bodies. They appear to be incapable of replacing the loss of matter sustained in animals by the action of the vital process, or of increasing in a perceptible degree the size of any organ; partly for this reason, and partly because those matters which serve as nourishment are wanting in them, and they are eaten in very small quantities only, they cannot be considered as nutriment.

But there is another class of azotized compounds most extensively diffused, although their number is small; one of these three or four substances appears in all plants without exception, the other three are found only as ingredients in certain families. These three substances, namely vegetable albumen, gluten, and legumin, are, properly speaking, the azotized nutritive principles of plants.

Vegetable albumen, which is distinguished by its solubility in water, is found in the juices of plants, but chiefly in oleaginous seeds.

Gluten is one of the chief ingredients of the seeds of the cereals; legumin is found in leguminous plants, chiefly in beans, peas, and lentils. These, with another substance, which I shall call vegetable fibrin, form the proper nutriment of graminivorous animals, from which their blood is produced, and from which all

* Translated by Robert Smith, Ph. D.

the azotized portions of their bodies take their rise. It is in the vegetable kingdom that the nourishment of animals in general is prepared; for, strictly speaking, carnivorous animals, when they consume other animals which have fed on vegetables, consume only those vegetable principles which have served the latter as nourishment. Every azotized animal substance has consequently its origin in plants.

It is well known that carnivorous and graminivorous animals have very different digestive organs, but that the assimilation is performed in both by the blood; the comparison becomes therefore a most important subject for the physiologist.

In carnivorous animals the process of nutrition is very simple; the nourishment they take is identically the same as the principal component parts of their own bodies: the flesh, blood, membranes, &c. which they consume, are in no respect different, chemically speaking, from their own flesh and blood. The food of carnivorous animals assumes a new form in the stomach and organs of digestion, but its chemical composition suffers no change; it is made soluble, and therefore becomes transferable to the different parts of the body, taking again the form of blood from which it originated. In that class of animals the vital action of the organs in digestion and the formation of blood, is confined to a mere change of the condition of the nutritive matter, as it is all capable of assimilation in the state in which it is taken; and such substances only pass unchanged through the alimentary canal, as the excess of inorganic substances in the food, and the earthy matter of the bones, with insoluble salts of magnesia. The process of nutrition in graminivorous animals appears much more complicated; their digestive organs are more complex, and their food has much less resemblance to the constituents of their bodies.

All those parts of plants which serve as nourishment to graminivorous animals, contain, besides the azotized compounds named, certain others absolutely necessary for the support of life, which yet contain no nitrogen. These compounds, among which are sugar, amylin, and gum, are evidently applied to some particular purpose, as they disappear in the organization; they, no doubt, take a part in certain processes, which, in carnivorous animals, are conducted in a different manner.

Before being able to decide with certainty as to the part in the vital processes of animals, performed by the substances destitute of nitrogen, it is necessary to know the composition of those vegetable compounds which contain that element.

If it is found, on inquiry, that the composition of vegetable albumen, gluten, fibrin, and legumin, differs from that of the blood of animals, or from that of the albumen and fibrin which they contain, it is clear that the starch, sugar, and gum must give up some portion of their elements to compensate for the difference.

If it is found, for example, that vegetable albumen contains the same quantity of nitrogen as animal albumen, but a smaller proportion of carbon, or that vegetable fibrin contains less carbon than animal fibrin, the necessity would be clearly seen, of adding to these azotized vegetable substances the elements of such bodies as sugar, amylin, and gum, so as to compose animal albumen and fibrin; or, in other words, to form blood. If vegetable albumen and fibrin were deficient in carbon, this want would be found to be supplied by the sugar, gum, and starch; for it is remarkable that these latter bodies contain only carbon and the elements of water, and they would add nothing to the azotized compounds but that carbon in which they are supposed deficient. But if, on the other hand, it is proved that the azotized nutritive principles of plants have the same composition as blood, or as albumen and fibrin, then, in whatever way the assimilation of the nutriment is conducted, it is clear that the carbon of starch and the other compounds destitute of nitrogen cannot possibly be consumed in the formation of blood. If vegetable albumen and fibrin possess the same proportion of carbon and nitrogen as the animal principles of the same name, the latter have no need of the carbon of the sugar or starch; as we cannot suppose that the one substance should give up a part of its carbon to receive an equal quantity of the same element from another substance; such an idea would refute itself. Thirdly, if azotized vegetable principles contain a greater proportion of carbon than the component parts of animals, to an equal proportion of nitrogen, then every probability at once ceases that sugar, gum, or starch should be used in the formation of the azotized animal compounds, or in supplying the place of what is consumed, because the azotized vegetable principles must lose their excess of carbon to become either blood or muscular fibre.

No organ employed in performing any vital function in animals, nor any essential constituent of such an organ, is destitute of nitrogen; the only animal substances indeed which do not contain that element, are water and fat, both without any distinct form, both without vital action, and serving only as connexions in the organization. Animals must receive as much nitrogen in their food, as is excreted and removed from the body by the decomposition of the organs; young animals must receive more, for the growth of these organs. When the proper quantity of nitrogen necessary for the reproduction of the decomposed parts is not found in the nutriment, the equilibrium is of course destroyed, the body can no longer grow, but must, on the contrary, decrease. Experiments have been made on the nutrition of animals, by means of sugar, gum, and starch, and it has been sufficiently proved that the vital action of animals is inadequate to the production of nitrogen, or any other element; for animals fed only with the nutriments mentioned invariably die of starvation.

The first case supposed above is the only one in which substances destitute of nitrogen can be of use in supplying the loss sustained in the organs of animals; namely, if the azotized principles of vegetables contain the same number of atoms of nitrogen, but a smaller proportion of carbon, than the blood and other component parts of animals. Then may sugar, and bodies of a similar composition, be available in the formation of the organism of animals, by imparting carbon.

Several talented and skilful chemists and physicians have devoted themselves, in this laboratory, during the past year, to the investigation of the composition of albumen, fibrin, the membranes, and gelatinous parts of animals, and the azotized compounds of vegetables. The preparation of the vegetable substances for analysis was undertaken by myself, and it is my object at present to communicate the principal conclusions of the whole inquiry.

Drs. Scherer and Jones will publish, in their own papers, a description of their experiments, with the more detailed results of their analyses; and I shall content myself at present with speaking only of their general relations, and of the results, which are independent of the percentage of the elements.

Fibrin, albumen, and casein, as they are found in nature, differ very much in their external properties and structure, and in their relations towards water and heat.

Fibrin derived from the blood is perfectly insoluble in cold water; the liquid albumen of the blood may be mixed with any quantity of water, and the albumen of the egg is soluble in water.

Fresh prepared fibrin has the form of transparent, soft, elastic threads, which are not at all glutinous, and cannot be united by kneading. When a solution of albumen is heated to a certain temperature, it coagulates into a white, soft, elastic mass, which cannot be kneaded. Albumen is not precipitated from its solution in water by acetic acid. The casein of the milk of animals, which is the chief nourishment of their young, is also distinguished from fibrin, by solubility in water. Heat does not coagulate the solution of casein like that of albumen, but a pellicle is formed by evaporation on the surface of the former solution, which, if removed, is continually renewed. Casein also is precipitated from solution by acetic acid, as a thick, coherent mass, or curd. Fibrin, albumen, and casein, comport themselves in the same manner with hydrochloric acid. They are dissolved by that acid with the aid of heat; and if the solution is exposed to a somewhat high temperature for a length of time, it assumes first a beautiful lilac, and then a violet blue colour. At this point of the decomposition, carbonate of ammonia, and other reagents, act in the same manner in all the three solutions.

Fibrin, albumen, and casein, possess the same composition, according to all the analyses hitherto made; and these analyses have been so often repeated, that no doubt can be entertained of the accuracy of the chief results. The proportion of organic elements being the same, they must be arranged in a different order, to account for the difference in properties of these principles. The gas obtained by burning any of these three substances with oxide of copper, in the ordinary process of analysis, is found to be a mixture of nitrogen and carbonic acid. When executed according to the qualitative method, in which relative quantities only are obtained, 8 volumes of the gas gave very nearly 7 volumes of carbonic acid, and 1 volume of nitrogen. When analysed so as to ascertain absolute quantities, or according to the new method of Drs. Will and Varrentrapp, by estimating the ammonia obtained from them, the atomic proportion was found to be as 8 : 1; or these bodies give 8 volumes of carbonic acid to 1 volume of nitrogen.

The proportion of carbon obtained, when these substances are burnt with oxide of copper, is smaller, because it is difficult to cause them to undergo complete combustion; but if chromate of lead is used, in the place of oxide of copper, and due precautions taken, the analysis is much more accurate, and the quantity of carbon approaches nearer to that obtained by the direct methods. The dried flesh of animals, and the dried muscular fibre of the ox, freed from fat, give, when burnt with oxide of copper, 1 volume of nitrogen to 7 volumes of carbonic acid. The result is the same with pure albumen, burnt in the same manner. The muscular fibre of the ox and roe, boiled or roasted, blood dried at 212°, and the dried flesh of fish, such as pike and cod, which so much resembles coagulated albumen, all give the same proportions of gas—(Dr. Playfair). Proceeding, then, on the foundation which these experiments afford, to compare the composition of azotized vegetable substances with the principal component parts of animals, a most important fact is at once discovered, namely, that all those nutritive vegetable principles, whatever they may be, possess either the same composition as fibrin, albumen, and casein, or, if the percentage be different, still have the same proportion of nitrogen and carbon as the animal substances possess. It is remarkable also that this resemblance goes still further, for these vegetable substances conduct themselves in a similar manner with chemical reagents, so that we may say that their form is merely changed, when animals produce blood and muscular fibre from them; for they are obtained from plants in a perfect state, as far as the proportion of their elements is concerned.

Graminivorous animals are fed on vegetable albumen, fibrin, and casein, which have therefore, chemically considered, the same composition, and in most cases the same properties, as their own blood, albumen, and muscular fibre.

The azotized principles of vegetables may be divided into three modifications, from their behaviour towards ammonia and acetic acid, at the ordinary temperature. I have given the name of *vegetable fibrin* to that ingredient of the cereals, of wheat, rye, barley, oats, buck-wheat, maize, and rice, which is insoluble in water and ammonia. It is not found in leguminous plants.

The name *vegetable albumen* is applied by me to that azotized ingredient of the juice or other parts of plants which is held in solution, or is soluble in water and coagulates like animal albumen, when boiled, and is not precipitated from solution by acetic acid.

Vegetable casein is soluble in cold water: its solution does not coagulate. Acids cause a precipitate in its solution which is soluble in ammonia, but insoluble in dilute acetic acid.

Vegetable casein is obtained from leguminous plants, such as beans, peas, and lentils, in the following manner:—warm water is poured over them, and they are allowed to soften for some hours, until they can be rubbed in a mortar to a syrupy consistence. Five or six volumes of water are then poured upon the mass, and the whole is thrown upon a fine sieve, through which the solution of vegetable casein flows, mixed with starch. It is now allowed to stand an hour, or two, till the starch is deposited.

The liquid which contains the vegetable casein in solution is then drawn off from the starch; it is not clear, but milky, and of a yellowish tinge. This cloudiness arises, partly from an admixture of fatty or waxy substances, and partly from a continued precipitation of the vegetable casein; a few drops of ammonia make the liquid somewhat clearer.

The substance of leguminous plants, finely pulverized and washed in cold water, has no action on vegetable colours; but if allowed to stand for some hours in water, it becomes slightly acid; this is the cause of the cloudiness of the last solution, and of the precipitation of the casein prepared from these plants. Boiling does not cause the slightest coagulation in the solution of vegetable casein; but a skin is formed on the surface of the liquid when evaporated, and is renewed as often as we remove it, exactly as in heated milk.

All acids, without exception, coagulate the solution; it becomes flocculent and of the consistence of jelly on the addition of acetic acid, which acid, even in excess, cannot dissolve coagulated vegetable casein. Tartaric and oxalic acid, in excess, dissolve the precipitates which they cause. Sulphuric and nitric acids precipitate the last solutions anew. The casein of milk comports itself exactly like vegetable casein; it is coagulated by acetic, tartaric and oxalic acids, the precipitate is dissolved in an excess of the latter two, and reappears on adding sulphuric or hydrochloric acid.

An acid reaction is observable in all the precipitates of vegetable casein produced by acids; they are, in fact, compounds formed with the acids, and react in every respect as the corresponding combinations of the casein of milk. Alcohol also coagulates it as it does milk. When the solution of vegetable casein, as it is obtained from beans, peas, and lentils, is allowed to stand for twenty-four hours, at a temperature of from 60° to 70° Fahr., a gelatinous precipitate is formed much resembling caseum. The supernatant liquid is of a greenish yellow colour, and has a decidedly acid reaction; a little gas is at the same time seen to escape. This acid is the lactic; for when it is evaporated with oxide of zinc, crystals are formed, possessing the characteristic insolubility of the lactate of zinc.

The coagulum is a lactate of vegetable casein; it has an acid reaction, and cannot be obtained otherwise by the longest washing with water or alcohol. The lactate of vegetable casein is very soluble in ammonia and the alkalies; alcohol and æther extract from it a green fatty matter. When an alkaline solution of vegetable casein is kept boiling for some time with an excess of potash, the addition of dilute sulphuric acid causes a precipitate, and the escape of sulphuretted hydrogen. Vegetable casein conducts itself with the salts of the earths and metals, exactly like the casein of milk.

The sulphate of magnesia, the acetate and other salts of lime, are not precipitated by an aqueous solution of pure animal casein, when cold; but the slightest heat causes immediate coagulation. Vegetable casein has the same properties; when it is dried and heated to redness, white alkaline ashes are obtained, which contain a great deal of potash, part of it united to phosphoric acid. The salts contained in vegetable casein, which are insoluble in water, are phosphates of magnesia, lime and iron, as in the milk of animals. It is impossible to obtain this casein soluble in water by itself, by adding carbonate of lime or barytes to the sulphate, as it appears to enter into insoluble combinations with these two earths much more readily than animal casein.

The soluble animal casein, obtained according to the method described by Braconnot, is never free from some foreign matter; it possesses also in an equal degree the power of forming combinations: its solutions cannot be warmed with carbonate of lime or barytes without being decomposed, forming perfectly insoluble compounds, which become hard as a stone in the air.

The animal casein prepared by Berzelius contained 6·5 per cent of foreign substances, such as phosphate of lime, magnesia, iron, and free lime, so that, strictly speaking, we are as little acquainted with a pure soluble animal as we are with a pure vegetable casein, free from bases or acids. In a word, it is impossible to find the slightest difference between the two bodies, either in composition or in their behaviour with reagents.

It is very remarkable that the identity of the two substances has hitherto escaped the attention of chemists, as Braconnot says in his treatise on the casein in the milk of animals (*Ann. de Chimie et de Physique*, xliii. p. 347): "I must confess, that in examining the seeds of the leguminous plants, before I was acquainted with the properties of casein, I fell into the error of describing legumin as a new and peculiar substance; at present it appears to me very much to resemble caseum."

Vegetable fibrin is an ingredient of the cereals, especially of wheat. It is found in combination with gluten when the dough of wheaten flour is kneaded, water being allowed to drop continually upon it. Vegetable albumen and starch are carried away by the water, and when this has taken place, or when the water ceases to be milky, a substance remains of a grayish white colour, tough, ductile, and perfectly insoluble in hot or cold water. The only difference between vegetable fibrin and albumen is this solubility in water. In this and in every other property it resembles animal fibrin obtained from arterial blood.

Vegetable albumen is obtained when the viscid part of wheaten flour is repeatedly washed with alcohol, till nothing more can be extracted; in this process the gluten is dissolved. When washed with alcohol, the first matter loses its viscid nature entirely. It is grayish white, soft and elastic, but not ductile as before, and is not free from starch and husks. When flour is mixed with water, dilute sulphuric acid added, and the whole kept warm until it is as liquid as water, the vegetable fibrin remains suspended in the liquid, in the form of a gray flocculent substance, which must be collected on a filter and washed with a weak solution of caustic potash. When carefully neutralized, a precipitate of vegetable fibrin and gluten is obtained, which alcohol will separate. Vegetable albumen is also contained in solution in the juices of plants, and may be extracted by cold water from corn and oily seeds. It is distinguished from vegetable casein by coagulating when heated, and by not being precipitated by acetic acid. When the solution of albumen is very dilute, the coagulum does not fall until the solution is evaporated.

The oily seeds contain vegetable casein and albumen in different proportions. When the concentrated milk of the seeds is mixed with æther free from alcohol and allowed to stand, two layers may be observed; the upper contains the oil, the under whatever is soluble in water. Boiling precipitates coagulated albumen from this aqueous solution; casein remains in the hot liquid, and may be removed by acetic acid. The albumen of sweet almonds is remarkable for its solubility, and for its property of causing the decomposition of amygdalin. When these almonds are well freed from oil by expression, treated with æther, and washed with cold water, acetic acid throws down casein from the aqueous solution, leaving albumen. It was this property which induced Robiquet to give a peculiar name to the albumen from that source.

Coagulated vegetable albumen is obtained from sweet almonds, when they are peeled, grated, and boiled a few minutes in water, which removes the sugar, gum, and the chief part of the casein, then washed with æther to remove the oil. In all its properties, in all its combinations, and in its behaviour towards acids and alkalies, it resembles the coagulated white of the egg. When burnt, these almonds leave 3·17 per cent of ashes, containing a great deal of carbonate of potash, besides phosphates of magnesia and lime, with traces of iron and alkaline phosphates.

These same salts are found in milk, and it is scarcely to be doubted that the potash in both is in combination with the casein and albumen. It is generally believed that the alkali of milk is in combination with lactic acid, but that acid has never been found in it when new. It is known that the acid in question begins to form as soon as milk leaves the udder, that it increases until the alkaline combinations are destroyed, and the consequence is coagulation, and the formation of lactate of casein, or cheese. Wheaten flour contains a considerable quantity of vegetable albumen, which may be extracted by cold water, and coagulated by boiling.

The juice of plants of every kind, when boiled, gives more or less coagulated albumen, of a gray or green colour; in most cases it is mixed with the green colouring matter of the leaves, and a colourless crystallizable fat or wax. There is a very great quantity of albumen in the juice of carrots, turnips, stalks of peas, cabbages, and garden vegetables in general.

Gluten is the name given to that part of the viscous matter of wheaten flour, which is soluble in alcohol. The alcohol is evaporated off, and the gluten remaining washed with hot water; it is a soft, yellowish mass, very viscous, and always somewhat acid in its action.

Gluten is a combination of casein; it is distinguished from vegetable fibrin by its solubility in boiling alcohol, and the ease with which it is dissolved at a common temperature in dilute ammonia. A few drops of acetic acid added to the saturated, boiling, ammoniacal solution, cause a white coagulum to be formed before the neutralization is effected; it cannot be distinguished from boiled caseum or coagulated white of egg. This precipitate contains ammonia in chemical combination; it may be removed by boiling in water acidulated with acetic acid, or by being well washed with water and dried.

When the viscous matter of wheat is rubbed with ammonia, vegetable fibrin remains, and a turbid solution of gluten is formed, which gives the same coagulum when acetic acid is added to it boiling: analysis proves it to have the same composition as albumen.

I will here give a comparative view of the composition of the azotized principles of Vegetable nutriments, and of Animal fibrin, albumen and casein.

Vegetable Fibrin.

	I. Dr. H. B. Jones.	II. Dr. Scherer.	III. Dr. Scherer.
Carbon	53·83	54·603	54·603
Nitrogen	15·59	15·810	15·810
Hydrogen	7·02	7·302	7·491
Oxygen, Sulphur, Phosphorus	23·56	22·285	22·096

Vegetable Albumen.

	From Rye. Dr. Jones.	From Wheat.	From Gluten. Drs. Will & Varrentrapp.
Carbon	54·74	55·01	54·85
Nitrogen	15·85	15·92	15·88
Hydrogen	7·77	7·23	6·98
Oxygen, Sulphur, Phosphorus	21·64	21·84	22·39

Vegetable Casein.

	Dr. Scherer.	Dr. Varrentrapp.
Carbon	54·138	51·41 } Sulphate (?)
Nitrogen	15·672	14·48 } C : N = 8 : 1
Hydrogen	7·156	
Oxygen, Sulphur	23·034	

Gluten.

	Dr. Jones.		
	Impure.	Purified with Æther.	Purer.
Carbon	58·47	56·80	55·22
Nitrogen	...	...	15·98
Hydrogen	7·65	7·60	7·42
Oxygen	...	...	21·38

To these analyses, which were made in this laboratory, I will add a few remarks.

From the results given, it is obvious that all these substances contain carbon and nitrogen in the same proportion.

Vegetable fibrin, albumen and casein, contain the same organic elements, and the composition is the same in all, viz. 8 equivalents of carbon to 1 of nitrogen. This proportion agrees exactly with that obtained by Mulder, as the composition of vegetable albumen. Marcet's analysis of the unwashed viscous matter of wheat gives a greater proportion of carbon; the analyses of vegetable albumen and gluten, by Boussingault, give a smaller. I will here give these analyses:—

Gluten of Wheat, not purified.

	Marcet.	Boussingault.
Carbon	55·7	53·5
Nitrogen	14·5	15·0
Hydrogen	7·8	7·0
Oxygen	22·0	24·5

Boussingault obtained impure gluten from the glutinous matter of wheat, by boiling it in alcohol and precipitating the solution by water. This was dissolved in acetic acid, and precipitated by carbonate of ammonia, for a second analysis. After abstracting the incombustible ingredients, the results are:—

	Washed out with Alcohol.	Dissolved in Acetic Acid.
Carbon	54·2	52·3
Nitrogen	13·9	18·9
Hydrogen	7·5	6·5
Oxygen	24·4	22·3

Vegetable albumen, that part of wheaten flour which is soluble in water, and is precipitated in a coagulum on being boiled and evaporated, was found to have the following composition, according to the same chemist:—

Carbon	52·7
Nitrogen	18·4
Hydrogen	6·9
Oxygen	22·0

The solution of gluten in acetic acid is turbid and mucous; ammonia and its carbonate precipitate that solution white, long before the acetic acid is neutralized; the matter is not glutinous in this state, but may be drawn out into threads. Mixed with it there is a substance resembling birdlime, which may be removed by æther. Boussingault neglected to use æther in purifying these substances; but this cannot be the cause of the difference in the carbon. The numerous analyses which have been made here, prove that the difference lies partly in the difficulty of obtaining these substances in a fine powder after they are dried: they are all tough and horny, and it is impossible to burn them completely with oxide of copper. It can only be done with the assistance of chlorate of potash, or by using chromate of lead. As to the greater proportion of nitrogen obtained by Boussingault, I must refer to what I have said at the beginning, that although the analyses with oxide of copper, according to the qualitative method, in the experiments of Drs. Will, Varrentrapp, Scherer, and Jones, did not give 18 per cent of nitrogen; they gave so much as 17· and 17·5, which is equal to 6·9, 7·0, 7·2, 7·3 of carbon to 1 of nitrogen. When chromate of lead was used, the proportion of 1 to 8 was found in the last tube of gas collected.

According to the quantitative method of analysis first described by myself, and followed by Mulder, the proportion was 8 equivalents of carbon to 1 of nitrogen. The same result was obtained, according to the new method of estimation, from the ammonia produced, which gives very accurate results. (*Annalen* of Sept.*) Gluten I consider to be a very variable ingredient of the flour of cereals, as rye, barley, buck-wheat, and also of the flour of lentils, peas, beans, and maize, which, when washed with alcohol, give out fatty and resinous substances, but very little gluten; it contains an organic acid, which I have not succeeded in obtaining pure. It is well known what a small quantity of acetic or of lactic acid is necessary to combine with albumen, or to coagulate casein, and that no method is known of separating

* See New Method of determining Nitrogen in Organic Compounds, by Drs. Will and Varrentrapp, in the Phil. Mag. for March 1842, p. 216.

the acid again in a state of purity. De Saussure has mentioned another body, which he found in small quantities in impure gluten; he calls it mucin. It contains nitrogen; its composition cannot be very different from vegetable fibrin, as the unpurified gluten of wheat is very little different in composition from vegetable fibrin and albumen.

The analyses of the constituents of Animals show, in a very distinct manner, how entirely the azotized principles of Vegetables agree in composition with them.

The following are Mulder's analyses after abstracting the incombustible parts:—

	Fibrin.	Albumen.		Casein.
		From Eggs.	From Serum.	
Carbon . . .	54·56	54·48	54·84	54·96
Nitrogen . .	15·72	15·70	15·83	15·80
Hydrogen . .	6·90	7·01	7·09	7·15
Oxygen . . . Phosphorus . Sulphur . . .	22·82	22·81	22·24	22·09

These results agree perfectly with those of Dr. Scherer, who obtained—

	Fibrin.	Albumen.		In the whey of sour coagulated Milk.
		From Eggs.	From Serum.	
Carbon . .	54·454	55·000	55·097	54·507
Nitrogen .	15·762	15·920	15·948	15·670
Hydrogen	7·069	7·073	6·880	6·900
Oxygen . . Phosphorus Sulphur . .	22·715	22·007	22·075	22·923

Dr. Scherer's analyses, according to the qualitative method, gave the proportion of 1 of nitrogen to 6·9, 7·1, 7·2, 7·3 of carbon. When chromate of lead was used, the result in the last tube of gas was 1 : 8. This last result was obtained also by Drs. Will and Varrentrapp's new method. When milk is allowed to stand till it becomes sour, what remains in the whey, and the analysis of which is here given, is clearly albumen, and is precipitated by boiling: it is certainly albumen, as caseum is more soluble in hot than in cold liquids.

A single glance at the results of these analyses, shows that graminivorous animals receive, in the vegetables which they eat, the ingredients of their blood, namely, their albumen and fibrin, although not in the same state; that the juices of plants contain albumen; that wheaten flour, and corn in general, contain the ingredients of the muscular fibres; that beans, peas, and lentils contain the same substances that are found in the milk of animals. They are nourished by the flesh, blood, and cheese produced by the plants, whilst their own flesh and blood serve as nourishment to carnivorous animals. The resemblance of the azotized principles of vegetables and the ingredients of the blood is not confined to the chemical composition; it is not merely a similarity in the numbers of atoms, but the behaviour with reagents is the same in vegetable and animal albumen and casein.

Vegetable albumen obtained from the juices of plants by boiling, and washed from fatty and colouring matter by alcohol and æther, cannot possibly be distinguished from animal albumen, precipitated from its aqueous solution by boiling; the external appearance of the former is the same as that of the latter, and also its behaviour with alkalies, acids, the infusion of gall-nuts, corrosive sublimate, creosote, &c. The same may be said of vegetable casein: this substance appears to occur very frequently in vegetable substances, and is found in considerable quantities in all oily seeds. An emulsion of these seeds is very like the milk of animals, but contains a much larger proportion of albumen. Vegetable milk contains a fat corresponding to butter, also sugar, casein, and albumen; these latter two are evidently in union with alkalies; when heated, the albumen coagulates, and rises with the oil to the surface of the liquid; when separated from the coagulum it becomes sour in twenty-four hours, and a pure precipitate of caseum is obtained, leaving lactic acid in the solution. A solution of pure crystallized cane sugar left with vegetable casein for several days in a gentle warmth, was converted entirely into acetic acid, lactic acid, and a body resembling gum arabic, just as when left with common animal cheese.

A considerable quantity of sulphuret of potassium is obtained from the casein of sweet almonds and leguminous plants, warmed for a length of time in caustic potash. Acids precipitate protein from this solution, and cause the escape of sulphuretted hydrogen.

The body named vegetable fibrin by me, is the same as that called by Berzelius vegetable albumen of the cereals; but if, as is proved by the analyses, these names refer only to different modifications of the same body, the name of albumen cannot be given to that ingredient of the seeds of the cereals which is entirely insoluble in water, because different names must be given to bodies in different conditions, and the idea of solubility in certain liquids, and coagulation by heat, is inseparably connected with albumen. This substance approaches the fibrin of the blood in all its properties, and all its relations to other bodies: the ashes contain no soluble alkali, whereas, all liquids which contain albumen, such as the serum of the blood, leave a great deal of alkaline carbonates when dried and burnt. The presence of an alkali may be the cause of its solubility in one case, and the absence of an alkali the cause of its insolubility in another; but the albumen of the serum, and the fibrin of the blood, owe their different conditions to the same cause. For this reason, and to avoid the extraordinary confusion observable in the usual descriptions of these bodies, which so greatly resemble each other, I have adopted the name of vegetable fibrin, to distinguish this insoluble modification, although it may not appear altogether appropriate.

Vegetable albumen, fibrin, and casein, dissolve in warm concentrated hydrochloric acid, with the same lilac or violet colour, as the corresponding animal substances; when heated alone, they give the same sulphurous products, and the same horny ammoniacal smell.

When left moist they putrefy; the products of the putrefaction of gluten and vegetable fibrin are in some measure known, and differ from those of caseum by evolving gas at the beginning, like flesh. Caseum does not do so, but the same solid products are found in both cases; they have the taste and smell of caseum, freed from butter, and as much aposepedin, or, as Gmelin terms it, oxide of caseum, may be obtained from it, in fine, bright scales, like mother of pearl, as from caseum.

Vegetable casein possesses in a high degree the power of fermenting sugar, if it is allowed to stand until putrefaction has commenced. If allowed to stand till putrefaction has made some progress, it is impossible to distinguish it from common caseum; and vegetable albumen gives out sulphuretted hydrogen exactly as the rotten egg. It is not very improbable that casein is contained, in a state of solution, in the juice of grapes and those plants which precipitate very little albumen on being heated and evaporated. It is known to be very soluble in tartaric acid, and the presence of this acid may be the reason that the sugar is decomposed into carbonic acid and alcohol, and not into lactic acid and mucous matter, as is the case when common caseum or fresh lactate of caseum is used.

It is well known that fermentations may be produced in saccharine solutions by more than one substance. Vegetable casein cannot be considered as the basis of yeast; but the circumstance of the juice of grapes not coagulating when boiled, and entering again into fermentation when allowed to stand, seems to prove that it does not contain vegetable albumen, as this body is well known to be entirely changed by boiling, even when the solution is so dilute that the coagulum cannot precipitate itself.

Another inquiry must, of course, be made into the properties of all these bodies, and I wish it distinctly to be understood that such is not the intention of the present paper; my desire is to call the attention of physiologists and physicians to the fact that the composition of azotized vegetable nutriments is the same as the constituents of animal bodies, and when this is proved, no doubt can be entertained as to the similarity of the process of nutrition, in graminivorous and carnivorous animals.

A carnivorous animal may be said to feed on what is in no way different from itself; it adds a piece of muscle, as it were, to its muscle: a graminivorous animal may be said also to do the same, because the food it consumes has the same composition with its own flesh and blood.

The flesh and the blood, the food of carnivorous animals, assumes precisely the same form in their organization as the vegetable casein, albumen, and fibrin in graminivorous animals.

In this sense, then, we may assert that vegetables generate the blood of animals, although physiologists cannot make use of this expression, however correct, chemically speaking, on account of the different states in which these ingredients are found in the vegetable and animal kingdom.

It is really a very remarkable circumstance, that the inorganic ingredients also are the same in both; magnesia, phosphoric acid, lime, iron, alkalies, and sulphur, are constantly found in them; both leave, when burnt, similar ashes.

Animals are distinguished from plants by their capability of moving from place to place, by their sensations, and sensibility, or, in one word, by their senses; for all these purposes certain organs are required, which are entirely wanting in plants; still, the same active principle gives to the bud, the leaves, and the fibres of the root, the same wonderful properties; the plant is alive, as truly as any part of the body of the living animal; they both receive, on the same principle, the properties of growth, reproduction, and the power of replacing again in the system what has been consumed. Of these properties true vegetable life consists; it is developed without consciousness.

Chemically speaking, animal life, although of a rank infinitely higher, generates only the substance of the nerves and of the brain, which are altogether wanting in plants. Although animals receive from vegetables all the ingredients requisite for the formation of blood, and cannot by their own organization generate them from carbonic acid and ammonia, as plants do, the power belongs to them alone of producing those bodies of a higher order, such as in the complex constituents of the brain, the spinal marrow and the nerves. Animals must have peculiar organs for the exercise of the will, the feelings, and locomotion; and these organs must be produced from that part to which the impulse is given. Physiology gives us no decided information on these points; the spleen and the numerous glands must all have some part to perform in the body, and a necessary one too, or they certainly would not exist.

The growth of plants depends on the continual supply of carbon, and two other elements; and this supply is obtained by the separation of oxygen from the ingredients of their food.

The growth of the organs of a graminivorous animal must depend also on a similar separation of oxygen; but we know that the life of animals, on the contrary, is characterized by a constant absorption of oxygen, although it does not remain in the body; and it is known, from a number of simple facts, that besides the oxygen of the atmosphere, which escapes in combination with carbon, another portion arising from the food must escape also, under certain circumstances, as carbonic acid.

This last oxygen arises from that nutriment which contains no nitrogen, when fat is formed; starch, sugar, and gum, cannot be used by animals for the formation of blood, or muscular fibre, because the azotized nourishment they receive contains all that is wanted. The membranes, the cellular tissue, skin, horn, and the claws of animals, contain more nitrogen, in proportion to their carbon, than albumen and fibrin. These latter must give up a certain portion of their carbon if the former are produced from the blood: that they are produced from substances with no nitrogen, is impossible.

Now we find that the flesh of graminivorous, and especially of domestic animals, which eat a great deal of food without nitrogen, is very fat; and that this fat may be increased, by increasing the supply of this kind of food. The flesh of carnivorous animals is without fat, and sinewy; all the food which they eat contains nitrogen, except the fat of the animals they devour.

It is evident that starch, sugar, and gum, are incapable of supplying that loss which is continually occasioned in animals by the vital powers; they are incapable of forming muscular fibre, cerebral matter, the membranes, or the bones and sinews, because their only ingredients are carbon, and the elements of water; they contain neither nitrogen, phosphorus, lime, sulphur, nor iron. Children fed on such food become very fat; but neither their muscles nor their bones can increase, and they themselves therefore cannot become stronger. Physicians are well acquainted with the fact, that children who are not supplied with a sufficient quantity of lime in their food, eat that which they collect from the walls of houses, with the same appetite that they have for their meals.

When we compare the chemical composition of such bodies as sugar, gum, and amylin, with that of fat, we find that they contain the same quantity of carbon and hydrogen, and that the only difference is in the quantity of oxygen, which is smaller in the fat bodies.

According to the analyses of Chevreul, which are the most accurate and most to be trusted, the following is the composition of the fat of swine, of sheep, and of man:—

	Swine.	Sheep.	Man.
Carbon . .	79·098	78·996	79·000
Hydrogen .	11·146	11·700	11·416
Oxygen . .	9·765	9·304	9·584

	Amylin.	Sugar of grapes and milk.	Gum.
Carbon . .	44·91	40·45	42·58
Hydrogen .	6·11	6·61	6·37
Oxygen . .	48·98	52·64	51·05

In amylin the proportion of carbon to hydrogen is the same as in the fat of swine, namely 44·91 : 6·11, or as 79 to 11.

Sugar of grapes, sugar of milk, cane sugar and gum, are distinguished from amylin by containing a certain quantity of carbon and hydrogen, in the same proportion as water, over and above what amylin contains; so that when the composition of amylin is expressed by $C_{12}\ H_{10}\ O_{10}$, that of sugar of milk and dry grape sugar will be $C_{12}\ H_{10}\ O_{10}$ plus 2 at. aq.; that of cane sugar $C_{12}\ H_{10}\ O_{10}$ plus 1 at. aq.

By merely giving up part of their oxygen, such bodies may become fat, the only substance which contains no nitrogen in the animal organization.

The question then may be asked, is a certain portion of food without nitrogen absolutely necessary to the existence of the life of some animals, merely for the sake of forming fat? Wild graminivorous animals have no fat, but more muscle than carnivorous; they become fat before the breeding season or before hibernation, when they take little or no nourishment. This fat must have some use.

Man and every other animal are exposed at every period of their lives to the unceasing and destructive action of the atmosphere; with every breath he breathes out a part of his body; every moment of his life he produces carbonic acid, the carbon of which his food must replace.

If we observe a man or other animal in sickness, or at any time when the body is not supplied with nourishment to compensate for the continual loss, we find him to become lean; the fat is the first to disappear; it vanishes through the skin and lungs, in the form of carbonic acid and water, as none of it can be found in the fæces or urine; it resists the action of the atmosphere on the body, and is a protection to the organs. But the action of the atmosphere does not end with the loss of fat; every soluble substance in the body gives up its carbon, until at last all resistance ceases, and death and decay begin, when every part of the body enters into combination with the oxygen of the air. The influence of the atmosphere is the cause of death, in most chronic diseases; from want of carbon to resist its action, that of the nerves and brain is used. In a normal state of health and nutrition, the carbon of the carbonic acid must have another source. In a second paper, I shall endeavour to show, that the carbon of such substances as sugar, gum, and starch, is used for the purposes of respiration and the production of animal heat; and that the latter is closely connected with the carbon of the food.

Article XI.

√ *Memoir on the Theory of Light.* By M. A. L. Cauchy.

[Read at the Royal Academy of Sciences, May 31 and June 7, 1830*.]

Part the First.

I FIRST† gave, in the *Exercices de Mathématiques* (third and fourth volumes), the general equations of equilibrium or of motion of a system of molecules acted upon by mutual forces of attraction or repulsion, admitting that these forces were represented by functions of the distances between the molecules; and I proved that these equations (which include a great number of coefficients depending on the nature of the system) were reducible in the case where the elasticity became the same in every direction to other formulæ which include but one coefficient, and which had been originally obtained by M. Navier. I also deduced from these equations those which determine the motions of elastic plates and rods when the elasticity is supposed not to be the same in every direction; and I thus obtained formulæ which comprehend, as particular cases, those which M. Poisson and some other geometricians had found on the opposite supposition. The remarkable agreement of these different formulæ, and of the laws which are deducible from them, with the observations of physicists, and especially the beautiful experiments of M. Savart, encouraged me to adopt the advice of some persons who urged me to make a new application of the general equations which I had given to the theory of light. Having followed this advice, I have been fortunate enough to arrive at results which I am about to detail in this Memoir, and which seem to me worthy of occupying for a moment the attention of physicists and geometricians.

* From *Mémoires de l'Académie Royale des Sciences de l'Institut de France*, t. x.

† The investigation which M. Cauchy here refers to, as establishing the principles which he assumes in this paper, are nearly the same as those of which an abstract was given by Prof. Powell in the L. and E. Phil. Mag., vol. vi. The reader may also refer to the same writer's volume on the Undulatory theory as applied to dispersion, &c. London, 1841, J. W. Parker;—especially the Introduction, p. xlii.; and a Note in L. E. and D. Phil. Mag. for November, 1841.

The three partial differential equations which represent the motion of a system of molecules, acted upon by mutually attracting and repelling forces, include, with the time t, and the rectangular coordinates x, y, z of any point of space, the displacements ξ, η, ζ of the molecule $\mathfrak{m}$, which coincides at the end of the time t, with the point in question; these displacements being measured parallel to the axes of the x, y, z. The same equations will offer one-and-twenty coefficients depending on the nature of the system, if we make abstraction of the coefficients which disappear when the masses m, m', m'' of the different molecules are two by two equal among themselves, and symmetrically distributed about the molecule $\mathfrak{m}$ upon right lines drawn through the point with which this molecule coincides. In short, these equations will be of the second order, that is to say, they will only contain derivatives of the second order from the principal variables ξ, η, ζ; and, by considering each coefficient as a constant quantity, we may reduce their integration to that of an equation of the sixth order, which will include only a single principal variable. Now, this latter may easily be integrated by the aid of the general methods which I have given in the 19th number of the Journal of the *Ecole Polytechnique* and in the memoir on the application of the residual calculus to questions of mathematical physics. By applying these methods to the case where the elasticity of the system remains the same in every direction, and reducing the value of the principal variable to the simplest form, by means of a theorem established long ago by M. Poisson, we obtain precisely the integrals which that geometrician has given in the Memoirs of the Academy. But in the general case the principal variable being represented by a definite sextuple integral, it was necessary, in order to discover the laws of the phænomena, to reduce this sextuple integral to an integral of a lower order. This reduction stopped me a long while; but I at last succeeded in effecting it, for the partial differential equation above mentioned, and indeed generally for all partial differential equations in which the different derivatives from the principal variable, compared with the independent variables x, y, z, t, are derivatives of the same order. I then obtained, to represent the principal variable, a definite quadruple integral, and I was enabled to investigate the laws of the phænomena, the knowledge of which should result from the integration of the proposed equations. This inquiry was the object of the last memoir which I had the honour of presenting to the Academy, and which contains, amongst others, the following proposition.

A partial differential equation being given, in which all the derivatives of the principal variable relative to the independent variables x, y, z, t, are of the same order, if the initial values of the principal variable and of its derivatives taken with relation to the time, are sensibly evanescent in all the points situated at a finite distance from the origin of the coordinates, this variable and its derivatives will no longer have sensible values at the end of the time t, in the interior of a certain surface, and consequently the vibrations, sonorous, luminous, or other, which may be determined by means of the partial differential equation, will be propagated in space, so as to produce a wave, sonorous, luminous, &c., whose surface will be precisely that which we have just indicated. Moreover, the equation of the surface of the wave will easily be obtained, by following the rule which I proceed to describe.

Let us suppose that in the partial differential equation we substitute for any derivative whatever of the principal variable, taken in relation to the independent variables x, y, z, t, the product of these variables raised to powers, the degrees of which are indicated for every independent variable by the number of differentiations relative to it. The new equation obtained will be of the form

$$F(x, y, z, t) = 0,$$

and will represent a certain curved surface. Now, consider the radius vector drawn from the origin to any point of this curved surface; take upon this radius vector, setting out from the origin, a length equal to the square of the time divided by this same radius; then draw through the extremity of this length a plane perpendicular to its direction. This plane will be the tangent plane to the surface of the wave, and consequently this surface will be the envelope of the space which the different planes will traverse that may be constructed by the operation just described. Finally, we come to the same conclusions by following another method, which I will now state in a few words, and which I explained in my last lectures at the College of France.

Let us suppose that the initial values of the principal variable and of its derivatives taken with relation to the time, are only sensible for points situated at very small distances from a certain plane drawn through the origin of the coordinates, and depend solely upon these distances. This same variable, and these derivatives, will not be sensible at the end of the time *t*, except in the vicinity of one of the parallel planes, constructed by means of the rule which we have before indicated. Consequently, if the sonorous, luminous, &c. vibrations are originally included in a plane wave, this wave, which we will call elementary, will divide into several others, each of which will be propagated in space, remaining parallel to itself, with a constant velocity. But these different waves will have different velocities of propagation. If now we suppose that at the first instant several elementary waves are included in different planes drawn through the origin of the coordinates, but little inclined upon each other, and that the sonorous, luminous, &c. vibrations are small enough to remain insensible in each elementary wave taken separately; then these vibrations not being capable of becoming sensible but by the superposition of a great number of elementary waves, it is clear that the phænomena relating to the propagation of sound, of light, &c., can be observed at the first moment only within a very small space around the origin of the coordinates, and at the end of the time *t*, only in the vicinity of the different sheets of the surface which will be touched by all the elementary waves. Now, this last surface will be precisely the curved surface of which we have spoken above, and which we generally name "the wave-surface."

This being established, if we consider the motion of propagation of the plane waves, in a system of molecules acted upon by mutually attracting and repelling forces, we may take successively for principal variables three rectangular displacements of a molecule $\mathfrak{m}$, measured parallel to the three axes of a certain ellipsoid, which will have for its centre the origin of the coordinates, and will be easy to construct as soon as we know the coefficients depending on the nature of the proposed system, and the direction of the plane A B C, which included a plane wave at the first instant. Then this wave will divide into six others, which will constantly be of the same breadth as the first, and will be propagated with constant velocities in planes parallel to A B C. These waves, taken two by two, will have equal velocities of propagation, but in contrary directions. Moreover, these velocities, measured in the direction of a right line perpendicular to the plane A B C, for the three waves which move in the same direction, will be constant, and respectively equal to the quotients which we obtain by dividing unity by the three semi-axes of the ellipsoid above mentioned. The points situated without these waves will be in repose; and if the three semi-axes of the ellipsoid are unequal, the absolute displacement, and the absolute velocity of the molecules, in a plane wave, will always remain parallel to that of the three axes of the ellipsoid, which will be reciprocally proportional to the velocity of propagation of this wave. But if two or three of the axes of the ellipsoid become equal, the plane waves, which will propagate themselves in the same direction with velocities reciprocally proportional to these axes, will coincide, and the absolute velocity of each molecule included in a plane wave will be, at the end of any given time, parallel to the right lines along which the initial velocities were projected on the plane drawn through the two equal axes of the ellipsoid, or even, if the ellipsoid is changed into a sphere, in the direction of these initial velocities.

Now, let us suppose that at the first instant several plane waves, slightly inclined one upon another, and on a certain plane A B C, meet, and are superposed at a certain point A. As the time increases, each of these waves will propagate itself in space, giving rise, on each side of the plane which originally included it, to three similar waves included in parallel planes, but possessed of different velocities of propagation; consequently, the system of plane waves, which we at first considered, will be subdivided into three other systems, and the *point of meeting** [*point de rencontre*] of the waves, which will make a part of one and the same system, will be displaced in the direction of a certain right line, with a velocity of propagation distinct from that of the plane waves. Thus, at the end of any given time t, the point A will be succeeded by three other points, whose positions in space may be calculated for a given direction of the plane A B C, and the different positions which the three points in question shall take for different directions originally attributed to the plane A B C, will determine a curved surface of three sheets, in which each sheet will be constantly touched by the plane waves, which will make part of the same system. Now, this curved surface will be precisely that of which we have already spoken above, and which we have named "the wave-surface."

Finally, in order that the propagation of the plane waves may be effected in an elastic body, it is necessary that the coefficients, or at least certain functions of the coefficients included in the partial differential equations which represent the motion of the elastic body, should remain positive. In the opposite case, the plane waves will no longer be able to propagate themselves, and we should be apprised of it by the calculus, which would give for the velocities of propagation imaginary values.

In the theory of light, under the name of æther, is designated the imponderable fluid, which is considered as the elastic medium in which the luminous waves are propagated. The *point of meeting* [*point de rencontre*] of a great number of plane waves, the planes of which are slightly inclined to each other, is that in which we suppose that light may be perceived by the eye. The series of positions which this point of meeting takes in space whilst the waves are displaced, constitutes what we call a luminous ray; and the velocity of the light, measured in the direction of this ray, should be carefully distinguished; 1st, from the velocity of propagation of the plane waves; 2nd, from the proper velocity of the æthereal molecules. Lastly, we call polarized rays those which correspond to plane waves, in which the vibrations of the molecules remain constantly parallel to a given right line, whatever may be the directions of the initial vibrations.

For greater generality, we shall say that in a luminous ray the light is polarized parallel to a right line or a given plane, when the vibrations of the luminous molecules shall be parallel to this right line or to this plane, without being parallel in every case to the directions of the initial vibrations; and we will call the plane of polarization the plane which will include the direction of the luminous ray, and that of the proper velocities of æthereal molecules. These definitions, as we shall see hereafter, agree with the received denominations.

This granted, it results from the principles above established, that in setting out from a given point in space, a ray of light, in which the proper velocities of the molecules have any given directions whatever, will generally be subdivided into three rays of light, polarized parallel to the three axes of a certain ellipsoid. But each of these polarized rays can be no further divided by the action of the elastic fluid, in which the light is propagated. Moreover, the mode of polarization will depend upon the constitution of this fluid, that is to say, upon the distribution of its molecules in space, or in a transparent body, and upon the plane which originally included the vibrating molecules. If the constitution of the elastic fluid is such that the velocities of propagation of plane waves become imaginary, this propagation can no longer be effected, and the body in which the æthereal fluid is comprised, will become what we term an opake body. If the body remain transparent, and if in this body the æthereal fluid is distributed in such a manner that its elasticity continues the same in every direction around any given point, the three polarized rays, into which a ray of light generally subdivides itself, will be directed along the same right line; and, as the velocity of the light will be the same in the two first rays, these will be confounded with each other; there will then remain but two polarized rays, one double, the other simple, having the same direction. Now, calculation shows that in the simple ray the light will be polarized in the direction spoken of; whilst, in the double ray, the light will be polarized perpendicular to this direction. If the initial vibrations of the luminous molecules are contained in a plane perpendicular to the direction in question, the simple ray will disappear, and the proper velocities of the molecules in the double ray will remain constantly directed along right lines parallel to the directions of the initial velocities; so that, to speak properly, there will be no more polarization. Then also the velocity of propagation of the light will be equivalent to the velocity of propagation of a plane wave, and the same in every direction around each point. Now, the reduction of all the rays to a single one, and the absence of all polarization in the media in which the light is propagated in every direction with the same velocity, being facts proved by experiment, we must conclude from what precedes, that in these media the proper velocities of the æthereal molecules are perpendicular to the directions of the luminous rays, and comprised in the plane waves. Thus the hypothesis, admitted by Fresnel, becomes a reality. This able physicist, of whom science has been unfortunately deprived by a premature death, was then right in saying, that in ordinary light the vibrations are transversal, that is to say, perpendicular to the directions of the rays. The ideas of Fresnel upon this subject have, in truth, been strongly combated by an illustrious academician, in several articles contained in the *Annales de Physique et de Chimie*, and one of which relates to the motions of two superposed fluids. According to the author of these articles, the vibrations of the molecules in æther will end by being always sensibly perpendicular to the surfaces of the waves which the motion produces in propagating itself; and from that time the polarization, such as it has been previously defined, will become impossible, and will disappear completely. Thus, also, the surface of the waves will always be an ellipsoid, and will present only a single sheet, so that, in order to explain the double refraction, we should be obliged to suppose two æthereal fluids simultaneously inclosed in the same medium. But it must be remarked, that the author, as he himself says, had deduced these different consequences from the integration of the known partial differential equations, which represent the motions of elastic fluids, and of that which is thence deduced, when we suppose the three coefficients of the partial derivatives of the principal variable unequal. Now, these equations do not appear applicable to the propagation of luminous waves in an æthereal fluid; and the remarkable agreement of the theory which I propose with experiment, seems to me sufficient to confirm the assertion which I have already published in a former Memoir on the motion of light,—to wit, that the differential equations of this motion are comprised in those contained in the 31st and 32nd Numbers of the *Exercices de Mathématiques*.

In the second part of this memoir, which I intend to read at the next meeting, I shall apply the principles which I have just established to the determination of the laws according to which light is propagated in crystals having but one, or two, optical axes; and I shall show how rules proper to make known the velocities of propagation of the elementary waves may be deduced from my formulæ and the planes of polarization of the luminous rays. When we stop at a first degree of approximation, these rules agree in a manner worthy of remark with those which several physicists have deduced from experiment, or from the hypothesis of undulations, and in particular with those which Fresnel has given in his excellent memoir on double refraction. Only he was mistaken in admitting that the vibrations of the æthereal molecules in a luminous ray were sensibly *perpendicular* to the plane generally called the plane of polarization. In reality, the plane of polarization *contains* the direction of the ray and that of the vibrations of the æther. A young geometrician, M. Blanchet, had, independently, and indeed before me, deduced this consequence, and the laws of polarization for the crystals having but one optical axis from the first formulæ which I had given. But the new analysis which I have employed leaves nothing to desire in this respect, and extends to all possible cases.

I shall also show in the second part of the memoir, that the pressure is evanescent in the æthereal fluid which propagates the luminous vibrations; and I shall show the conditions which the coefficients, contained in the differential equations of the motion of elastic bodies, should satisfy, in order that the surface of the luminous wave should acquire the form indicated by experiment. Lastly, in a third part, I shall show how we may establish the laws of reflection and of refraction at the first or at the second surface of a transparent body, and determine the proportion of light reflected or refracted. Here again, theory perfectly agrees with observation, and analysis leads me to the laws which several physicists have deduced from experiment. Thus, in particular, calculation furnishes me with the law of Sir D. Brewster on the angle of complete polarization by reflection, and the law of M. Arago on the quantity of light reflected at the first and at the second surface of a transparent medium. I also obtain the formulæ which Fresnel has inserted in the Seventeenth Number of the *Annales de Physique et de Chimie,* and which would alone suffice to prove the truly wonderful sagacity of this illustrious philosopher.

Lastly, I shall investigate the means, by aid of which physicists may verify the reality of the triple refraction, or, what comes to the same thing, the existence of the third polarized ray, traversing a medium whose elasticity is not the same in every direction.

Part the Second.

[Presented to the Academy the 14th of June, 1830.]

As we have seen in the first part of this Memoir, the integration of the partial differential equations, which I have given in the 'Exercices,' to represent the motion of a system of molecules acted upon by forces of mutual attraction or repulsion, leads directly to the explanation of the different phænomena which the theory of light presents. But further, to establish this theory, it is not necessary to have recourse to the general integrals of the equations in question. It will suffice to discuss the particular integrals which express the motion of propagation of a plane wave in an elastic medium. In fact, the sensation of light being supposed to be produced by the vibrations of the molecules of an æthereal fluid, to determine the direction and the laws according to which such vibrations, at first circumscribed in very narrow limits, around a certain point O, would propagate themselves through this fluid, it is sufficient to consider at the first instant a great number of plane waves, which are superposed in the neighbourhood of the point O, and to admit that, the planes of these waves being slightly inclined one upon another, the vibrations of the molecules are small enough to remain insensible in each wave, taken separately, but become sensible by the superposition just mentioned. Now, calculation has shown us, that in an æthereal fluid, whose elasticity is not the same in every direction, each plane wave generally subdivides itself into three others of the same thickness, comprised in parallel planes, but propagated with different velocities on each side of the plane which contained the initial wave. From this we concluded, that a system of plane waves, superposed at first in the vicinity of a given point O, subdivides into three systems of waves, which are successively superposed in different points of space, and we have given the name of a "*luminous ray*" to the right line which contains, for one of the systems, all the points of superposition. We have also shown that three luminous rays generally result from molecular vibrations, which at first spread but to a very small distance around the point O. We have, moreover, found that in each of these luminous rays the vibrations of the æthereal molecules remained constantly parallel to one of the three axes of a certain ellipsoid, and that consequently in the three rays the light was polarized in three directions perpendicular to each other, and parallel to the three axes of the ellipsoid, whatever in other respects might be the directions of the initial vibrations. We have seen the three rays reduced to two, or even to one only, when the initial vibrations were parallel to one of the principal planes of the ellipsoid, or to one of its axes, and hence it has been easy to comprehend why the polarized rays do not become subdivided to infinity. We have proved, that in the case where the elasticity of the æther is the same in every direction, the three rays become reduced to two, to wit, a simple ray and a double ray, in the direction of the same right line, and polarized, the first parallel, the second perpendicularly, to this right line. In fine, we have seen the simple ray disappear when the initial vibrations of the molecules of the æther were supposed perpendicular to the directions of the rays, and then, properly speaking, there was no more polarization. Now, the reduction of all the rays to a single one, and the absence of all polarization in the media in which the light remains the same in every direction, being verified by experiment, we have drawn this definitive conclusion from our analysis, that, in ordinary light, the vibrations are transversal, that is to say, perpendicular to the directions of the rays; and thus the hypothesis which Fresnel proposed, notwithstanding the arguments and calculations of an illustrious adversary, has become a reality.

We shall now briefly apply the theory which we have just recapitulated to the propagation of light in the crystals having one, or two, optical axes. In order to effect this, it will not be necessary to employ the general equations which we gave in the Thirty-first Part of the 'Exercices,' to represent the motion of a system of molecules acted upon by mutual attracting or repelling forces, and these equations may be reduced to the formulæ (68) of page 208 of the Third Volume; that is to say, to the formulæ which express the motion of a system which offers three axes of elasticity perpendicular one to another. We may moreover suppose that no force arising within itself is applied to the system, and then the formulæ in question will only include the time t, the coordinates x, y, z of any molecule $\mathfrak{m}$, its displacements ξ, η, ζ, measured parallel to the coordinate axes, and nine coefficients G, H, I, L, M, N, P, Q, R, the three first of which are proportional to the pressures sustained in the natural state of the æthereal fluid, by three planes respectively perpendicular to these same axes. The coefficients here in question being considered as constant, we shall easily construct the ellipsoid whose three axes are reciprocally proportional to the three velocities of propagation of the plane waves parallel to a given plane, and drawn parallel to the right lines, according to which are measured the proper velocities of the æthereal molecules in these plane waves. We may also determine, 1st, the directions of the three polarized rays produced by the subdivision of a luminous ray in which the vibrations of the molecules would have any directions whatever; 2nd, the velocity of the light in each of these three rays; 3rd, the different values which this velocity would take in the polarized rays produced by the subdivision of several luminous rays, which should set out simultaneously from the same point. Finally, we might construct the surface of three sheets, which, at the end of the time t, would pass through the extremities of these rays, and which we call the wave-surface. As to the intensity of the light, it will be measured in each ray by the square of the velocity of the molecules. This being laid down, if the elasticity of the æthereal fluid remain the same in every direction around any axis parallel to the axis of z, we shall have

$$G = H,\ L = M = 3R,\ P = Q; \quad . \quad . \quad . \quad (1.)$$

and consequently the nine coefficients dependent on the distribution of the molecules in space will be reduced to five, viz. H, I, N, Q, R. Further, two sheets, of the surface above-mentioned, may be reduced to the system of two ellipsoids of revolution circumscribed one on the other; and for this last reduction to take place, it will suffice that the condition

$$(3R - Q)(N - Q) = 4Q^2 \quad . \quad . \quad . \quad . \quad (2.)$$

be fulfilled. In fine, one of the two ellipsoids will become a sphere, which will have for diameter the axis of revolution of the other ellipsoid, if we suppose

$$H = I; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

and then the course of the two polarized rays will be precisely that which the theorem of Huyghens indicates, relative to crystals which present a single optical axis. Now, the accuracy of this theorem having been put out of doubt by the numerous experiments of the most skilful physicists, it results from our analysis, that in crystals having one optical axis, the coefficients H, I, N, Q, R, verify the conditions (2.) and (3.). Besides, the elasticity of the æthereal fluid not being, by hypothesis, the same in all directions, but only around the axis of Z, it is not natural to admit that we should have $G = H = I$, unless we suppose the three coefficients G, H, I, generally evanescent. It is then very probable that in its natural state these three coefficients disappear in æther, and with them the pressures sustained by any plane. This hypothesis being admitted, the ellipsoid and the sphere above mentioned will be represented by the equations

$$\frac{x^2 + y^2}{R} + \frac{z^2}{Q} = t^2, \quad \frac{x^2 + y^2 + z^2}{Q} = t^2; \quad . \quad . \quad (4.)$$

so that $\sqrt{Q}$ will be the semi-diameter of the sphere, and $\sqrt{R}$ the semi-diameter of the equator in the ellipsoid. It is important to observe, that in crystals possessing but one optical axis, these two semi-diameters, or their squares Q, R, always differ very little one from the other, and that in consequence the generating ellipse of the ellipsoid exhibits a very small eccentricity. Hence it also results that the condition (2.) sensibly reduces itself to the following:

$$N = 3\,R,$$

that is to say, to a condition which is fulfilled, whenever the elasticity of a medium continues the same in every direction around any point. We may add, that the intensity of the light determined by calculation for each of the two polarized rays which we are here considering, is precisely that which observation furnishes. As to the third polarized ray, calculation shows that it is very difficult to perceive it, inasmuch as the intensity of the light continues always very small in it when it is not rigorously nothing. We will hereafter seek the means of proving its existence.

Let us now suppose that, in the æthereal fluid, the elasticity ceases to be the same in all directions around an axis parallel to the axis of z. If we cut the surface of the luminous waves by the coordinate planes, the sections made with two sheets of this surface may be reduced to the three circles, and to the three ellipses represented by the equations

$$\left.\begin{array}{ll} \dfrac{y^2}{R} + \dfrac{z^2}{Q} = t^2, & \dfrac{y^2 + z^2}{P} = t^2; \\[2ex] \dfrac{z^2}{P} + \dfrac{x^2}{R} = t^2, & \dfrac{z^2 + x^2}{Q} = t^2; \\[2ex] \dfrac{x^2}{Q} + \dfrac{y^2}{P} = t^2, & \dfrac{x^2 + y^2}{R} = t; \end{array}\right\} \quad . \quad . \quad . \quad (5.)$$

and, in order that this reduction may take place, it will suffice that the coefficients G, H, I, being evanescent, the three conditions

$$\left.\begin{array}{c} (M - P)\,(N - P) = 4\,P^2, \quad (N - Q)\,(L - Q) = 4\,Q^2, \\ (L - R)\,(M - R) = 4\,R^2, \end{array}\right\} \ . \ (6.)$$

all three similar to the condition (2.), be verified. Furthermore, if the eccentricities of the three ellipses are small enough for their squares to be neglected, the conditions (6.) will lead to the following:

$$(M - P)\,(N - Q)\,(L - R) = (N - P)\,(L - Q)\,(M - R) = 8\,P\,Q\,R,$$

and the equation of the wave-surface may be reduced to

$$\left.\begin{array}{l}(x^2+y^2+z^2)\,(\mathrm{P}\,x^2+\mathrm{Q}\,y^2+\mathrm{R}\,z^2)\\ -[\mathrm{P}\,(\mathrm{Q}+\mathrm{R})\,x^2+\mathrm{Q}\,(\mathrm{R}+\mathrm{P})\,y^2+\mathrm{R}\,(\mathrm{P}-\mathrm{Q})\,z^2]\,t^2+t^4=0\end{array}\right\} \quad (7.)$$

Now, the three circles, the three ellipses, and the surface of the fourth degree represented by the equations (5.), (7.), are precisely those which Fresnel has given as indicating the course of the two polarized rays, as hitherto recognized in crystals having two optical axes; and we also know that the eccentricities of the ellipses are very small in these crystals. The conditions (6.) therefore should be sensibly verified there. Finally, it is well to observe, that if the eccentricities should become null, or, in other words, if we had

$$\mathrm{P}=\mathrm{Q}=\mathrm{R}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (8.)$$

the conditions (6.) would give

$$\mathrm{L}=\mathrm{M}=\mathrm{N}=3\,\mathrm{R}, \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

and that the conditions (8.), (9.), are precisely those which ought to be fulfilled, in order that the elasticity of a medium should remain the same in every direction.

With respect to the third polarized ray, as the intensity of its light is very little, it will generally be very difficult to perceive it, as we have already remarked.

In summing up what has been said, we see that the conditions (6.) being supposed to be rigorously fulfilled, the sections made with the surface of the luminous waves by the coordinate planes will coincide exactly with those given by Fresnel. As to the surface itself, it will be but little different from the surface of the fourth degree, which this celebrated philosopher has obtained, and consequently this last is, in the theory of light, what the elliptic motion of the planets is in the system of the world.

The eccentricities of the ellipses, which are the sections of the wave surface, with the coordinate planes, being generally very small for crystals having one or two optical axes, it follows that we can determine, with a close approximation, the velocities of propagation of plane waves in these crystals, and the planes of polarization of the luminous rays, by help of the rule which I shall now give.

To obtain the velocities of propagation of the plane waves parallel to a given plane A B C, and corresponding to the two polarized rays which a crystal of one or two optical axes transmits, it is sufficient to cut the ellipsoid represented by the equation

$$\frac{x^2}{P} + \frac{y^2}{Q} + \frac{z^2}{R} = 1, \quad . \quad . \quad . \quad . \quad (10.)$$

by a diametral plane parallel to the given plane. The section thus obtained will be an ellipse, whose two axes will be numerically equal to the velocities of propagation of the plane waves in the two rays. Furthermore, of these two rays, that one in which the plane waves propagate themselves with a velocity represented by the major axis of the ellipse, will be polarized parallel to the minor axis; and reciprocally the ray in which the plane waves propagate themselves with a velocity represented by the minor axis of the ellipse, will be polarized parallel to the major axis. If we make the plane A, B, C, coincide with one of the principal planes of the ellipsoid, the two polarized rays will follow the same course, and the two velocities of light in these rays will be precisely the velocities of propagation of the plane waves. Consequently, the velocities of light in the six polarized rays, whose directions coincide with the three axes of the ellipsoid, are two by two equal with one another, and to one of the numbers $\sqrt{P}$, $\sqrt{Q}$, $\sqrt{R}$. Let us add, that the two rays, whose velocity is $\sqrt{P}$, are polarized perpendicularly to the axis of x; those whose velocity is $\sqrt{Q}$, perpendicular to the axis of y, and those whose velocity is $\sqrt{R}$ perpendicular to the axis of z. In the particular case where the quantities P Q, become equal to one another, the surface represented by the equation (10.), or

$$\frac{x^2 + y^2}{Q} + \frac{z^2}{R} = 1, \quad . \quad . \quad . \quad . \quad . \quad (11.)$$

becomes an ellipsoid of revolution, the axis of which is what we call the optical axis of the crystal. Then, one of the semi-axes of the section made by any diametral plane, is constantly equal to $\sqrt{Q}$, as well as the velocity of the light in one of the two polarized rays. The ray in question is that which we name "the ordinary ray," and it is polarized parallel to the right line, which in the plane A B C forms the least and the greatest angle with the optical axis; whilst the other ray, called "the extraordinary ray," is polarized parallel to the right line of intersection of the plane A B C, and of a plane perpendicular to the optical axis. Also, the two rays, ordinary and extraordinary, *are superposed*, when they are in the direction of the optical axis, and are reduced to a single ray, which no longer shows any trace of polarization.

When the three quantities P, Q, R, are unequal, the ellipsoid, represented by the equation (10.), may have circular sections formed by two diametral planes, both of which include the mean axis; therefore, the two polarized rays are superposed when the plane waves become parallel to one of these planes. Then, the common direction of the two rays is what is called an optical axis; for crystals then, in which the elasticity of the æther is not the same in every direction around an axis, there exist two optical axes, in the direction of which the rays show no further trace of polarization.

All these consequences of our analysis are conformable with experience, and even, in the lectures given at the Royal College of France, M. Ampère had already remarked that the construction of the ellipsoid, represented by the equation (10.), furnishes the means of determining the velocities of propagation of the plane waves, and of the planes of polarization of the luminous rays. Only these planes, which were supposed *perpendicular* to the directions of the proper velocities of the æthereal molecules, on the contrary *contain* these same directions.

We will add, that for the equation (10.), the following might be substituted:—

$$P x^2 + Q y^2 + R z^2 = 1 \quad . \quad . \quad . \quad . \quad . \quad (12.)$$

In fact, the two sections made by one and the same plane, in the two ellipsoids which the equations (10.) and (12.) represent, have their axes parallel, and those of the second section are respectively equal to the quotients which are obtained by dividing unity by the axes of the first.

P.S. In order that the principles above set forth should be better comprehended, I shall unfold, in a second Memoir, the different formulæ which I have only mentioned here. I will also make two important remarks with regard to the same principles; first, when we speak of the mutual attraction or repulsion of the molecules of an æthereal fluid, we should only understand that, in the theory of light, everything goes on as if the molecules of æther effectively attracted or repelled each other. Thus, the investigation of the laws which the very diversified phænomena of the propagation, reflection, refraction, etc., of light present, is reduced to the discovery of a more general law, which includes all the others. It is thus that, in the system of the universe, we reduce the determination of the laws, according to which the celestial bodies move to the single hypothesis of universal gravitation.

In the second place, I shall remark, that, in order to establish the propositions put forth in this Memoir, we have had recourse to the formula (68.), of page 208 of the "*Exercices de Mathématiques*," and that, in order to reduce the differential equations of the motion of a system of molecules acted upon by the forces of mutual attraction or repulsion to the formulæ in question, we are compelled to neglect several terms; for example, those which include the superior powers of the displacements ξ, η, ζ, and of their derivatives taken in relation to the independent variables x, y, z. When we do not neglect these same terms, we obtain, as I shall show in a new Memoir, already presented to the Academy, formulæ, by help of which we are able not only to assign the cause of the dispersion of colours by the prism, but also to discover the laws of this phænomenon, which, notwithstanding the numerous and important labours of natural philosophers on this subject, had remained unknown until the present day.

Note.

[The investigations here promised, as well as those before referred to, as to be published, seem to be furnished in the author's subsequent papers, in his *Exercices de Mathématiques*, in his *Mémoire sur la Dispersion* (Paris, 1830), and his *Nouveaux Exercices*, &c. (Prague, 1835). But the more full developments of his views is to be found in a lithographed Memoir, published in Aug. 1836, *sur la théorie de la lumière*, &c.—Ed.]

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ARTICLE XII.

Researches on the Cacodyl Series. By RUD. BUNSEN *.

IN the following researches I have examined some of the numerous products which arise from the decomposition of alcarsin. They are not only well worthy of attention from the peculiarity of their composition and characters, but also as members of a series of compounds equally unusual and extensive, and must possess a general interest in extending our knowledge of the laws which regulate organic combinations.

The simple relation which exists among these members appears as the direct expression of the facts observed, and leads us to a view of the constitution of this class of bodies, which is also supported by the characters of the inorganic elements, and which seems to exclude every other method of interpretation.

When we survey this group of substances, we immediately recognize an unchangeable member whose composition may be represented by

$$C_4 H_6 As_2 \dagger,$$

which, in respect of the relative number of its atoms (but only in this respect), answers to common alcohol, in which the atom of oxygen has been replaced by half an equivalent of arsenic. The constituent elements of this member are united by a powerful affinity to each other, and collectively taken act a part in all the phænomena which attend and characterize the decomposition of these bodies. They form in this combination a unit of a higher order, which we call an organic atom or radical, which assumes a rank among the combination of organic elements, as figures do in our numerical systems, being subject at the same time to the laws of the original units, by whose aggregation they have been formed. As respects organic chemistry, the unparalleled power of affinity with which this radical is endowed, the facility with which it can be transferred from one compound to another, the numerous proportions in which it combines with the metalloids, and, above all, the electro-chemical character of the compounds

* Translated from the German MS. of the author by Thomas Richardson, Esq.

† In this translation H, Cl, &c. represent the double atoms or single equivalents, as used by English chemists. As represents 470·04 arsenic, and Hg 1265·82 mercury.

which result, afford a striking example of accordance between the laws of organic and inorganic bodies. This harmony may not, perhaps, be without some influence in a science where every idea is supported by analogical reasoning. In consequence of the difficulties which attend the analyses of these substances, it appears to me necessary that I should at first make some general observations on the nature of the analytical methods employed in the following researches.

Nitric acid oxidizes arsenic only imperfectly, but it is performed, without loss, by means of a red heat. Neither oxide of copper, nor such bodies as give off oxygen when heated, can be employed as the substance for combustion, because in the first case, the separation afterwards of the arsenic and copper is attended with insuperable difficulties, and in the other it is impossible to avoid the most dangerous explosions. I have therefore employed a fused mixture of Glauber-salt, bisulphate of soda and powdered glass. The experiment even succeeds better by using oxide of zinc or oxide of nickel free from arsenic. In preparing the latter in the best form for analysis, the sulphate should be used, which loses its acid without melting at a white heat, and leaves the oxide behind in the form of a very fine powder, which is much more bulky than the oxide of copper prepared in a similar way from the nitrate. When the nickel of commerce is used, a current of sulphuretted hydrogen gas ought to be passed through the warm sulphuric acid solution at least eight or ten days, as it usually contains molybdenum, which is thrown down with great difficulty by this reagent. The estimation of the carbon and hydrogen cannot be accomplished at the same time by means of this substance, as it only perfectly oxidizes the arsenic. In order to avoid any loss, the combustion tube ought to extend some inches out of the furnace, and be connected with a Liebig's condenser filled with water. The insupportable smell of alcarsin which is absorbed by the fluid in the condenser, quickly indicates an imperfect combustion of the arsenic, which however seldom happens in a well-executed experiment. The contents of the combustion tubes are then dissolved in aqua regia by a gentle digestion, sulphuric acid added, and the whole evaporated to get rid of the nitric acid, again heated with water and filtered. The solution thus obtained contains the arsenic, which can be determined in the usual way by means of sulphuretted hydrogen.

The analysis of the other constituents of these substances, whether volatile or not, can be determined without difficulty by any of the methods already known. As many of these substances become solid by absorption of oxygen, it is dangerous to cut off the points of the glass balls which contain the fluid destined for analysis outside of the combustion tube, for the solidifying of a portion in the tube might cause an explosion during the course of the experiment. In weighing out these fluids I have therefore used small glass tubes, one end of which has been drawn out into a fine point three or four inches long. They also possess the advantage, that the weight and contents can be marked on the outside by means of a diamond, and a great many filled at one time, which is very necessary, in consequence of the ease with which these substances are decomposed by contact with the air. Ten or fifteen of these small tubes are then filled with carbonic acid, and all connected together by a piece of platinum wire. The larger end is heated and immediately immersed in a tube, (previously hermetically sealed but one end broken off at the moment,) to allow all the fine points to dip into the fluid, which then immediately rises up in these small tubes as far as the heated part. As soon as this takes place the fluid must be made to enter the body of the tube by a gentle jerk. The fluid then boils and drives out all the carbonic acid, and on cooling the tube is filled with fluid for analysis. This operation may be performed in thirty seconds, and it is impossible to fill the tubes one by one, as the fluid usually becomes so muddy in the course of three or four minutes as to be quite unfit for analysis. These small tubes can be easily opened inside of the combustion tube, (which has been bent and then again drawn out in the horizontal direction,) by means of a wire bent in the form of a cork-screw. The estimation of the hydrogen and oxygen is more difficult, if other substances are present. It can however be accomplished with oxide of copper by using a longer combustion tube, the fore-part of which is filled with chromate of lead or copper chips. Some of these compounds cannot be analysed by means of oxide of copper, as they are suddenly decomposed before the termination of the combustion; I have therefore employed chromate of lead.

In concluding these remarks, I ought to notice one thing more, which is of great importance in preparing these substances in a state of purity, viz. never to use larger vessels in the experiment than is absolutely necessary. It is also of great consequence, in order to avoid the most dangerous explosions, to fill the apparatus which is intended to hold these substances most carefully with carbonic acid. The smell which most of these substances diffuse is frightful. It immediately produces vomiting in delicate persons, and appears especially to affect the nerves when long exposed to its action. The only cases of rapid poisoning take place from cyanide of cacodyl, which produces giddiness, stupefaction, and even fainting, when diffused in the smallest quantity in the atmosphere. I am myself however convinced, by a long and continued study of these compounds, that however tedious it may be, in consequence of the numerous precautions necessary, every danger may by a little care be avoided.

I. THE LOWER DEGREES OF COMBINATION OF CACODYL.

A. *Amphide Compounds of Cacodyl.*

1. *Oxide of Cacodyl.*

The great difficulties in the way of the direct estimation of the oxygen of this substance, which I formerly designated by the empirical name alcarsin, induced me to believe at first that it contained none, and this supposition received considerable countenance from its remarkable property of spontaneous combustion and its reactions with potassium. The peculiar mode of its formation, from the acetic acid salts with alkaline bases, does not however favour this supposition, as it presupposes a decomposition equally complicated and unusual. Berzelius concluded that it most probably contained oxygen, which is easily explained by abstracting two atoms of carbonic acid from two atoms of acetone (hydrated oxide of œnyl) and one atom of arsenious acid, which would leave one atom of alcarsin behind, as in the following equation:—

2 at.	Acetone	C_6	H_6		O_2
1 ...	Arsenious acid . .			As_2	O_3
2 ...	Carbonic acid . . .	C_2			O_2
1 ...	Oxide of cacodyl .	C_4	H_6	As_2	O

In order to determine this question experimentally, I have instituted a new series of most careful researches on this substance, and as the results of the same have meanwhile been published, I should have considered a detailed exposition of them here as unnecessary, had not Dumas more lately asserted the truth of his former experiments on the same subject, which do not correspond with my results.

Although this celebrated chemist does not appear to have prosecuted his experiments so far as the nature of the difficulties connected with them seemed to require, deterred perhaps by the offensive characters of the substance, yet I cannot hesitate to oppose the following results to an authority such as his, even where he has himself regarded his own results as decisive.

He determined the arsenic partly by oxidation with aqua regia in a retort which was connected with a receiver, collecting the liquid condensed and weighing the arsenic acid after it had been thoroughly dried; partly also by conducting oxygen gas through the combustion tube till the arsenic was perfectly oxidized, and then determining the weight of the arsenious acid formed by the loss of weight in the combustion tube. In the first instance he obtained 69·3 per cent., and in the second 68·93 and 69·0 per cent. I have also, in my former paper on this subject, detailed an experiment in which I obtained a result similar to the first of Dumas; but with this difference, that the oxidation of the arsenic was conducted in a glass tube hermetically sealed, and the arsenic estimated not as arsenic acid, but as arseniate of iron. Any loss of arsenic in my experiment was therefore impossible. Nevertheless I only obtained, under the most favourable circumstances, 64·2 per cent., and consequently less than I ought to have had according to theory. I formerly attributed this loss to the imperfect oxidation of the arsenic, which may be easily shown by heating the solution, which has no smell, with chloride of tin or sulphuretted hydrogen. In the first case the frightfully penetrating and stupifying smell of chloride of cacodyl appears, and in the latter the no less characteristic odour of sulphuret of cacodyl. The solution must therefore have contained alcargen (cacodylic acid), which is not decomposed even by evaporation to dryness, and being reduced by the above deoxidizing agent is converted into the corresponding sulphur and chlorine compounds. It is therefore probable that the substance analysed by Dumas was not pure, or that the arsenic acid from which the weight of the arsenic was deduced had retained some water. Neither could the other method by weighing the combustion tube afford an accurate result, which would be too high, for arseniate of copper would be formed along with the arsenious acid, as Dumas himself had conjectured.

It appeared to me, therefore, that the greatest care was necessary in the preparation of the oxide of cacodyl in order to determine the question.

Although it is impossibe to prevent the anhydrous substance from decomposing, with the formation of alcargen, by contact with the air, it may yet be accomplished by employing the apparatus formerly described as the distillation tube*. The method formerly given is however attended with some great inconveniences, as we are compelled, in driving out all carbonic acid, to allow such a quantity of inflammable alcarsin vapour to pass out at the same time that everything near becomes covered with arsenious acid. It is most advisable, in order to avoid this inconvenience, to commence the distillation without driving the carbonic acid out beforehand, and which can be performed without any danger, provided the leg destined to receive the condensed vapour be of sufficient length. The bursting of the apparatus from the tension of the vapour is prevented very easily, by keeping that portion of the tube which is destined for the reception of the same, cool in water at 8° to 10° C., and by hastening the distillation as much as possible. It is quite impossible to prevent an explosion if the distillation is carelessly conducted, or the heat so long continued that permanent gases are evolved, or allowing that portion of the bulb above the level of the liquid to become too hot. Should a drop of the liquid during the ebullition fall on that part, the whole apparatus is shattered to pieces, and an arsenious flame several feet high rises up, covering everything near with a black layer of offensive arsenic. It is therefore best to conduct this distillation behind some boards, in which a small glass pane is fixed, through which we can see how to regulate the operation. After it is finished the lamp can be removed by the operator by means of a string conveniently fixed in the board, and there is no more danger so soon as the apparatus has cooled.

I have not thought it too great a digression to go into all these details, as nearly the whole of the compounds belonging to this series can be procured pure only in the moist way.

The product of the distillation was divided into two portions.

* Memoirs of the Chemical Society of London, p. 51.

The alcarsin prepared from the first portion of the distillation afforded the following results :—

I. 0·548 grm. substance yielded 0·2688 grm. water, and 0·4532 carbonic acid.

That prepared from the second portion furnished—

II. 0·5974 grm. substance 0·2915 grm. water, and 0·49 carbonic acid.

The experiments correspond with

	I.	II.		Calculated.
Carbon . .	22·86	22·68	C_4	21·52
Hydrogen .	5·44	5·42	H_6	5·27

These members, compared with those given in my first memoir on this subject, appear to lie between them. This result, as well as the other, cannot be reconciled with the opinion that there is oxygen present, nor with the contrary one. In the first case the proportion of carbon and hydrogen is too high, and in the last too low. It results therefore from the accordance between the different analyses, that the constant slight variation depends most probably more on the impurity of the substance than on an error in the analysis. This opinion is strengthened by the fact, that the substance analysed above, when heated with muriatic acid, yielded, besides the chloride of cacodyl, a brick-red powder, insoluble in everything, and which appears also to be formed when alcarsin is conducted through heated tubes; I have therefore executed the distillation of the crude fluid of Cadet under a layer of water freed from atmospheric air by boiling, which not only prevented all access of air, but also dissolved any of the oxidized substances that might happen to be formed, and effectually prevented the temperature rising so high as to cause any decomposition. In this way I obtained alcarsin in a state of perfect purity, as the following experiments will show:—

I. 0·4711 grm. substance gave 0·2235 grm. of water, and 0·3708 grm. carbonic acid.

II. 0·5164 grm. substance gave 0·2485 grm. of water, and 0·4015 grm. carbonic acid.

0·5258 grm. substance, when burnt with oxide of nickel, yielded 0·7142 grm. sulphuret of arsenic.

0·711 grm. of the above, treated with nitric acid, yielded 0·2131 grm. melted sulphur, and 1·2332 grm. sulphate of barytes.

On repeating this experiment, 0·5504 grm. yielded 0·7911 grm. sulphuret of arsenic; of which 0·7448 grm. produced by oxidation 0·2233 grm. of sulphur, and 1·342 grm. sulphate of barytes. The first experiment is equivalent to 66·12 per cent., the last to 65·38 per cent., which corresponds very closely with the theory. The whole results are, therefore,

		Calculated.	I.	II.
C_4 . .	305·74	21·52	21·76	21·65
H_6 . .	74·88	5·27	5·27	5·34
As_2 . .	940·08	66·17	66·12	65·38
O . .	100·00	7·04	6·85	7·63
	1420·70	100·00	100·00	100·00

I have determined the specific gravity of the vapour of alcarsin again, because that given in my previous memoir agrees more with my former supposition than with the above composition. In order to avoid any error arising from the oxidation of the alcarsin, I prepared a new quantity expressly for this experiment, and boiled the mercury in the measuring receiver beforehand, so that the liquid collected on the top of the mercury, after the experiment was concluded, was quite free from any air.

Liquid in the small glass tube . . .	0·2414 grm.
Volume of the vapour	47·04 C. C.
Temperature of the oil-bath	148·5 centig.
Higher level of the mercury in receiver	74 lines.
Pressure of the oil column	148 ...
Barometer	335·5 ...

From these data the specific gravity of the alcarsin is 7·555; and that calculated, is

4 vols.	Carbon	3·3713
12 ...	Hydrogen	0·8256
2 ...	Arsenic	10·3654
1 vol.	Oxygen	1·1026

15·6649 : 2 = 7·8324.

When we remember that in estimating the specific gravity of vapours by the method of Gay-Lussac the result must be too low, the difference above between that calculated and observed will appear small. I must confess, however, that the difference did at first appear to myself inexplicable, as I had used every possible care, and employed the very best instruments. We must also recollect, that a difference even quite within the limits of observation and experiment in vapours of great density, exercises a serious influence on the ultimate result; that these errors of observation increase with the boiling point of the fluid; and lastly, that the small quantity of protoxide with which the mercury is mechanically mixed, may occasion a considerable oxidation of the alcarsin.

The experiments do not therefore leave any doubt that the composition of the alcarsin is represented by the empirical formula $C_4 H_6 As_2 O$; and the order in which these elements are grouped among themselves, is no less clearly shown in the transformations which this substance undergoes. When we observe that it is the oxygen which is increased or diminished, displaced or substituted; when we observe, further, that these changes do not take place with the carbon, arsenic, or hydrogen, which remain united together in the same relative proportion, in the long series of these compounds, we are compelled to regard this unchanging atomic group as a collective member, a unity, and as such only to play a part in the decompositions of this class of bodies. This member, which we call cacodyl, and denote by the expression $C_4 H_6 As_2 = Kd$, passes from the region of hypothesis to that of reality, when we find in it those laws and characters which are now regarded in our science as sufficient to justify us in forming some conception of the constitution of inorganic bodies. How far this is applicable to the class of bodies before us, will be seen from the following researches.

If we apply this view to explain the constitution of alcarsin, we must regard it as the lowest degree of oxidation of cacodyl, and represent it by the rational formula $C_4 H_6 As_2 + O = Kd\,O$. The characters which we observe perfectly accord with this view. It is a saline base, and approaches those inorganic oxides which, although possessing the characters of weak acids, must be regarded as bases. It does not react either alkaline or acid, but unites with acids and forms peculiar substances soluble in water.

Phosphoric acid dissolves a considerable quantity, and forms an offensive viscous fluid, which can neither be crystallized nor rendered neutral. When it is heated, pure water at first passes off, and then mixed with alcarsin, which retains all its characters unchanged, and the phosphoric acid remains behind quite pure in the retort.

Diluted nitric acid unites with it in the cold without decomposition, and forms a thick liquid; concentrated or hot acid immediately produces oxidation, with the formation of alcargen.

The sulphate of cacodyl can be procured in a crystallized state. It may be formed by digesting the oxide in concentrated sulphuric acid. The liquid changes on cooling into a white mass, which consists of groups of small crystalline needles. These crystals can be purified by pressure between folds of bibulous paper: they have an acid reaction, and deliquesce on exposure to air. The odour arising from them is most odious.

I did not consider it necessary to prosecute the examination of these salts, as they can possess no other interest than what arises from the fact of their existence. The precipitates, however, which the nitrate of cacodyl produces in metallic solutions, are much more remarkable. My experiments to determine their nature have all failed, in consequence of their instability. What I have made out is confined to a few changes, but I must defer their prosecution to another opportunity.

The oxide of cacodyl agrees perfectly with inorganic bases in its reactions with the hydrogen acids. Haloid salts and water are formed; the latter is either separated, or more seldom remains combined. The affinity of oxygen for the oxide of cacodyl is remarkably energetic. It not only combines directly, but expels other bodies from their combination with that oxide: oxide of mercury, oxide of silver, oxide of gold are reduced, and even arsenic acid and indigo undergo a deoxidation. The characters of the degree of oxidation which thereby results, is already known from the laws of the compounds of the inorganic kingdom. The electro-chemical character, in both cases, depends upon the number of the atoms of oxygen which combine with the radical. Besides alcargen (cacodylic acid), there appears to be an intermediate degree of oxidation, which reacts as a superoxide. In another paper I will return to this subject, as well as to the other corresponding compounds of cacodyl.

In concluding these remarks I may perhaps observe, that this oxide of cacodyl is a remarkably delicate test for arsenious acid, and may be used in judicial investigations as a most simple and safe means of distinguishing between antimony and arsenic. Boil the matter containing arsenic in Marsh's apparatus with some water containing air, till the whole is dissolved, and mix the solution with a little potash and acetic acid. On evaporating to dryness, we obtain a substance, which, when heated to redness in a glass tube, emits the repulsive odour of alcarsin. This odour is changed into the no less characteristic one of the chloride of cacodyl, by moistening the inside of the tube with a few drops of chloride of tin. Oxide of antimony does not react in the same way. The oxide of cacodyl can also be employed to detect acetic acid salts in mixed liquids, by treating the same with potash and arsenious acid, evaporating, and heating to redness: the addition of potash is necessary, because alcarsin is only formed from the acetate of the alkaline bases.

2. *Sulphuret of Cacodyl.*

This substance is obtained by distilling a mixture of chloride of cacodyl and hydrosulphuret of barium in solution, when a great escape of sulphuretted hydrogen takes place. The sulphur compound passes over as soon as the temperature rises to the boiling point, while chloride of barium remains behind in the retort.

$$\left.\begin{array}{l} \mathrm{Kd\,Cl} \\ \mathrm{Ba\,S + S\,H} \end{array}\right\} = \left\{\begin{array}{l} \mathrm{Kd\,S} \\ \mathrm{Ba\,Cl} \\ \mathrm{S\,H} \end{array}\right.$$

Sulphuret of barium will not answer, because the chloride of cacodyl generally contains some oxide of cacodyl mixed with it, which is decomposed by sulphuretted hydrogen and not by sulphuret of barium. It is therefore necessary to employ the above salt in order to obtain a product free from oxide. A small quantity of a viscous offensive matter, which consists of sulphur and a solution of the persulphurets in protosulphuret of cacodyl, remains behind in the retort; it is formed at the expense of the hyposulphuret of barytes and the bisulphuret of barium, with which the salt employed above is usually mixed; for in the decomposition of these mixed products the uncombined sulphur combines with the protosulphuret of cacodyl and forms the solid persulphuret. The sulphuret of cacodyl dissolves any sulphuret of iron which may be present of a blue colour: the distilled fluid however does not possess this colour. It is necessary to distil it a second time with the sulphur salts to ensure a perfect decomposition. The access of the air need not be so carefully prevented so long as the liquid is covered with a layer of water holding sulphuretted hydrogen in solution, because that gas decomposes any oxidized product of the cacodyl, and therefore no oxidation can take place while the gas is present. The water and excess of sulphuretted hydrogen are separated by chloride of calcium and carbonate of lead. The access of the air must be most carefully prevented as soon as the lead salt causes no black coloration, and especially in filling the distillation tube. In this latter operation the substance is entirely freed from any persulphuret of cacodyl which may possibly have been present, which remains behind in the leg of the tube in the form of a yellow, nauseous, viscous fluid mixed with some crystalline grains.

This substance can also be procured in large quantities from the acid liquid which is obtained in the preparation of the fluid of Cadet. The large proportion of oxide of cacodyl which remains dissolved in the acetic acid in that liquid, can be precipitated as sulphuret of cacodyl by adding the sulphur salts already mentioned; for this substance is nearly as insoluble in that acid liquid as in water. The decomposition which takes place is as follows:—

$$\left.\begin{array}{l}\mathrm{Kd\,O} \\ \mathrm{Ba\,S} + \mathrm{S\,H} \\ \overline{\mathrm{A}}\end{array}\right\} = \left\{\begin{array}{l}\mathrm{Kd\,S} \\ \mathrm{Ba\,O} + \overline{\mathrm{A}} \\ \mathrm{S\,H}\end{array}\right.$$

The formation of this sulphur compound from alcargen (cacodylic acid), is perhaps more interesting in a theoretical point of view than useful as a means of preparing it for research. The same phænomenon is observed in conducting a current of sulphuretted hydrogen through an aqueous solution of cacodylic acid, as takes place with many inorganic oxides; as for example arsenic acid, which is reduced with the separation of sulphur and formation of its sulphur compound.

$$\left.\begin{array}{l}\mathrm{Kd\,O_4} + \mathrm{aq} \\ 4\,\mathrm{S\,H}\end{array}\right\} = \left\{\begin{array}{l}\mathrm{Kd\,S} \\ 4\,\mathrm{H\,O} \\ 3\,\mathrm{S}\end{array}\right.$$

Persulphuret of cacodyl is formed, on the contrary, when we use an alcoholic solution, because the sulphur which separates immediately combines with the sulphuret of cacodyl.

Sulphuret of cacodyl is a limpid æthereal liquid, which does not smoke in contact with the air, and has a most penetrating offensive odour, which is very similar to that of mercaptan and alcarsin, and adheres for a long time to any substance. It does not solidify at a temperature of − 40° C. It distils over with water, although its boiling point is considerably higher. When it is heated to redness in a glass bulb, it deposits the vapours of arsenic, sulphuret of arsenic, and charcoal. It inflames in common air, and burns with a pale arsenious flame, which is coloured bright blue on the exterior. The sulphur is oxidized when it is treated with moderately concentrated nitric acid, but the cacodyl does not undergo any change. It is almost insoluble in water, but the water acquires its disagreeable penetrating odour. It is miscible with æther and alcohol in any proportion, and is again precipitated from the latter by adding water. Sulphur combines with it when in the anhydrous form, or dissolved in alcohol, and forms a sulphur combination of a higher order, which can be obtained from æther in a crystalline form. This body possesses very remarkable properties, and I shall recur to it in another place. A similar compound is formed with selenium, which crystallizes in large colourless plates. It dissolves phosphorus, which is deposited again unchanged on cooling. It forms a most peculiar crystalline substance with iodine. It is converted into large transparent crystals in contact with oxygen, which consist of alcargen and another substance not yet examined.

Muriatic acid decomposes the sulphuret of cacodyl into chloride of cacodyl and sulphuretted hydrogen. Sulphuric and phosphoric acids also expel sulphuretted hydrogen, and form corresponding salts of the oxide of cacodyl. The weaker acids, as acetic, &c., do not effect this decomposition. Carbonate of lead also is not decomposed by sulphuret of cacodyl.

The analysis of this substance is attended with no difficulty: the sulphur is determined by oxidation with nitric acid. 1·309 grm. heated with nitric acid, with the usual precautions, afforded 1·061 grm. sulphate of barytes, and 0·013 grm. sulphur.

Burnt with chromate of lead,

I. 0·9713 grm. yielded 0·720 grm. carbonic acid, and 0·4385 grm. water.

II. 0·902 grm. yielded 0·664 grm. carbonic acid, and 0·407 grm. water.

If we estimate the arsenic by the loss, we obtain the following calculated and experimental results:—

	I.	II.	Calculated.
Carbon . . . C_4	20·49	20·35	20·1
Hydrogen . . H_{12}	5·02	5·01	4·9
Arsenic . . . As_2	63·32	...	61·8
Sulphur . . . S	12·17	...	13·2
	100·00		100·0

The difference as regards the sulphur, which is scarcely 1 per cent., is easily explicable from the unavoidable loss in oxidation by means of nitric acid.

The facility with which the sulphuret of cacodyl is oxidized, renders it impossible to determine the specific gravity of its vapours by Dumas's method; and, in short, an approximation is all that can be obtained by Gay-Lussac's method, in consequence of the temperature, 200°, approaching so nearly the point at which the substance is decomposed with the formation of sulphuret of mercury.

The following result was obtained from a very carefully conducted experiment:—

Substance employed	0·194 grm.
Barometer	328 lines.
Mercurial pressure (to be deducted)	48·9 ...
Temperature	215° centig.
Volume of vapour	41·6 C. C.

The specific gravity calculated from the above, after making every correction, is 7·72, and that estimated is

4 vols.	Carbon	3·3712
12 ...	Hydrogen	0·8256
2 ...	Arsenic	10·3654
1 vol.	Sulphur	2·2180
		16·7802 : 2 = 8·39.

Although the difference between the calculated and observed specific gravities amount to nearly one-tenth, it is fully explained from the circumstances already noticed; and I think the result justifies me in concluding that the relative condensation of the sulphuret of cacodyl agrees with that of the oxide.

3. *Seleniet of Cacodyl.*

This compound is formed in a similar manner to the corresponding sulphuret. Pure chloride of cacodyl must be distilled two or three times with a solution of seleniet of sodium.

$$\left.\begin{matrix}\text{Kd Cl}\\ \text{Na Se}\end{matrix}\right\}=\left\{\begin{matrix}\text{Kd Se}\\ \text{Na Cl}\end{matrix}\right.$$

The liquid which passes over with the aqueous vapour has a yellow colour, and collects at the bottom of the receiver in the form of heavy oily drops. The mode of purifying is the same as that employed with the sulphur compound.

The seleniet of cacodyl obtained as above is a perfectly transparent yellow liquid, possessing a peculiarly offensive and highly penetrating smell, which is somewhat similar to that of the sulphuret, but more æthereal and aromatic. It is insoluble in water, but very soluble in æther and alcohol. It does not smoke in contact with the air, but in a short time absorbs oxygen and deposits colourless crystals. It is less volatile than any of the other cacodyl compounds, but may be distilled without decomposition. It deposits selenium and a ring of arsenic when conducted through a heated glass tube. It burns in the air with a fine blue flame, and emits the penetrating odour of oxide of selenium.

It reacts with metallic solutions in the same way as the inorganic metallic compounds of selenium; a seleniet and oxide of cacodyl in combination with the acid of the metal result. It causes a black precipitate with acetate of lead, nitrate of silver, &c. It produces a black precipitate of seleniet of mercury in a solution of corrosive sublimate; but on adding a larger quantity it is changed into the white compound of oxide of cacodyl and chloride of mercury, which easily dissolves in boiling water, and on cooling forms silky crystalline plates.

$$\left.\begin{array}{l} \mathrm{Kd\,Se} \\ 3\,\mathrm{Hg\,Cl} \\ \mathrm{H\,O} \end{array}\right\} = \left\{\begin{array}{l} \mathrm{Hg\,Se} \\ \mathrm{Kd\,O} + 2\,\mathrm{Hg\,Cl} \\ \mathrm{H\,Cl} \end{array}\right.$$

It is easily oxidized by nitric acid; and the same effect is produced by concentrated sulphuric acid, when heated, with the formation of sulphurous acid, while a red powder of selenium is deposited.

I did not consider it necessary to make any analysis of it, as there can be no doubt of its composition.

B. *Haloid Compounds of Cacodyl.*

4. *Cyanuret of Cacodyl.*

I have thought it best to commence this division with the description of this beautiful but highly poisonous compound, on account of the extraordinary facility with which it crystallizes, and the ease with which it may be obtained perfectly pure. It may be prepared by distilling a mixture of concentrated prussic acid and oxide of cacodyl. This method of preparation is attended with great danger, and affords a product which is rendered very impure from a large mixture of the oxide. It is also extremely difficult to purify it by crystallization, as the latter is easily oxidized and the cyanuret is so truly fearful as a poison. Its production will be understood from the following equation :—

$$\left.\begin{matrix} \text{Kd O} \\ \text{H Cy} \end{matrix}\right\} = \left\{\begin{matrix} \text{Kd Cy} \\ \text{H O} \end{matrix}\right.$$

It affords an example of a very common phænomenon attending the ordinary decomposition of inorganic oxides. The substance which was submitted to the following inquiries was prepared in a much shorter and less dangerous way. On mixing a concentrated solution of cyanide of mercury with oxide of cacodyl, the mercury is deposited, this cyanogen compound formed, and the other portion of the oxide is more highly oxidized. Not the least trace of prussic acid or oxide of cacodyl passed over on distillation, and the only impurity mixed with the cyanide of cacodyl is the higher oxide of cacodyl. The cyanide forms a yellow oily layer under the water; after a short time nearly the whole crystallizes in large well-formed prisms, which sometimes shoot up into the water lying above. The water and the liquid portion is poured off, and the crystals dried between folds of blotting paper. It is absolutely necessary to conduct this operation in the open air, and to respire through a long glass tube which passes out of the influence of this volatile cyanide.

The crystals obtained possess quite a diamond lustre, and are nearly pure: they must be melted, freed from water by means of barytes in the distillation tube filled with carbonic acid, and about one-half distilled. The distilled portion contains traces of foreign matters, and in order to separate these I pursued the following plan. The leg of the distillation tube containing the anhydrous crystals is broken off, and they are immediately transferred into one end of a glass tube, bent at right angles, and filled with carbonic acid. The open end of the glass tube must be quickly melted: the shorter end of the glass tube containing the crystals is immersed in water heated to 50° or 60° C., when the crystals melt, and by allowing the liquid to cool very slowly, it crystallizes in large prisms, which are surrounded by a portion of the fluid. When two-thirds of the whole has crystallized the remaining liquid portion is poured into the longer leg of the glass tube. This is repeated as long as the liquid has a yellow colour; and when this disappears, the portion remaining in the short leg of the glass tube may be regarded as perfectly pure.

The cyanide of cacodyl thus obtained, forms at 33° C. an æthereal colourless liquid, which is strongly refractive, and at 32·5° C.* becomes a mass of large crystals of a diamond lustre, which are similar to frozen moisture on the surface of glass, and do not differ much from arsenic acid in appearance. This substance has a great tendency to crystallization, and is deposited in extraordinary large crystals by slowly cooling the liquid which comes off from the first crystallization. The crystals may however be obtained much more beautiful, by allowing the substance to sublime at the ordinary temperature and deposit itself on a glass tube moistened with water. They sometimes attain a length of four or five lines in this way. The form is a slightly oblique four-sided prism, the edges of the smaller sides being truncated. It was impossible to make any accurate measurement of the angles, in consequence of the volatility of this poisonous compound. This substance appeared to boil at a temperature somewhere about 140° C. It inflames when heated in the open air, and burns with a reddish blue flame, emitting a strong smoke of arsenious acid. It is slightly soluble in water, but very soluble in æther and alcohol. It appears to be the most poisonous of all the cacodyl compounds. It produces a sudden cessation of muscular power in the hands and feet, giddiness and insensibility, which ends in total unconsciousness. These effects do not however last long, provided the person is not long exposed to the action of its fumes.

A solution of silver is precipitated as cyanide of silver: nitrate of the protoxide of mercury is reduced, but no effect is produced in a solution of the nitrate of the peroxide. Chloride of mercury is immediately precipitated in combination with the oxide of cacodyl. A solution of this substance mixed with salts of the two oxides of iron, precipitated by potash, and heated with acetic acid to redissolve the hydrated precipitate, did not produce any prussian blue; but on adding a stronger acid as the solvent it was immediately formed. Weak acids do not decompose it, but the stronger acids act in precisely the same way as with the other soluble cyanides.

The carbon and hydrogen were easily determined by the usual

* In determining the melting point of this substance I employed a very convenient and safe method, which might be applicable in many other cases. A very thin glass tube is partly filled with the substance and closed at both ends. This is immersed in water heated to different degrees of temperature, and it is thus very easy to determine the melting point with great accuracy.

methods. The first analysis was made with chromate of lead, and the second with oxide of copper.

I. 0·5160 grm. of substance yielded 0·5285 grm. carbonic acid, and 0·2123 grm. water.

II. 0·3870 grm. of substance yielded 0·388 grm. carbonic acid, and 0·1625 grm. water.

The experiments which were made to determine the azote all failed, as the proportion to the carbon is so very small, and the substance undergoes such an unequal combustion. The first tubes indicated the relation of the azote to the carbon as 1 : 6·7, the intermediate ones as 1 : 11, and the last as 1 : 5·3. I therefore employed a new method which is much simpler, and in which all the errors that more or less accompany the old plans may be easily avoided. As I intend to make this method the subject of a separate communication, I will not here enter into particulars, further than to say, that not more than from 0·03 to 0·08 grm. of substance with 2 grms. of oxide of copper, and a few copper turnings, are quite sufficient. These are placed in a small glass tube filled with hydrogen, then exhausted by the air-pump, and finally hermetically sealed. This tube with the mixture is then exposed for half an hour to a gentle red heat. The tube is fixed in an apparatus which prevents its bursting by the pressure of the gas, which amounts to 8 or 10 atmospheres, or cracking on cooling. It is placed on cooling under mercury, and the contents emptied into a graduated receiver, when the relative proportion of carbonic acid and azote are determined in the usual way by potash. The following is the result of the experiment.

Gas volume at commencement	266·0 C. C.
Barometer	0·7358 metre.
Mercurial column above the level of the cistern	0·2850 ...
Temperature	13°·7 centig.
Tension of steam at this temperature . .	0·012 metre.
Volume after absorption of carbonic acid .	46·2 C. C.
Barometer	0·7358 metre.
Mercurial column above the level of the cistern	0·3558 ...
Temperature	18°·7 C.

The relation of the carbonic acid to the azote is therefore as 6 : 1·040.

The experiment was repeated as follows:—

Volume of gas at commencement	361·7
Barometer	0·7453

Mercurial column above the level of the cistern	0·2435
Temperature	20° C.
Tension of steam at this temperature	0·017
Volume of gas after absorption of the carbonic acid	68·6
Barometer	0·7444
Mercurial column above the level of the cistern	0·3688
Temperature	20° C.

The relation of the carbonic acid to the azote in this second experiment is therefore as 6 : 1·035.

A third experiment afforded the following results:—

Volume of gas at commencement	557·7
Barometer	0·7489
Mercurial column above the level of the cistern .	0·1637
Temperature	19°·5 C.
Tension of steam at this temperature	0·0165
Volume of gas after absorption of the carbonic acid	115·7
Barometer	0·7489
Mercurial column above the level of the cistern .	0·3471
Temperature	19°·5 C.

The relation in this experiment is therefore as 6 : 1·027.

0·368 grm. of the substance was heated with oxide of nickel to determine the arsenic, and 0·460 grm. of sulphuret of arsenic was obtained. 0·433 grm. of the same, heated with nitric acid, &c., yielded 1·7121 sulphate of barytes. These results collected, are as follows:—

		I.	II.	Calculated.
Carbon . . .	C_6	28·29	27·72	27·79
Hydrogen .	H_6	4·57	4·66	4·53
Azote	A	11·10	11·01	10·74
Arsenic . . .	As_2	56·04	56·81	56·96
		100·00	100·00	100·00

And this composition corresponds with the formula Kd Cy.

The specific gravity of the vapour of the cyanide of cacodyl can be estimated with greater ease than any of the other compounds, as it is much more volatile and more difficult to decompose. The following are the details of the experiment:—

Weight of the substance	0·1795 grm.
Volume of the vapour	53·11 C. C.
Temperature	152° C.
Barometer	324 lines.
Pressure to be deducted	29 ...

And the specific gravity, calculated from the above, is therefore 4·63. This theory requires 4·547, as will appear from the following:—

4 vols.	Carbon vapour	. . . .	3·371
12 ...	Hydrogen	. . . . . .	0·825
2 ...	Arsenic	. . . .	10·367
2 ...	Cyanogen	. . . . . .	3·638
			18·191 : 4 = 4·547

The specific gravity of the vapour of cacodyl itself may be deduced with some considerable probability from the relations in the condensation of the cyanide and oxide of cacodyl. There are one measure of oxygen and two measures of oxide of cacodyl, therefore the remaining one is that of the radical. If we argue from the relations in the condensations of inorganic compounds, this radical may be represented by one or two measures. In the first case, the elements would be combined without condensation, as in muriatic acid, &c.; in the other, three measures would be condensed into two, as in water. The specific gravity of the cyanide of cacodyl, and, in short, of the other haloid compounds, proves that the last supposition is the most correct. Four measures of the cyanogen compound contain two measures of cyanogen. On the supposition that the elements follow the same law of condensation in their organic as in their inorganic compounds, the radical must be combined without condensation, and consequently be represented by two measures; we have, therefore,

4 vols.	Carbon	. . . .	3·371
12 ...	Hydrogen	. . . . . .	0·825
2 ...	Arsenic	. . . .	10·367
			14·563 : 2 = 7·281

The same conclusion may be deduced from the relations in the condensation of the sulphuret and chloride of cacodyl.

5. *Protochloride of Cacodyl.*

This substance cannot be procured pure by simply distilling oxide of cacodyl with muriatic acid, for an oxy-chloride is formed at the same time, which cannot be entirely decomposed by distillation with an acid. The best and most simple plan of procuring it pure, is to distil a mixture of chloride of mercury and oxide of cacodyl, with highly concentrated muriatic acid; then dry the product thus obtained over chloride of calcium and caustic lime, without bringing it in contact with water at all; and, lastly, distil it in a tube hermetically sealed at both ends, previously filled with carbonic acid.

$$\left.\begin{array}{l} Kd\,O + Hg_2\,Cl_2 \\ H\,Cl \end{array}\right\} = \left\{\begin{array}{l} Kd\,Cl \\ H\,O \\ Hg_2\,Cl_2 \end{array}\right.$$

The substance prepared in this way is a perfectly limpid liquid, of an æthereal odour, does not solidify at − 45° C., becomes gaseous at a temperature a little above 100° C., and inflames spontaneously on contact with air. Heated alone it burns with a pale arsenious flame, and deposits either arsenic or arsenious acid, according to the quantity of air present. It explodes violently when heated in a confined atmosphere of oxygen. It deposits beautiful transparent crystals on slowly admitting the air. It inflames spontaneously in chlorine, and burns with the formation of a large quantity of carbon. It does not smoke in contact with air, but diffuses the most dreadfully pungent and suffocating odour, which far surpasses that of alcarsin. It produces a great irritation in the mucous membrane of the nose with inflammation, and causes the eyes to be suffused with blood when present in large quantities. I know no substance except acrolein which can be compared in this respect to chloride of cacodyl. It sinks in water without sensibly dissolving in that liquid, but imparts its penetrating odour.

It is also insoluble in æther, but dissolves in any proportion in alcohol. Weak nitric acid dissolves it without decomposition, but the concentrated acid causes it to inflame with an explosion. The chlorine is perfectly precipitated from this solution by means of a silver salt. Caustic lime and barytes do not extract any muriatic acid, nor is chloride of calcium formed by conducting its vapour over heated lime, before the temperature rises so high as to decompose the radical. An alcoholic solution of potash decomposes this substance, chloride of potassium and an æthereal volatile substance being formed. The latter does not contain chlorine, is easily soluble in water and alcohol, and has a peculiar penetrating odour ($C_4\ H_5\ As_2$?). This chloride is also converted into a saline mass in dry ammonia, which leaves salammoniac behind when heated with alcohol. The different products formed in these decompositions will be noticed hereafter.

The weak acids have no action on this substance, but sulphuric and phosphoric acids, on the contrary, disengage muriatic acid.

The analysis of this substance can only be accomplished by means of chromate of lead, as the combustion with oxide of copper is interrupted by sudden imperfect decompositions.

I. 0·7165 grm. of substance yielded 0·4620 grm. carbonic acid, and 0·282 grm. water.

II. 0·947 grm. of substance yielded 0·602 grm. carbonic acid, and 0·374 grm. water.

0·3405 grm. of this substance yielded 0·308 grm. chloride of silver, and 0·06 grm. was obtained in addition by burning the filter. If we allow the deficiency to be arsenic, we obtain the following results:—

		I.	II.	Calculated.
Carbon . .	C_4	17·83	17·57	17·32
Hydrogen	H_6	4·37	4·34	4·24
Arsenic . .	As_2	...	...	53·34
Chlorine .	Cl	22·90	...	25·10
				100·00

The small deficiency of chlorine arises from the presence of a small quantity of the chloride of oxide of cacodyl, which cannot be entirely separated.

The following are the details of the estimation of the vapour:

Substance employed	0·414 grm.
Volume	102·0 C. C.
Mercurial pressure of the oil column to be deducted . . .	7·6 lines.
Mercurial pressure to be deducted .	8·5 ...
Temperature	117° centig.
Barometer	332 lines.

The specific gravity calculated from the above is therefore 4·56, which corresponds with

4 vols.	Carbon	3·371
12 ...	Hydrogen	0·825
2 ...	Arsenic	10·365
2 ...	Chlorine	4·880
		19·441 : 4 = 4·86

The constituents are condensed in the same way as in the cyanogen compound, for one measure of the vapour of cacodyl is united with one measure of chlorine without condensation.

6. *Hydrous Chloride of Cacodyl.*

If muriatic acid gas, well dried by sulphuric acid and chloride of calcium, be conducted into pure oxide of cacodyl, so as to allow no air to enter at the same time, it will be absorbed with great violence and the evolution of much heat. The liquid divides into two layers, and a small quantity of a brick-red powder is deposited, which will be described in another place. A homogeneous liquid is obtained by placing the oxide in a cooling mixture, and continuing the absorption of the gas till no more takes place. A quantity of gas escapes from this liquid by simply stirring it with an angular substance. It also divides itself into the two layers by heating it in an atmosphere of carbonic acid so long as any gas makes its escape. The upper layer is very limpid, and presents all the characters of the chloride of cacodyl. The under layer is so viscous that it cannot be drawn up into a fine glass tube. It is clear that this under layer can be nothing else than hydrous chloride of cacodyl, as no other product is formed in this process. We know, from former observations, that chloride of cacodyl and water are formed by the action of the gas, and this latter body is not deposited, as it can be dissolved by the new substance formed, and therefore immediately unites with the chloride in the moment of its formation. This water is partially separated by distillation, and hence the formation of the two layers, which consist respectively of anhydrous and hydrous chloride of cacodyl, as they are only very slightly soluble in each other. This view is also supported by the fact, that chloride of calcium deliquesces in the viscous fluid and leaves pure chloride of cacodyl. I could not make an analysis of this interesting compound, in consequence of its being decomposed by distillation, without which it would contain traces of muriatic acid.

7. *Iodide of Cacodyl.*

If concentrated hydriodic acid be distilled with oxide of cacodyl, a yellow oily liquid collects in the receiver under the water, which on cooling deposits a yellow crust on the surface; and if this be allowed to proceed slowly, very fine, well-formed, and transparent rhombohedral plates make their appearance. The fluid portion is the iodide of cacodyl, and in order to separate this completely, the whole must be cooled down as low as possible in a freezing mixture. The fluid which remains must be once more distilled with concentrated hydriodic acid. The ice which is mixed with the other crystalline body also affords a quantity of this iodide, but it is not so pure. It must be allowed to stand some days in contact with quicklime and chloride of calcium in a glass tube hermetically sealed and filled with carbonic acid, to free it from water and the excess of hydriodic acid. It must then be distilled in the distillation tube filled with carbonic acid. The iodide obtained in this way is a limpid yellow fluid, and has a most disagreeable irritating smell, very similar to that of chloride of cacodyl. It possesses a very high specific gravity, for chloride of calcium swims on its surface; it remains fluid at $-10°$ C. Its boiling point appears to be considerably higher than 100°, and yet it distils over with the aqueous vapour. The gas is yellow coloured, similar to that of hypochlorous acid. It does not smoke in contact with the air, but deposits very beautiful prismatic crystals on long exposure. These crystals are oblique four-sided prisms, truncated opposite to the edges of the smaller side. This form is connected with that of alcargen, as far as I could ascertain it without any accurate measurement; and it might arise by lengthening the principal axis, and an increase of one terminal surface till the other disappeared. The iodide is soluble in æther and alcohol, but insoluble in water. Sulphuric and nitric acids decompose it, and disengage iodine. When it is heated in air it burns with a clear luminous flame, and the formation of fumes of iodine. It reacts with sublimate in the same way as the bodies already noticed.

The first analysis was made with chromate of lead.

I. 1·0095 grm. of substance yielded 0·373 grm. carbonic acid, and 0·235 grm. water.

A small quantity of iodine passed over into the chloride of calcium tube; the second analysis was therefore made with oxide of copper. A small trace of iodine was perceptible in the chloride of calcium.

II. 1·3590 grm. of substance gave 0·5215 grm. carbonic acid, and 0·3160 grm. water.

III. 1·1650 grm. of substance gave 0·4570 grm. carbonic acid, and 0·2735 grm. water.

The iodine was estimated in the following way:—1·284 grm. was dissolved in dilute alcohol and nitrate of silver added. The precipitate was gently warmed with nitric acid, and after heating to redness, weighed 1·317 iodide of silver.

1·201 grm. oxidized by means of oxide of zinc, yielded 0·8605 sulphuret of arsenic, of which 0·728 was oxidized by nitric acid, and produced 1·0142 sulphate of barytes, and 0·2683 grm. sulphur. The composition of this substance is, therefore,

	I.	II.	III.	Calculated.
Carbon . C_4	10·21	10·76	10·84	10·55
Hydrogen H_6	2·58	2·62	2·61	2·58
Arsenic . As_2	...	...	31·47	32·43
Iodine . . I	...	...	55·25	54·44
			100·17	100·00

The experiments which I made with the view of determining the specific gravity of its vapour did not afford any result, in consequence of this compound being partially decomposed even before it reaches the boiling point, and forming iodide of mercury. The relation in the condensation will, however, not differ from that of the chloride. By assuming the following formula, $C_4\ H_6\ As_2 + I = Kd\ I$, the following specific gravity may be calculated:—

4 vols.	vapour of Carbon . . .	3·371
12 ...	Hydrogen	0·825
2 ...	vapour of Arsenic . . .	10·367
2 ...	vapour of Iodine . . .	8·701
		23·264 : 4 = 5·816

8. *Bromide of Cacodyl.*

This compound may be obtained by distilling a mixture of concentrated hydrobromic acid with chloride of mercury and oxide of cacodyl. It forms a yellow liquid, which does not smoke, and agrees in all its characters most perfectly with the chloride. I have therefore not considered it necessary to make any analysis, as there can be no doubt that its composition is represented by the formula $C_4\ H_6\ As_2 + Br = Kd\ Br$. If it be heated with water, an oxibromide is formed, which smokes on exposure to the air.

9. *Fluoride of Cacodyl.*

This substance can be procured in a similar manner to the former. It is a colourless liquid, and possesses the most intolerably nauseous and penetrating odour. It is insoluble in water, but appears to undergo a similar decomposition to that of the bromide. It destroys glass, and can therefore only be procured in a pure state in platinum vessels. Its composition may with the greatest certainty be represented by $C_4 H_6 As_2 + F = Kd F$.

C. *Compounds of the Oxygen and Haloid Salts of Cacodyl.*

The substances to which our attention is directed in this division are very remarkable in many respects. They afford an example of compounds of organic oxides with haloid salts, which can be formed directly from their proximate constituents, and again be decomposed in the same way. Their formation corresponds with one of the most common phænomena attending decomposition in inorganic chemistry, but which I believe is quite new in organic. It is well known that the chlorine compounds of bismuth, tin, antimony, and many other metals, are partially decomposed by the action of water into muriatic acid and oxide, which last combines with the unchanged chloride, and forms what we call basic chlorides, or oxichlorides. Cacodyl corresponds most perfectly with these metals in this species of reaction. We find similar compounds formed under the same conditions. But these oxyhaloid compounds may, moreover, be formed directly from their proximate constituents. The oxide of cacodyl combines with its iodide and forms a crystallizable compound, in the same way that the oxide of mercury combines with its chloride.

Different views are taken of the constitution of these bodies. They are regarded as compounds of the oxide and haloid, or as oxides in which a portion of the oxygen has been replaced by an equivalent portion of the haloid. When we attempt to apply this latter view to the explanation of the cacodyl compounds, certain facts arise, which stand in direct contradiction with some views which are universally regarded as correct. There can therefore be no doubt as to the interpretation of these results, and we believe we have found an important ground for the opinion, that the true character of organic compounds is to be sought for less in the number and arrangement of the atoms, than in the opposite view which is founded on their peculiar nature. The investigation of the science in this field is extending, and it is here that this opposite view appears concealed from our perception, and hence it becomes the more important to publish those cases in which it is expressed with so much clearness, and which have been so full of promise in the development of organic chemistry.

The following results will bear out these observations:—

10. *Compound of Chloride of Mercury with Oxide of Cacodyl.*

This compound is formed by the action of sublimate on the oxide of cacodyl. A voluminous white precipitate is formed on mixing together a weak alcoholic solution of the latter with a weak solution of sublimate. It consists of a mixture of the oxide and chloride. The penetrating odour of the liquid entirely disappears. It may be rendered quite pure by pressure between the folds of blotting-paper, dissolving again in boiling water and three or four crystallizations. The preparation is even more easy by employing the liquid obtained by the slow oxidation of oxide of cacodyl in the air, and which is easily soluble in alcohol. It is necessary in both cases not to add an excess of the chloride, as the new compound is immediately decomposed by sublimate. It sometimes happens that nothing but protochloride of mercury is obtained by neglecting this precaution. All the compounds of cacodyl corresponding to the oxide are equally applicable in the preparation of this substance. The combustion of this substance is best conducted with oxide of copper in a long tube which contains chromate of lead in one end. As it is impossible to prevent a small quantity of chloride of mercury passing over into the chloride of calcium tube, it is necessary to blow two small bulbs on the long end of this tube, so that the chloride may be deposited in them before it arrives at the chloride of calcium. In the experiments made it was found to be wholly deposited in the first bulb, and its weight was very easily ascertained by cutting off this bulb after the combustion, &c. was finished, drying it to expel any moisture, and then weighing. The substance analysed was prepared from pure cyanide of cacodyl, three times crystallized, and dried at the ordinary temperature over sulphuric acid *in vacuo*.

1.	Substance employed in analysis . . .	2·162 grm.
	Water and chloride of mercury . . .	0·4235 ...
	Chloride of mercury	0·0805 ...
	Carbonic acid	0·4870 ...

2. Substance analysed	1·3245 grm.
Water and chloride of mercury	0·2140 ...
Chloride of mercury	0·0040 ...
Carbonic acid	0·3000 ...

I employed a method to determine the mercury, which has great advantages over that by the moist way, and as it is more accurate, deserves attention. 2·082 grm. of the substance with a mixture of lime and chlorate of potash, were poured into an ordinary combustion tube, which had the open end extending some inches out of the furnace, and at the other end a piece of chlorate of potash. The combustion was conducted with the same precautions as that in use in ordinary organic analyses. After the escape of gas had ceased, that end of the tube which contained the metallic mercury was cut off, dried and weighed, from which it appeared to contain 1·0593 grm. of mercury. The experiment was repeated with 1·142 grm. of the substance, and 0·579 grm. of mercury was obtained.

The amount of arsenic was determined in this way, viz. 0·8584 grm. of the substance was oxidized in the combustion tube by a mixture of two parts chlorate of potash and one part lime, and the mercury separated at the same time. The contents of the combustion tube were dissolved in muriatic acid, and yielded 0·3745 grm. of sulphuret of arsenic free from mercury. 0·327 grm. of this precipitate was then dissolved in fuming nitric acid, and left 0·004 grm. behind; the solution afforded 1·2953 grm. sulphate of barytes. This experiment gives 19·25 per cent. arsenic. As chloride of silver is not quite insoluble in a liquid containing mercury, I passed 2·082 grms. of the substance over heated lime, in the usual way, to separate the mercury. This lime was dissolved in acetic acid, and afforded 1·450 grm. chloride of silver, and 0·0528 grm. silver on the filter.

These results collected make the composition of the substance to be as follows:—

		I.	II.	Calculated.
Carbon	C_4	6·23	6·26	6·18
Hydrogen	H_6	1·76	1·76	1·55
Arsenic	As_2	19·25	...	19·43
Oxygen	O	3·94	...	2·06
Mercury	Hg_2	50·80	50·70	52·33
Chlorine	Cl_2	18·02	...	18·30
		100·00		100·00

The correctness of this assumed atomic constitution, as far as respects the carbon, hydrogen, arsenic, mercury, and chlorine, is placed beyond a doubt, by the correspondence between the numbers found experimentally and those calculated. There is not, however, the same certainty respecting the oxygen. We can assume 1, $1\frac{1}{2}$, and even 2 atoms, without passing the limits of experimental errors, which arises from the imperfect analytical means at our disposal. I do not therefore adopt the above formula in the following remarks as the only one which I conceive may be true, but because it most easily explains all the varied phænomena attending the decompositions. Some results which I have obtained in experimenting on the higher oxides of cacodyl, render it probable that this compound contains $1\frac{1}{2}$ atom of oxygen. Until I have finished the examination of the compound, which appears to afford the key to the solution of this question, I must hesitate in adopting this latter opinion, which presupposes the most extraordinary phænomena in the decompositions of organic chemistry. I do not however bind myself to the first supposition, although it is the ground-work of the following remarks, but expect to return to this subject in discussing the higher degrees of combination of cacodyl.

If we assume the empirical formula of this substance to be $C_4 H_6 As_2 O Hg_2 Cl_2$, the rational composition may be expressed in two ways. We can view it as a compound of a higher chloride of cacodyl with suboxide of mercury, viz. $Kd Cl_2 + Hg_2 O$, or as a compound of oxide of cacodyl and sublimate, $Kd O + 2 Hg Cl$. The following are the grounds which incline me to adopt this latter view.

If we mix a weak solution of this substance with less hydrate of potash than is necessary to precipitate the whole of the sublimate, yellow oxide of mercury is separated, which is converted in a few seconds into subchloride of mercury, while it acts as an oxidizing agent on the remaining oxide of cacodyl. This subchloride is afterwards decomposed, on adding a larger quantity of hydrate of potash, into suboxide of mercury, which again oxidizes the free oxide of cacodyl, and undergoes itself a partial reduction. Hydrate of potash therefore first produces a chloride of mercury, and the black precipitate which is sometimes observed must now be regarded as the result of a later decomposition.

When hydriodic acid is poured over this compound, red iodide of mercury is immediately formed, which dissolves again in the excess of acid with the separation of some yellow oily drops. This oily substance passes off on distillation, and has all the characters of iodide of cacodyl. Hydriodate of mercury remains behind in the retort.

$$\left.\begin{matrix}\text{Kd O} + 2\,\text{Hg Cl} \\ \text{H I}\end{matrix}\right\} = \left\{\begin{matrix}\text{Kd I} \\ \text{Hg}_2\,\text{Cl}_2 + \text{H O}\end{matrix}\right.$$

Muriatic acid reacts in precisely the same way. Chloride of cacodyl and sublimate are formed.

$$\left.\begin{matrix}\text{Kd O} + \text{Hg}_2\,\text{Cl}_2 \\ \text{H Cl}\end{matrix}\right\} = \left\{\begin{matrix}\text{Kd Cl} \\ \text{Hg}_2\,\text{Cl}_2 \\ \text{H O}\end{matrix}\right.$$

and the other hydrogen acids all have the same reaction.

The stronger oxygen acids, as phosphoric acid, scarcely decompose this compound. It is true that the water which passes over on distillation has the odour of chloride of cacodyl, but it is present only in the most minute quantities. This reaction is scarcely reconcileable with the existence of suboxide of mercury in this compound.

When phosphorous acid is distilled with this compound, chloride of cacodyl is formed, and subchloride of mercury separated, while the acid reduces the oxide of cacodyl, whose radical combines with one half of the chlorine of the sublimate.

$$\left.\begin{matrix}2\,(\text{Kd O} + \text{Hg}_2\,\text{Cl}_2) \\ \text{P}_2\,\text{O}_3\end{matrix}\right\} = \left\{\begin{matrix}2\,\text{Kd Cl} \\ 2\,\text{Hg}_2\,\text{Cl} \\ \text{P}_2\,\text{O}_5\end{matrix}\right.$$

A larger quantity of phosphorous acid completely reduces the mercury.

Metallic tin, mercury, and all those bodies which reduce the sublimate, react in the same way.

Chloride of gold, and the easily reducible metallic oxides, are reduced by "the oxide of cacodyl with chloride of mercury," as by the free oxide, while muriatic and cacodylic acids are formed.

$$\left.\begin{matrix}\text{Au}_2\,\text{Cl}_3 \\ 3\,\text{H O} \\ \text{Kd O}\end{matrix}\right\} = \left\{\begin{matrix}\text{Au}_2 \\ 3\,\text{H Cl} \\ \text{Kd O}_4\end{matrix}\right.$$

The decomposition which this substance undergoes when boiled by itself and with an excess of chloride of mercury, depends upon the same circumstance. Subchloride of mercury falls to the bottom, chloride of cacodyl passes off with the aqueous vapour, and cacodylic acid remains in solution.

$$4\,(\mathrm{Kd\,O} + \mathrm{Hg_2\,Cl_2}) = \begin{cases} 3\,\mathrm{Kd\,Cl} \\ \mathrm{Kd\,O_4} \\ 3\,\mathrm{Hg_2\,Cl} \\ \mathrm{Hg_2\,Cl} \end{cases}$$

If the formation of oxide of cacodyl with chloride of mercury does not now present any difficulties, it must however appear most extraordinary, when we perceive the oxide of cacodyl remain unchanged in contact with one of the most powerful oxidizing agents we possess, and much more when we call to mind the strong affinity of the oxide for oxygen, which rises to such intensity in oxygen gas, that it becomes heated till it inflames. This fact would indeed be unfavourable to the view taken above with respect to the amount of oxygen, did not the formation of this substance, on the other hand, from a liquid obtained by direct oxidation of the oxide of cacodyl in the air, lead to another explanation which meets the difficulty.

The substance on which the formation of the mercurial compound depends in this mixture of oxidized products, is a liquid which nearly corresponds with alcarsin in its composition, but widely differs from that body in its characters, for it neither smokes in contact with the air nor inflames, and undergoes oxidation very slowly.

The experiments which I have hitherto made on this substance, do not decide whether it is a compound isomeric with oxide of cacodyl, or a higher degree of oxidation, answering to the formula $\mathrm{Kd_2\,O_3}$. If the first supposition be true, we see that the formation of the mercurial compound arises from the difficulty of the oxidation of this modification. But should the latter view, on the contrary, prove correct, and the mercurial compound contain the higher degree of oxidation instead of the lower, not only would its formation agree with one of the most common reactions, but its relation to oxidizing and reducing agents, as well as to the hydrogen acids, would be capable of the most natural solution. In this way, this higher degree of oxidation would be decomposed by the action of the hydrogen acids into oxide of cacodyl and cacodylic acid, in precisely the same way as the inorganic peroxides, and the former would afterwards further undergo its own peculiar decompositions. I hope shortly to be able, from some experiments now in progress, to decide this question, and shall return to the subject in the second part of this memoir, when I propose discussing the higher compounds of cacodyl.

The chloride of mercury with oxide of cacodyl forms a fine white crystalline powder when obtained in the form of a precipitate. It is deposited in the form of groups of large and very soft crystals when its aqueous solution is slowly cooled. They assume the form of small rhombic plates with angles of about 60° and 120°, but the crystals are too small to be accurately measured.

100 parts of boiling water dissolve 3·47 parts, and water at 18° C. only takes up 0·21 parts.

Alcohol also dissolves it, and in larger quantity when warm than cold. It is quite inodorous; but if the smallest quantity reaches the nose, a continuous, insupportable smell is felt. The taste is disagreeably metallic, and produces nausea. It is very poisonous in larger quantities. It is easily decomposed by heat, and diffuses itself in the air without any residue. When heated in close vessels it emits an odious vapour, a sublimate containing chloride of mercury, calomel, and erytrarsin, and leaves a light porous charcoal behind, which burns with an arsenious odour without any residue.

11. *Compound of Bromide of Mercury with Oxide of Cacodyl.*

This substance agrees so completely with the corresponding chlorine compound, that I have not thought it necessary to make any analysis. It also is formed by adding bromide of mercury to oxide of cacodyl, or to the mixed oxidized product noticed before, and may be rendered quite pure by crystallization. It forms a white crystalline powder, which has a slight shade of yellow, and does not appear so capable of being crystallized from its aqueous solution as the chlorine compound.

It has nearly the same degree of solubility as the chlorine compound, is also inodorous, has the same disagreeable metallic taste, and is easily decomposed by boiling the aqueous solution. When it is heated in close vessels, it melts before it is decomposed. It undergoes decomposition when strongly heated; pro- and perbromide of mercury sublime, a disagreeable liquid containing bromine distils, and charcoal is left behind. It burns in contact with the air without leaving any residue. Its other relations and decompositions are precisely the same as those described in the preceding section.

12. *Basic Chloride of Cacodyl.*

This substance is obtained by heating the protochloride with water, or still easier by distilling oxide of cacodyl with hydrous muriatic acid. The following experiments were made upon the substance prepared by distilling 40 grms. oxide of cacodyl with dilute acid, and then distilling the product in a close vessel with water and lime powder. This was then dried over chloride of calcium, and once more distilled in the distillation tube.

Analysed by means of chromate of lead, the following results were obtained:—

I. 0·8682 grm. of substance gave 0·553 grm. carbonic acid, and 0·3351 grm. water.

II. 0·7041 grm. of substance gave 0·4377 grm. carbonic acid, and 0·2688 grm. water.

0·6481 grm. burnt with oxide of nickel yielded 0·8005 grm. sulphuret of arsenic. 0·748 grm. of the same yielded again 2·602 grm. sulphate of barytes, and 0·055 grm. melted sulphur.

0·5419 grm. boiled with nitric acid and nitrate of silver yielded 0·3997 grm. chloride of silver, and 0·04 grm. metallic silver from the filter.

0·5298 grm. conducted over heated lime in a combustion tube yielded 0·407 grm. chloride of silver, and 0·007 silver. The lime which was used contained 0·0027 grm. chlorine, as I determined by a preliminary examination.

In order to set aside all doubt as to the composition, I made this substance by two other methods. I will call the first *a*, which was prepared by distilling the oxide of cacodyl twice with concentrated muriatic acid; and the other *b*, which was three times distilled with the acid. The following are the results obtained by analysing these products:—

III. 0·679 grm. of substance *a* gave 0·4387 grm. carbonic acid, and 0·2642 grm. water.

1·3945 grm. of *a*, oxidized by nitric acid, afforded 1·0247 chloride of silver, and 0·006 grm. metallic silver.

0·7811 grm. of *b*, heated in the same way, produced 0·578 grm. chloride of silver, and 0·0098 grm. metallic silver.

From these experiments, of which I consider Nos. I. and III. the most accurate, the following composition is deduced:—

	I.	II.	III.	Mean of I. and III.	
Carbon .	17·62	17·12	17·86	...	17·74
Hydrogen	4·29	4·24	4·32	...	4·31
Arsenic .	55·15	...	...	...	55·15
Chlorine .	18·88	18·43	18·69	18·34	18·78
Oxygen .	4·10	...	...	...	4·02
	100·00				100·00

The atomic relation of the carbon, hydrogen, arsenic, and chlorine in the above composition, is exactly $C_4\ H_6\ As_2\ Cl_{\frac{3}{4}}$. We cannot determine from the analysis how much of the 4 per cent. oxygen really belongs to the substance itself, as it is impossible to avoid a partial oxidation even with the greatest care; and, moreover, the whole of the errors of experiment always fall upon this element. That the whole amount of oxygen does not much exceed $1\frac{1}{2}$ per cent., may be assumed on the supposition that all the other estimations being reconcilable with a possible theoretical composition, it must be an approximation. Assuming then an impurity of 2 per cent. of oxygen, we obtain the following results:—

		Found.	Calculated.
Carbon . . .	C_4	18·21	18·22
Hydrogen . .	H_6	4·43	4·46
Arsenic . . .	As_2	56·61	56·04
Chlorine . . .	$Cl_{\frac{3}{4}}$	19·28	19·79
Oxygen . . .	$O_{\frac{1}{4}}$	1·47	1·47
		100·00	100·00

We must therefore regard this substance as a compound of 1 at. oxide and 3 at. of the chloride, viz.

$$(C_4\,H_6\,As_2 + O) + 3\,(C_4\,H_6\,As_2 + Cl) = Kd\,O + 3\,Kd\,Cl.$$

The formation of the substance is easily explained; the chloride of cacodyl is decomposed along with water into muriatic acid and this basic chloride.

$$\left.\begin{matrix} 4\,Kd\,Cl \\ H\,O \end{matrix}\right\} = \left\{\begin{matrix} Kd\,O + 3\,Kd\,Cl \\ Cl\,H \end{matrix}\right.$$

The following are the details of the experiment to estimate the specific gravity of its vapour:—

Substance employed	0·247 grm.
Volume of the vapour	69·78 C. C.
Temperature	164° centig.

Barometer	. . . .	328 lines.
The correction of the oil receiver	.	57·8 ...

The specific gravity calculated from these data is 5·46, and it corresponds, as nearly as the nature of the experiment allows, with the supposition that the oxide and chloride have combined without condensation, viz.

3 vols. Chloride of cacodyl	. .	13·58
1 vol. Oxide of cacodyl	. . .	7·83

$$\frac{21{\cdot}41}{4} = 5{\cdot}35$$

The characters of this basic compound correspond in general so closely with those of the neutral chloride, that I cannot do better than refer back to that section. It differs from the neutral compound in emitting a less disagreeable odour, and white vapours in contact with the air. It boils at about 109° C.

13. *Basic Bromide of Cacodyl.*

This substance also agrees very closely with the corresponding chlorine compound, and is obtained in a similar manner. It smokes in contact with the air, is yellow coloured, but becomes colourless on being heated, and on cooling again turns yellow. It evinces a most remarkable reaction on being heated with metallic mercury, which I could not unfortunately investigate, from want of a sufficient quantity of the material. The change it undergoes is not attended with any evolution of gas, but it is converted into a solid substance of a citron yellow colour, which is easily fusible. This substance can be converted into vapour without decomposition, and when boiled with water is changed into mercury and a fuming substance that passes off with the vapour. It is decomposed by a strong heat into mercury, subbromide of mercury, and a very disagreeable volatile body containing arsenic. As it agrees very closely in its other characters with the chloride, I shall confine myself to giving its analysis. The substance analysed was prepared by twice distilling a mixture of oxide of cacodyl and concentrated hydrobromic acid, and purifying it in the same way as the chloride. In conducting the analysis with chromate of lead, the fore part of the combustion tube was filled with fine copper turnings, which prevented the bromide of copper passing over with the water.

I. 0·915 grm. of substance yielded 0·292 grm. water, and 0·475 grm. carbonic acid.

II. 0·999 grm. of substance yielded 0·3305 grm. water, and 0·5375 grm. carbonic acid.

0·636 grm. bromide of silver, and 0·026 grm. of silver on the filter, were obtained by dissolving 0·8265 grm. of this substance in alcohol, precipitating by nitrate of silver, and heating with a little nitric acid.

0·631 grm. sulphuret of arsenic was obtained by oxidizing 0·5988 grm. with oxide of zinc, dissolving the same in nitric acid, and removing the nitric acid by means of the sulphuric. 0·590 grm. of this sulphuret, after oxidation, yielded 0·8882 grm. sulphate of barytes, and 0·2148 grm. sulphur.

These experiments give the following numbers:—

		I.	II.	Calculated.
Carbon . .	C_4	14·35	14·84	14·70
Hydrogen .	H_6	3·55	3·67	3·60
Arsenic . .	As_2	45·15	...	45·21
Bromine . .	$Br_{\frac{3}{4}}$	34·60	...	35·29
Oxygen . .	$O_{\frac{1}{4}}$	2·35	...	1·20
		100·00		100·00

Whence it follows that this substance is represented by a formula exactly corresponding to that of the basic chloride, viz.

$$(C_4 H_6 As_2 + O) + 3(C_4 H_6 As_2 + Br) = Kd\,O + 3\,Kd\,Br.$$

14. *Basic Iodide of Cacodyl.*

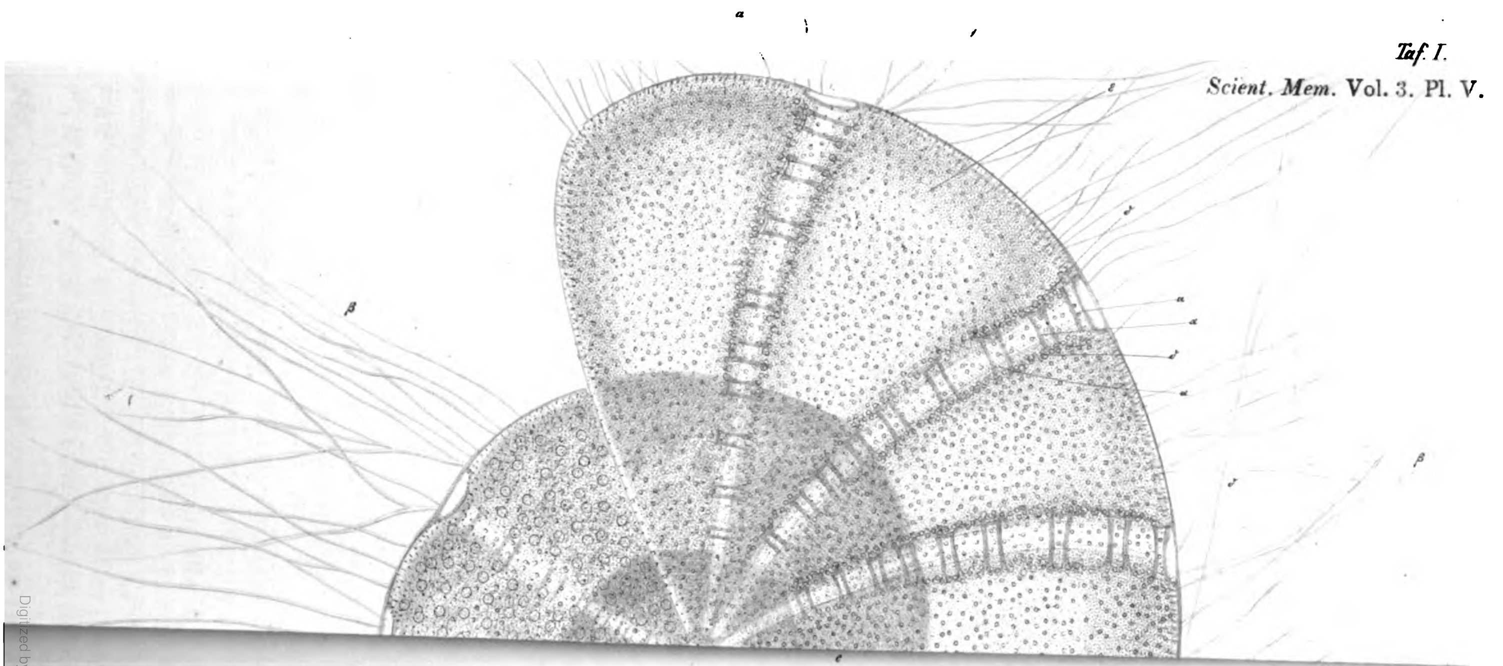
This basic iodide is formed at the same time as the iodide, on distilling the oxide of cacodyl and hydriodic acid. It is deposited from the neutral compound in the form of yellow crystalline crusts. It may be nearly freed from the fluid iodide by pressing these crusts between folds of blotting-paper under water containing no atmospheric air. It must then be dissolved in absolute alcohol, from which it can be obtained in fine large crystals by cooling the solution. The alcohol which is retained by the crystals can be removed by pressure in blotting-paper under water. By allowing it to remain in contact with chloride of calcium in a fluid state for some days, it is freed from the water, and must lastly be distilled in the distillation tube till about half has passed over. It is impossible to obtain this substance free from some oxide, as it possesses as great an affinity for oxygen as the oxide of cacodyl itself. It could not even be accomplished by pressing the substance between folds of blotting-paper covered with tinfoil, in the case of an hydraulic press which was surrounded with carbonic acid. It becomes heated by the oxidation on removing the paper out of the apparatus previous to immersing it in the water, and melts, when it is absorbed by the paper. I have in vain endeavoured to obtain it in a state of purity fit for analysis, in consequence of its great affinity for oxygen. There can however be no doubt as to its composition, from its character and the mode of its formation. Besides the neutral and basic compounds, there is no other substance formed on distilling the oxide of cacodyl with hydriodic acid. The substance in question therefore can only be composed of iodide and oxide of cacodyl and water, or hydriodic acid. The latter cannot be true, for protiodide of cacodyl remains unaltered by digestion with hydriodic acid. But it is, on the contrary, very easy to form it by mixing the protiodide with the oxide. These two bodies are miscible in any proportion with each other without undergoing any alteration. The moment however that a drop of water is added, the whole becomes a mass of yellow crystals, which have all the characters of the substance obtained by distilling the oxide with hydriodic acid, and which form the same large crystals when covered with a layer of water as we observe in the other case. And further, when we find that the same products are formed by oxidation in the air in both cases, there cannot be a doubt as to its composition. We cannot however assume that there is the same atomic relation among the constituents, which is also proved by the difference in the characters.

It forms a crystalline mass of a yellow colour, which crystallizes from its solutions in fine transparent rhombic plates. It is only slightly soluble in water, but easily so in alcohol. It melts considerably below the boiling point of water, and can be distilled without decomposition. It emits thick white vapours in the air, becomes heated so as to melt, and sometimes inflames. It burns with a sooty flame with the emission of iodine vapour.

If these compounds of organic oxides with haloid salts possess some interest in supporting the view which might be suggested regarding the nature of organic compounds, it is of great importance to determine accurately the peculiarities by which these substances are distinguishable from the corresponding ones in inorganic chemistry. I think therefore that the fact that this oxiodide cannot be reconverted into the iodide by digestion or distillation with hydroiodic acid, is one of these peculiarities. The powerful affinity existing between an oxide and haloid compound of the same organic radical, which this example presents, appears to me peculiarly adapted to support the views which Berzelius has published with regard to the constitution of those bodies obtained from alcohol by the substitution of chlorine, and suggests the possibility of forming these substances by the direct combination of their proximate constituents instead of the indirect method of substitution.

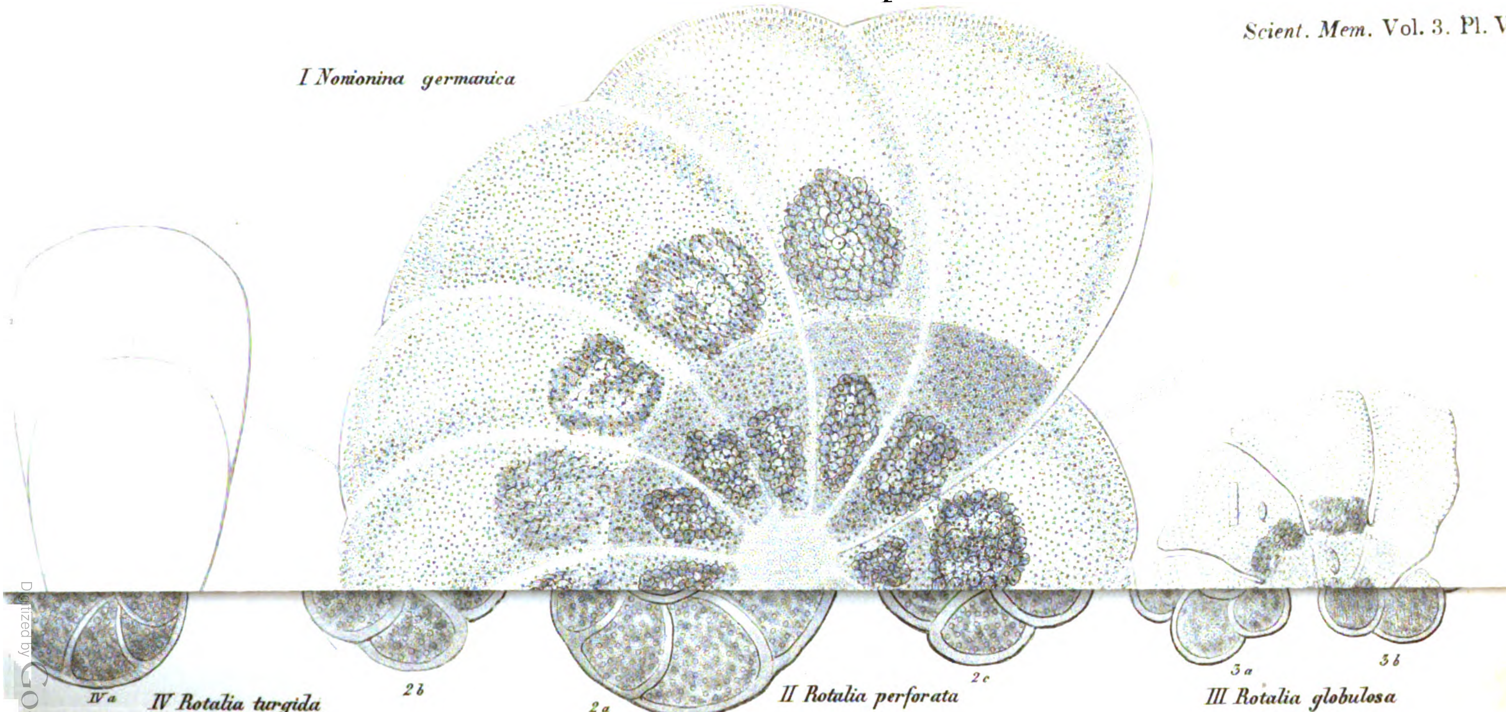
I shall close this division of my subject by collecting below, in a tabular form, all those lower degrees of combination of cacodyl which have been examined, and propose in the next to treat of the higher degrees of combination which possess so much interest in connexion with the former.

$C_4 H_6 As_2$	$= Kd$	Cacodyl.
$C_4 H_6 As_2 O$	$= Kd\,O$	Oxide of cacodyl.
$C_4 H_6 As_2 S$	$= Kd\,S$	Sulphuret of cacodyl.
$C_4 H_6 As_2 Se$	$= Kd\,Se$	Seleniet of cacodyl.
$C_4 H_6 As_2 Cy$	$= Kd\,Cy$	Cyanide of cacodyl.
$C_4 H_6 As_2 Cl$	$= Kd\,Cl$	Chloride of cacodyl.
$C_4 H_7 As_2 Cl\,O$	$= Kd\,Cl + H\,O$	Hydrous chloride of cacodyl.
$C_4 H_6 As_2 I$	$= Kd\,I$	Iodide of cacodyl.
$C_4 H_6 As_2 Br$	$= Kd\,Br$	Bromide of cacodyl.
$C_4 H_6 As_2 F$	$= Kd\,F$	Fluoride of cacodyl.
$C_4 H_6 As_2 O\,Hg_2 Cl_2$	$= Kd\,O + 2\,Hg\,Cl$	Oxide of cacodyl with Chloride of mercury.
$C_4 H_6 As_2 O\,Hg_2 Br_2$	$= Kd\,O + 2\,Hg Br$	Oxide of cacodyl with Bromide of mercury.
$C_4 H_6 As_2 Cl_{\frac{3}{4}} O_{\frac{1}{4}}$	$= Kd\,O + 3\,Kd\,Cl$	Basic chloride of cacodyl.
$C_4 H_6 As_2 Br_{\frac{3}{4}} O_{\frac{1}{4}}$	$= Kd\,O + 3\,Kd\,Br$	Basic bromide of cacodyl.
$C_4 H_6 As_2 I_{\frac{3}{4}} O_{\frac{1}{4}}$	$= Kd\,O + 3\,Kd\,I$?	Basic iodide of cacodyl.



Organisation lebender Polythalamien.

gez. v. Ehrenberg.



Jetzt lebende und denen der Kreideformation gleiche Polythalamien.

gez. v. Ehrenberg. gest. v. C. F. Weber.

Taf. III.

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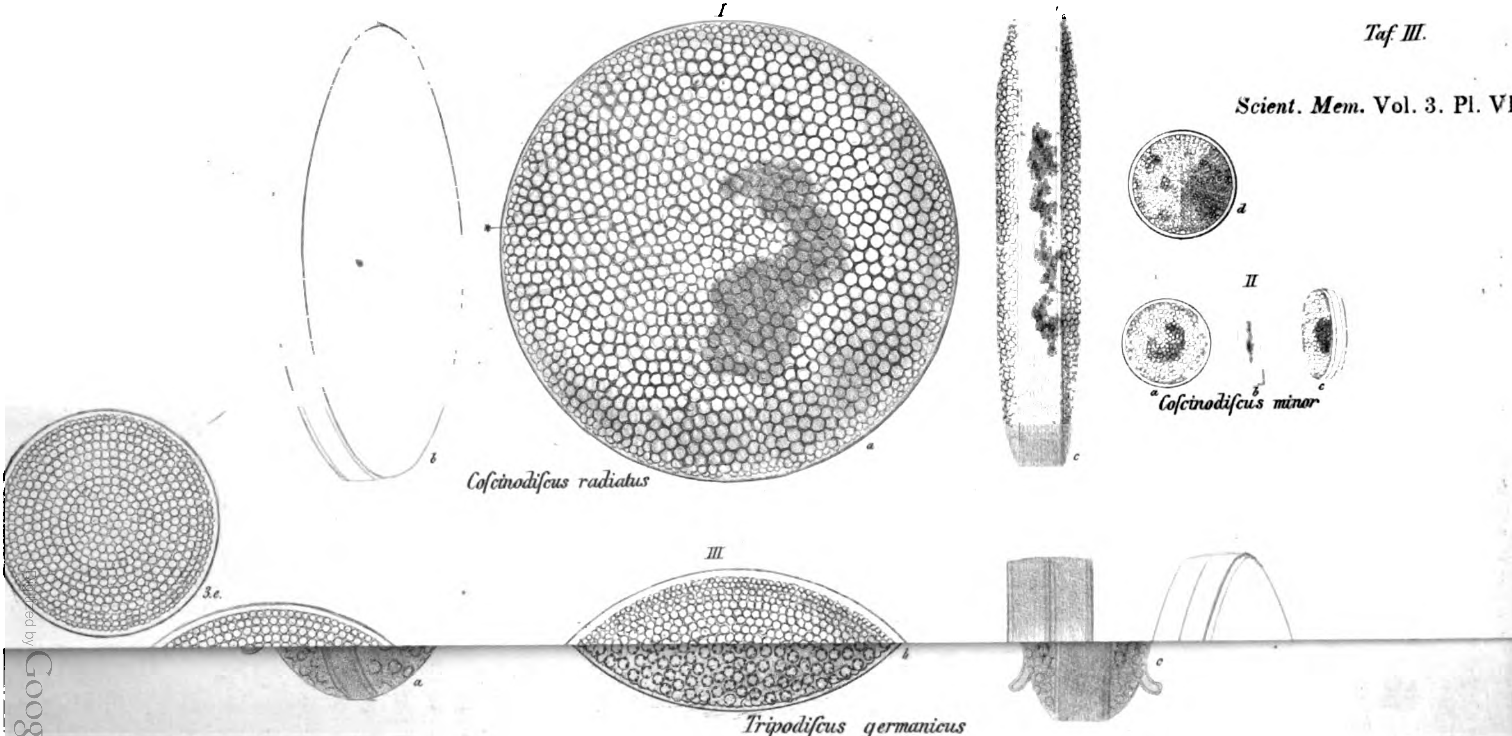
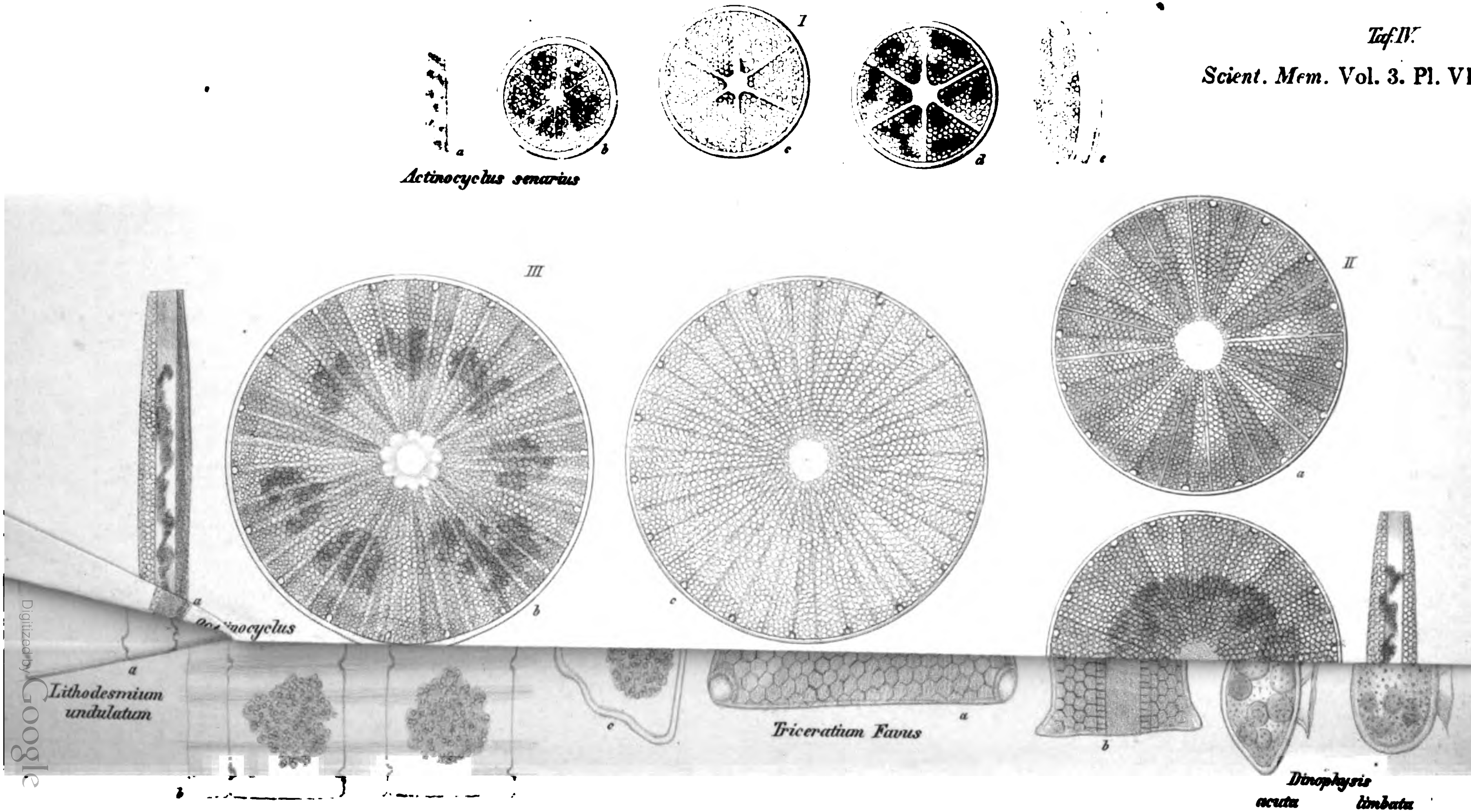


Jeztlebende Kieselthierchen der Kreidemergel und der Nordsee.

gez. v. Ehrenberg

Jetztlebende Kieselthierchen der Kreidemergel und der Nordsee



Jetztlebende Kieselthierchen der Kreidemergel und der Nordsee.

gez. v. Ehrenberg. gest. v. C. K. Weber

Article XIII.

On numerous Animals of the Chalk Formation which are still to be found in a living state. By Dr. C. G. Ehrenberg*.

[Read in the Royal Academy of Sciences of Berlin on the 17th and 31st of October, 1839, and 16th of January, 1840, with subsequent additions.]

Introduction.

THE most careful researches of modern times have uniformly tended to confirm the opinion, that only in the upper and most recent molasse and tertiary strata of the earth's crust fossil remains occur, which are identical not merely with genera, but even species of existing organisms; and that all the organic forms the remains of which are met with in the subjacent chalk of the secondary formation, as well as those of the still lower oolitic and transition formations, are entirely distinct from species now living. Upon this result of observation have been built theories of the development of organized nature; and the present organic world, including man, has been characterized as an entirely secondary one, all the fundamental types of which lay preserved piecemeal in the lower and earlier strata of the earth; nay, it has been frequently asserted, in the most recent physical descriptions of the earth, as a result of the science of palæontology, that the now living organisms, together with man, are the further periodical development and improvement of forms, which being situated lowest in the earth's mass, are therefore naturally and necessarily nowhere at present found in a living state.

George Cuvier's comprehensive physiological researches of 1795†, which in 1806 were published at length in the *Annales du Museum*, concisely separated the Vertebrata of the antediluvian from those of the present world. Leopold von Buch and Deshayes have subsequently verified, in numerous forms of

* From the Transactions of the Royal Academy of Berlin for 1840. Published also separately in folio by Voss, of Leipzig.—Translated from the German by William Francis, Ph.D., A.L.S.

† The younger Camper likewise expressly states in the splendid posthumous work of his father, Peter Camper, on the 'Anatomy of the Elephant,' 1802, that the merit of the brilliant discoveries relative to the distinction between fossil and recent animals had belonged to Cuvier ever since 1795; and Blumenbach's first denomination of the fossil elephant, as *Elephas primigenius*, although proper to be retained, is of a later date than Cuvier's far more comprehensive distinction.

Conchylia, the same phænomenon. With equal scientific accuracy, also, have the recent researches of Milne Edwards on the genus *Eschara* led to the result, that not a single one of the numerous fossil species (27) of the oolite and chalk formation agrees with recent species; and the latest extensive and profound inquiries of Agassiz on fossil Fish and Echinites, have yielded a similar result*.

Deshayes and Lyell subsequently collected these facts into systematic order; and the latter ingenious and experienced English geologist laid it down as a fundamental position, that according to the most accurate researches, neither in the transition and oolite formation, nor in the chalk of the secondary formation, did remains exist of recent species; but that these first present themselves with the newer tertiary epoch of the earth's formation. He therefore divided the tertiary epoch into four periods or times of deposition:

the Eocene,

or earliest dawning period of the present organic world, in whose strata very few recent species only have hitherto been found;

the Miocene,

in which less than the half;

the Older Pliocene,

in which more than the half; and

the Newer Pliocene,

in which almost all the fossil remains agree with species† still living.

This view has met with a very extensive reception, and has been echoed from many quarters; and the more so, from its adoption by Dr. Buckland in his widely diffused 'Bridgewater Treatise,' 2nd edit. 1837. i. pp. 78, 79. It has acquired greater claims to favour, on account of the opinion having been also promulgated from other quarters, that our earth has been exposed at different periods to highly varied relations of temperature; and, consequently, also to varied states of combina-

* The only species of fish found both recent and fossil mentioned by Agassiz is *Mallotus villosus* of Cuvier. Alluding to the figure of it in his *Recherches sur les Poissons Fossilles*, Agassiz says, "Les exemplaires contenus dans les géodes marneuses que l'on trouve sur les côtes d'Islande, sont identiques avec les exemplaires vivans dans ces parages. On dit qu'il se forme encore tous les jours de ces géodes. C'est le seul exemple que je connaisse de fossiles identiques avec une espece vivante."—*Rech. sur les Poiss. Foss. feuilleton additionnel*, p. 116. 1838.—Ed.

† Lyell's Geology, 4th edit. vol. iii. p. 308. German translation by Hartmann, vol. iii. p. 42.

tion of the atmosphere and waters, in accordance with which the organic inhabitants have also been modified, and upon which their extinction or their thorough transformation, in such manner as we find them successively in the crust of the earth, has been dependent. The recent organisms could not have lived, such is the belief, under the former relations of the earth; and the earlier beings must have died out or have become essentially modified in order to continue to exist in the altered external conditions. But the most remarkable commencement of an entirely new period of organic beings was to all appearance manifest between the secondary and tertiary formations; since the whole series of the earlier organisms up to the chalk or secondary formation still linked into one another, and contained, among the more than thousands of their species, not a single generally-acknowledged recent species; but beginning with the tertiary formation, or the beds immediately above the chalk, the species were with few exceptions different and mixed with some recent ones, so that these, few and scattered in the lower, became constantly more numerous in the upper strata, as if the day of the life surrounding us, and of our own existence, had only begun its dawn above the chalk formation.

Such being the present state of the science, it seemed advantageous to communicate to the Academy various well-ascertained facts, leading to an opposite conclusion, relative to numerous still actually living animals of the chalk, the number of which I have succeeded in increasing considerably since the summer vacation, and which form a further continuation of some researches read before the Academy at the commencement of the year (1839).

I. *First traces of recent Animals of the Chalk.*

An accurate distinction of the fossil and recent minute animal forms has been effected only of late; the opinions of earlier writers, therefore, possess no scientific value. Nevertheless, an opinion has recently been here and there entertained, that some species of still existing animals were to be found in the chalk. Thus Soyer Willemet thought he had found alive, fixt upon Corsican marine Algæ, the *Spirolina cylindracea*, Lam, which Alcide d'Orbigny in 1826 placed among the Foraminifera, a Polythalamian form, said to be from the Paris chalk, but which is imbedded there in the sand and calcaire grossier*. In the first

* *Grobkalk*; equivalent of the London clay.—Ed.

place the form has been nowhere observed with certainty in the chalk, which is likewise the opinion of Bronn in the *Lethæa Geognostica*, ii. p. 1135, 1838; and, secondly, it is uncertain whether the so-called living form was not merely a particle of sea-sand as old as the arenaceous beds of the calcaire grossier of Paris, since the observer has recognised and described none of the characters of the living animal, and wherever chalk cliffs are situated near the sea, shells of chalk animals are found detached in the sand. Nothing can therefore be concluded from this form, especially as the living animal from the Red Sea, which, under the name of *Coscinospira Hemprichii*, I have placed in quite a different family of Polythalamia*, likewise very much resembles externally the Paris fossil species.

A far more important investigation of recent animals occurring in the chalk was published by Leopold von Buch in 1834, to which, however, attention has not hitherto been paid in geological compilations. It relates to some species of the genus *Terebratula*. M. von Buch, in his critical classification of this genus of shells so abundant in forms and of such geological importance, published in 1835 in the Transactions of the Academy for the year 1833, p. 45†, observes, "Nevertheless there is no (living) species of this section (Loricata) which could be considered as perfectly identical with fossil species; moreover, this perfect identity is limited, even to the present day, to but very few species, perhaps merely to two or three. *Terebratula vitrea* is not rare in the chalk; and *Ter. striatula*, of Mantell and Sowerby, which occurs in the chalk and upper Jura beds, differs but little from the well-known *Ter. caput serpentis*." In this representation, it is positively asserted of *Ter. vitrea* only that it is a living form of chalk animals; and he who sees and examines the specimen upon which this conclusion is based, as I have done, will be able to pronounce no other opinion. Now, although an identity has been here recognised and can be safely asserted, yet there exists an element of embarrassment as regards important conclusions in the great variety of form in the species of this genus, which, from a chaos of names hitherto destitute of any certain limitation, had, by that difficult but important

* 'On the formation of the chalk and the chalk marl out of invisible organisms;' in the Transactions of the Berlin Academy for 1838, published in 1839, p. 120. pl. i., and p. 131. pl. ii. fig. 2.

† In the separate edition on *Terebratulæ*, which was published in 1834, it is p. 25.

labour, been reduced to a general classification and definition of forms welcome alike to zoology and to geology. Among the very numerous, clearly defined, and frequently large forms of the genus, it is only some smaller, delicate, and less sculptured ones which stand in connection with living species.

Moreover, in 1836, Desmoulins and Grateloup* determined several species of Echinidæ in the (anomalous) chalk of Dax as being identical with now living species, specimens of which were in Desmoulins' collection. Agassiz, the most ingenious and latest systematic observer of the Echinidæ, has expressed himself decidedly against these observations. The latter asserts, that there are neither fossil fish nor fossil Echinidæ known to him that really belong to still existing species, and he published this declaration expressly with a view to these observations of Dax, 1838†. Bronn only remarks in answer to this, that nevertheless the identity of the species is not inconsistent with the history of the development of the earth; but he himself has not published any further confirmations or observations.

In December 1838, I made known to the Academy‡ that there had been found from five to six species of Infusoria in the chalk, which appeared so similar to the still existing forms of the present world, that they did not present any character clearly distinct; consequently it was not possible to give them any different names. They are,—

Eunotia Zebra,
Fragilaria rhabdosoma,
Fragilaria striolata?
Gallionella aurichalcea,
Navicula ventricosa,
Synedra Ulna.

The *Synedra Ulna* I had already recognised and specified, in 1837, in the Polirschiefer, or Chalk-marl of Oran; there, also, was found the more distinct *Navicula ventricosa*. *Eunotia Zebra* was from a Greek marl, in which, at the same time, calcareous-shelled Polythalamia of the white chalk appeared. The *Gallionella aurichalcea* was from the decided white chalk of Rügen. Both the *Fragilariæ* were from the decided white chalk of Gravesend.

* Memoir on the Echinides of Dax, 1836–38. See Bronn's *Lethæa*, ii. p. 771, 1838.

† Bronn's *Lethæa Geogn.*, ii. p. 771, 1838.

‡ Transactions of the Academy of the year 1838, pp. 85, 91 *seq.*, and p. 149, in the tabular conspectus at the end. Separate edition: on the formation of the chalk, &c., pp. 29, 35, and at the end.

I further enumerated at that time, among the widely-diffused calcareous-shelled animals of the chalk, *Globigerina bullacea* and *helicina, Rosalina globularis,* and *Textilaria aciculata,* with a note of interrogation, all which Polythalamic forms had been announced by D'Orbigny as at present living only in the Adriatic sea, or in the ocean.

Although even at that time this greater number of forms entirely similar to recent ones and not to be distinguished from them very much excited my attention, still it appeared to me best merely to mention them *en passant,* for this reason, that the same difficulty in pronouncing an opinion existed with regard to these forms that attended the *Terebratulæ,* viz. that they were precisely such species of Infusoria and Polythalamia as were *least characterized* by their form; besides, I had never myself observed the living forms of these Polythalamia, and had frequently, in forms of microscopic animals apparently similar, subsequently discovered distinguishing characters. It appeared indeed to me very remarkable, but as yet unfit to be a scientific basis for further conclusions.

Besides these forms, M. von Hagenow, in 1839*, specified, as a constituent of the white chalk of Rügen, *Oculina virginea* under the name of *Lithodendron virgineum,* according to Goldfuss. It is there observed that it is only the young brood of the now living form, whence therefore it is at the same time evident how little safety there can be in any conclusions with regard to identity, as our views can be attended with any degree of certainty only when the various states of age occur together.

II. *Incitements to further Inquiry.*

a. Preliminary considerations.

Notwithstanding the numerous recorded observations which have just been mentioned, several of the most eminent English geologists, together with some French philosophers of consideration, and the naturalist of Neufchatel the best fossil ichthyologist of our day, have continued to consider the fossil organisms of the chalk as being all distinct from still existing similar bodies, as is stated in Bronn's *Lethæa,* p. 771, and in the above-mentioned works. Thus, then, the idea of the Eocene or dawning period of the organisms still existing with us above the chalk has found

* *Monographie der Rugenschen Kreide Versteinerungen,* von Dr. v. Hagenow in Leonhardt's *Jahrbuch für Mineralogie,* 1839.

powerful defenders, and has maintained its ground to the present day.

As the first traces of fossil Infusoria were recognised only four years ago, it was not surprising that their forms appeared for the greatest part perfectly identical with existing ones; and it was obviously more essential to make further careful search among the living, than to regard such as were not identical with those now existing as actually extinct; for the relations under which they presented themselves to observation belonged to the outermost surface of the earth's crust. The phænomenon at Bilin now excited curiosity and almost surprise, namely, that the polishing slate* of that place, situated according to Elie de Beaumont's opinion† above the upper greensand‡, and to be assigned to the middle division of the geological tertiary period, together with its semi-opals, was found to consist of more than two-thirds of now living species; nay, the very animal (*Gallionella distans*) principally constituting the mass, was found alive near Berlin. So likewise (to say nothing of the superficially occurring farinaceous loose Bergmehls and Kieselguhrs) there were also found four-fifths of existing species of Infusoria in the polishing slate from Habichtswald near Cassel, which distinctly belongs to the tertiary formation. It seemed to result from hence, that the infusorial forms had continued to exist from the

* *Polirschiefer.*—See Prof. Ehrenberg on Fossil Infusoria, Art. XX. of our First Volume, p. 400.—Ed.

† *Comptes Rendus*, 1838, ii. p. 501. It seems necessary to observe here, that the meritorious geologist is evidently *à priori* rather inclined to ascribe this deposit to the more superficial relations, but nevertheless assigns it decidedly enough to the middle tertiary period. M. Turpin's analysis of the specimens furnished him by E. de Beaumont is most strangely and surprisingly *manqué*. He powdered the mass of the semi-opal to test it microscopically, by which process all organisms were of course rendered indistinct and destroyed, and then states that he found in them few organic constituents; but, nevertheless, there were still among the constituents found a *Protococcus* (!), and other blackish globules, which he regards as Infusoria eggs (!), and in part as mere pieces of the egg-shells (! !). He further found organic filaments, and an insect's leg (!), all of which he has figured. It seems to me as if M. Turpin had discerned nothing of the organized beings of the Bilin stone, but rather given a magnified representation of the organic constituents of the dust of his room; and as if the insect-leg, with its fine hairs and claws, which had remained miraculously unhurt in pulverizing the stone, was evidently no part of the semi-opal, but rather a part of a small *Acarus* from the dust of the object-table, or from the not quite pure water which M. Turpin employed for moistening. I have, moreover, never been able to go the length of recognising the egg-shells of Infusoria, in the sense of Turpin (of *Polygastrica*). This is not the way to examine. The since deceased active and talented author has deserved gratitude in other branches of natural history. *Mortuo sit terra levis!*

‡ *Plänerkalk.*

tertiary period more identical in kind than all the other larger organisms. However, the knowledge of these phænomena had in 1837 already proceeded so far, that, besides the fossil occurrence of several living species, such a considerable number of those which had been nowhere observed living could be enumerated*, that there was no longer any reason for not considering all the peculiar species of the fossil strata as still existing and only having hitherto escaped attention. The results of these observations also rather fell into the already formed track of the other fossil organisms, according to which in the deeper (tertiary) strata of the earth living forms occurred mingled with many species and genera no longer to be found. Hence, then, the former seemingly obvious opinion was continually more and more supplanted, that the collective loose Bergmehls and Kieselguhrs consisting of Infusoria were only formations of the most recent time and of the most superficial kind; for there was likewise in them always a considerable number of species which had not yet occurred living; according to which these formations of Franzensbad, Sweden, Finland, &c., likewise appeared not to belong to the present world, but to an earlier period of the tertiary epoch of the earth's formation.

Notwithstanding this apparent reconciliation of the previous differences of the microscopic results from those afforded by the consideration of larger bodies, and notwithstanding my endeavours to promote this agreement by most accurate observation, yet continued inquiries, instead of presenting a constantly more evident disclosure of new genera at the expense of formerly existing ones, have continually extended more of the life of the present world into the dead mass of the former one; and the constantly increasing number of the now living species from the fossil strata, even from the lowest tertiary layers, has gradually awakened the thought, *that the microscopic organisms*, whose numbers and relative masses are so inconceivable, *may have preeminently outlived certain great catastrophes of the earth.*

b. Physiological possibility of long duration of life of the Infusoria.

This result of reflection acquired increasing interest from a physiological ground, suggested by the investigation of existing species, for ascribing to infusorial forms a possible permanence

* In the Memoir on the relations of Masses of existing Siliceous Infusoria, printed in 1837, this relation has been treated of at length, p. 3 and 4.

superior to that of all others, to which indeed I have already briefly drawn attention in a remark in my last Memoir on the formation of the chalk*, but which I will here treat of somewhat more at length.

In the remarkable mode of reproduction by self-division, and in the indifference of these minute independent beings to climatic variations, there appear to reside characters which sufficiently distinguish them from larger beings, so as to make them pre-eminently adapted to a greater duration and extension through entire and successive formation-epochs of the earth.

α. Climatic relations.

In the first place, as regards insensibility to climatic variations, progressively extended researches have connected the most remote districts of the earth by similar living species. Europe, Africa, Asia, and America, together with the Islands of the Ocean (the Isle of France, the Isle of Bourbon), have abundantly presented perfectly similar minute bodies, partly fossil, partly living, in the most varied relations, as has been from time to time communicated to the Academy since 1830, and this knowledge has recently received very considerable accessions. Moreover, an exceedingly extensive diffusion of many similar species had manifested itself in fresh water and in the sea; and forms had been found on the Siberian Alps of the Altai so coincident with those of the Berlin waters, and of the Nile in Dongola, that the greater part of those brought with me, when placed side by side, and examined together along with the drawings sketched after nature, offered to the naturalist no characters by which they could be distinguished.

β. Self-division.

The other also of the characters above-mentioned, that of self-division, had, under continued observation, risen to a most surprising power, surpassing all calculation of its possible effect. By this mode of production and increase, the Infusoria not only stood connected with the other organisms, but they plainly exhibited a power of increase and transformation of matter which placed them as regards these functions at the head of all other known organized beings. The possibility of the multiplying of an individual to a million in less than forty-eight hours was exhibited in them (without taking into account an abundant development by eggs),

* Transactions of the Academy of the year 1838, p. 85.

by the mere process that each single animalcule can divide itself, within one hour, completely lengthwise or across, and after the lapse of one hour's rest can repeat the same thing. The vast effect of this activity, not perceptible in its detail, is, that a single animalcule, perfectly invisible to the naked eye, under circumstances favourable for all individuals, which usually do not occur, can possibly be increased in four days to 140 billions of independent animalcules. Now in the polishing slate of Bilin, about 41,000 millions form one cubic inch of stone, as may easily and pretty accurately be determined from the size and form of the corpuscles; consequently about seventy billions to each cubic foot of the geognostical stratum actually occurring there. Accordingly, this prolific nature of the living animalcules, if it pertains in like manner to *Gallionella distans*, would cause the production in four days, from one of the latter, of 140 billions of siliceous shells; or, which is the same thing, two cubic foot of a stone similar to the polishing slate or foliated tripoli of Bilin might be formed in four days from an imperceptible animal. We may here call to mind the exuberant but for the most part unproductive flowering of fruit-trees, and Buffon's celebrated calculation of the possible but never-occurring production of wood from the seed of a single elm; yet these views of existing forces and arrangements in organic nature are by no means empty speculations to be compared with the idle calculations of the number of steps which a man takes in his lifetime, or the number of grains of sand which would fill the earth's space; but they are records of actual relations of the living effect of hidden forces whose gradual tranquil progress astonishes man by its product, after the lapse of several of his generations, though perhaps unheeded in the lifetime of an individual; so that this simple natural effect, whenever, being favoured by collateral circumstances, it is more strikingly apparent than usual, may well seem marvellous or difficult of explanation; while the attentive observer only recognises in its extraordinary extension the accustomed course of nature less than usually disturbed. Thus one might well stand surprised and amazed before mountain chains composed of imperceptible minute animalcules nearly of one and the same species; but just as readily will a satisfactory explanation be found in the above physiological observations of the possible productiveness of these same animalcules. Nor shall we be in haste to conclude that for the formation of such astonishingly vast solid masses an immense period must have been requisite; as the concurrence of conditions favourable for organic life and action is able to produce in a very short time immense and wholly inconceivable masses. Thus, according to the above-stated facts and laws,—(I trust this last apparent, even when rarely scientifically useful, yet inductive play with numbers, will be allowed and pardoned me,)—a single animalcule, which can form in four days by its siliceous shell the solid mass of two cubic foot of polishing slate of that stratum near Bilin fourteen foot in depth, may have, in only eight days of equally and tranquilly continued undisturbed organic activity, increased to so great a number of individuals, that its mass filled the cube of a German mile; and fewer individual animalcules than are contained in a cubic inch of the Bilin polishing slate, in contemporaneous organic activity, favoured in every respect, would suffice in the same time to afford a mass of silica which would equal the size of the earth. All this would take place in that period, by an imperceptible Infusorium, and the simple act of the organic self-division of each individual within one hour after a repose of the same duration. Thus a number of small imperceptible drops of rain, disengaged with unusual rapidity and acting collectively, are able to destroy houses and hills. How many centuries, then, are requisite to form a bed of polishing slate fourteen foot thick near Bilin, or a layer of berg-mehl twenty-eight foot thick in the plain of Lunebourg, or a layer of chalk some hundred foot thick with alternate infusorial marls or substituent strata of flint, needs no further indication. It is almost superfluous to observe that he who should call the above explanation mere speculation would forget that it is based upon experience and observations; and he who might still find the phænomena inexplicable and marvellous would forget that they are of daily occurrence in the waters surrounding him, in conflict with many other forces quite as gigantic, more frequently overcome than overcoming. Whether these beings create carbon, lime, silica, and iron, or merely transform them one into the other, it is impossible to say, for the extent of their influence is beyond the reach of observation. I cannot deny that these considerations have acted as incitements to me, not indeed to a hasty systematizing, but to a further earnest prosecution of inquiry.

Self-division, as a mode of increase in the Infusoria, has another aspect, which is found in no other animal group in a degree so decidedly influential, and which is of peculiar importance as regards the fossil relations here to be explained. Whilst in organic nature, not merely in the larger organisms, but in all cases where accurate examination could be made, multiplication by impregnated seed and eggs has been recognised as giving rise to a certain fluctuation in the types of form of the posterity, by which the characters of the species frequently pass into each other, so spontaneous fission as well as gemmation, are modes of increase co-existing with oviparous generation, which do not favour this fluctuation of the types of form, but rather necessitate a certain constancy and stability, nay, similarity of the posterity in relation to their progenitors. In this, gemmation differs from spontaneous fission in degree. Gemmation, which is chiefly peculiar to vegetable organisms, yet also occurs frequently in minute animals, perpetuates distinctly the peculiarity of the parent body, but soon affords to the posterity a perfectly independent further development, which, merely through the elongated immediate substance and community of circulation of the parent body, remains for a time in closer connexion with the individuality of the same than the egg, and needs no impregnation. Self-division, on the other hand, restricts the free individual development of the posterity in the highest degree. The animals individualizing themselves, *i. e.* becoming organically independent, are, up to the half, the parent body itself, whose other half then constantly regenerates itself out of each individual. This mode of increase appears at first sight similar to the "laying down" of plants in horticulture, but physiologically it is something quite different. The layer or disconnected shoot, or even the stem divided into several parts for separate planting, is indeed the divided parent body itself, and one expects with confidence that it will have like development, and bear like blossoms and fruit. Nevertheless, the stem or twig is no demonstrated nor demonstrable individual; and in these cases we have rather undertaken a process which is similar to the division of a coral stem, by which the individual is not necessarily altered, nay, not even necessarily touched, merely the social form has been disturbed and changed in its combination. But it is expressly the organic individual which by self-division of the animal is essentially and necessarily altered, as it divides itself only in such manner that it separates its perfect organism self-actively into two halves, and closes anew [*i. e.* heals the wound of division], converting each half, by means of an activity similar to the organic regenerative force, into a separate organism, frequently capable of perfect independency.

On perfect self-division two individual bodies originate from one individual body, each of which two possesses, and actually is, a half of the other, which half perfects itself to its separate and closed individuality. This completion is effected by an internal activity, allied to regeneration, called into action by mere tension of the parts. It is not a true regeneration, for there is nothing wanting where it makes its appearance, and its very occurrence originates the separation. It is likewise no true reproduction, for it does not reabsorb local substances and supply their place at the same spot by new; it rather constructs from out of the organism new parts essentially changing it, which even do not belong to its individual growth. It is rather to be compared to organic exuberance, and yet it is an activity organically regulated in all parts, which may be best designated and distinguished as *self-division, spontaneous fissiveness.* All relations of form and of organism are by the mode of increase by self-division, with the aid of the divisive power, accurately prescribed from the mother body, and a fluctuation upon development can be but slight. In this manner there originate from the old forms, by means of the insertion or development of always two new half-bodies between each two old halves, constantly half-young forms, which nevertheless remain immediate parts of the substance of the old ones; and, even after a third division, the one of the halves then separated is no longer a previously integrant portion of the first parent body, yet it is completely determined and prescribed in all parts by this for the perpetuation of the form. It is the perpetuated life of the organic individual, in reproduction overpassing its limits, constantly becoming young by insertion of new half-bodies between the old by the aid of spontaneous fissiveness. The species is by this mode of reproduction completely subordinated to and formed after the parent type, according to which alone it can develope itself.

Certainly this character of reproduction by self-division will be of so much the more influence on very extensive series of organisms dependent on it, the more powerfully their increase takes place by this mode. From this evident, even if not frequently predominant yet exceedingly great, effect of self-division upon the increase of Infusoria, it will be easily intelligible if their forms recur very constant in very different circumstances, since they are frequently merely as it were numberless impressions and images of individual mother forms, nay, properly speaking, merely innumerable organically completed parts of one and the same divided individual. If there remain, as I have remarked above, such numberless multitudes of divided individuals together in a small space, then a slight local change of the earth's surface can destroy the whole mass, like other organisms; but if they become diffused over the earth by currents of the ocean and by the wind, then do we undoubtedly recognise a capability, denied to all shells and fish, &c. more confined to a certain climate, of withstanding for an incalculable period the destroying influences of the earth's surface, not merely as genus and species, but even as (only always partially reproduced) individual forms.

A constant and close reflection on these discoveries which were long ago made by me, the results of which I have hitherto made only a partial and occasional mention of because I felt the necessity of first making myself still more confident as to them, has had much influence upon my subsequent labours in this branch of science; and if others are more disposed to pass by these considerations with indifference, it may, as usual, perhaps arise from this circumstance, that the subjects lie remote from the previous circle of inquiry, and will then only obtain their natural interest when they shall have become clearly manifest. After having been constantly attaining a firmer basis, and a continually increasing conviction of the reality of these views, I felt incited to further deductions and inferences.

I first applied myself to the contradiction in my previous observations upon the relation of the microscopic organisms, according to which there were found as the main mass of the chalk marls of Sicily and Oran, six genera of infusorial shells consisting of numerous species quite different from any of the present world, while in the same marls, and in the white chalk itself, I had gradually recognised six species of recent Infusoria. I concluded, therefore, that either the six recent Infusoria, together with the two to three small Terebratulæ, which form exceptions to the more general law of nature indicated by the greater and supposed extinct forms of the fossil organisms of the chalk, are actually different from the recent; or that the previously unfolded and other similar physiological properties render the smaller organisms especially and more than others capable of longer duration, and of explaining several hitherto obscure geological relations of general interest.

III. *Researches on Recent Animals of the Chalk Formation, and their results.*

The motives which I have thus stated, induced me, during the vacation, Sept. 1839, to visit the sea coast, in order to be able to pursue more accurate observations on the microscopic oceanic forms which may concur in the formation of the chalk. A fortunate combination of circumstances enabled me to reach the nearest sea, the Baltic, although at the same time that which is the poorest in organisms. To examine my native chalk cliffs of Rügen, and to become acquainted with the actually existing animals of the sea there and in Wismar, was my design. I have accomplished both, but not at the same spot. From Rügen I brought with me a rich assortment of the organisms just visible to the naked eye, forming the chalk there, which recently have been so industriously collected and described by Fr. v. Hagenow. At Wismar I have obtained new explanations with regard to a series of organisms of the flints of the chalk which there cover the beach*; but at none of these points did I find abundant living marine organisms, and materials for the object of my inquiries. This determined me to make an excursion from Wismar to Kiel, the harbour of which, full of microscopic life, was already favourably known to me from the investigation of luminous animalcules; and here, indeed, my expectations were fulfilled and surpassed. Two whole evenings did the luminosity of the sea and microscopic investigations delight and employ me till late in the night. Dr. Michaelis, the second discoverer and first establisher of the luminosity of Infusoria, and Dr. Behn, the accomplished anatomist, accompanied me, and kindly aided me by their local knowledge. It was again evident that all the splendid light of the sea proceeded from Infusoria; and among their numberless forms, several of which had never yet been seen, I suddenly descried a light of a different kind, which, although of itself small and dim, far more excited my joy than the real light. It was what I was seeking, and scarcely any longer hoped to find. It was one of the most characteristic of those siliceous loricated animalcules which I was acquainted with from the chalk marls of Sicily, and which had already been represented on pl. iv. of my last academical lecture on the chalk. It was the actually still living *Dictyocha Speculum*, precisely the most remarkable of all the siliceous shells from the white marls of the chalk of Caltanisetta, expressly confirmed by Friedrich Hoffmann. Prof. Behn saw it in the living state in my possession, and I brought it away with me dried and well preserved.

* Some notices of this have been published in the Reports of the Academy, October, 1839, p. 157, which, together with the notices on the natural flannel of Sabor, will be communicated in greater detail at some other opportunity.

It was the first animal decidedly peculiar to the chalk formation observed alive, and a species of a genus hitherto regarded as purely antediluvial. The shortness of the time did not allow me to prolong this successful investigation in Kiel, although I thought that I had already recognised *Peridinium pyrophorum* of the flint of Delitzsch, also living and luminous, of which I subsequently became more fully convinced from the preparations which I brought away with me. I was now desirous of casting at least one look upon the more open North Sea. We hastened from Kiel by Hamburg to Cuxhaven, where, at Neuwerk, I wished again to examine some drops of the great ocean, and expected still more abundant booty of the same kind. I had only time to remain there one day, which unfortunately was rainy; but having gone to the neighbourhood of Neuwerk, I took samples of the water, and of the deposits of the sea at spots of the great surface left by the ebb which seemed most suitable. During low water I also took, in the neighbourhood of Cuxhaven, from various places, water for examination, and late at night also I had a bucket of sea-water taken for me, although the inhabitants and sailors present quite denied the luminosity of the sea under such circumstances, namely, during high tide and in rain. I soon examined with the microscope what I had procured, having filtered the water of the bucket to the amount of a bottle-full, and was immediately more fully convinced that *the species of organisms of the chalk formation still abound in our oceans.* As the circumstances under which I made the observations impressed themselves strongly on my mind, I have briefly described them. I was never so much struck at the mass of life in the ocean, as when viewing that bucket of water in Cuxhaven, full of large sparks of light produced by the *Mammaria scintillans*, and which also contained the richest treasure of millions of Infusoria, several of which were the same species as had occurred at Kiel. What again most excited my attention were several species of two of those genera of siliceous-shelled Infusoria which occur not only single and rare in the chalk marls of Caltanisetta in Sicily, Oran, Zante, and in Greece, but are exactly those forms which by the inconceivable number of their minute shells constitute *the great mass of these infusorial marls*, and at the same time also belong to genera which until now had *never been observed living*. I have succeeded in bringing the greater number of these species alive to Berlin, and have indeed this day, October 17, whilst writing, and whilst delivering my lecture, had before me, with their inner organs still well-preserved, *Actinocyclus senarius*, *Gallionella sulcata*, and *Coscinodiscus Patina*, the principal forms of the chalk marls of Sicily. Nor was it these species alone, so remarkable from their diffusion in the chalk formation, that were found in the water which I had procured for examination, but with them were also living a great number of invisible animalcules, exceedingly interesting, at least in a systematic point of view. Five of them are so peculiar in their structure, that they will form in the system of the polygastric animals four new genera, which I designate as

Eucampia,
Lithodesmium,
Triceratium, and
Zygoceros.

One or perhaps two other hitherto unknown forms from Kiel could only be placed with difficulty under genera already known: and for arrangement in the system must be separated as

Dinophysis, and
Ceratoneïs.

The following list contains those marine animalcules from Cuxhaven observed living together, which at the same time are constituent parts of the Sicilian and other chalk marls, as appears in the table annexed to the former memoir on the Chalk:

1. *Dictyocha Speculum*,
2. *Coscinodiscus Patina*,
3. ——— *radiatus*,
4. ——— *Argus*,
5. *Coscinodiscus minor*,
6. *Actinocyclus senarius*,
7. *Gallionella sulcata*.

These seven species, with the six already published, make up thirteen living species of silica-shelled Infusoria observed in the secondary formation; and these new and well-marked animals belonging to peculiar genera entitle us now to consider the previous six not so well characterized species belonging to known genera, as sufficiently well determined.

IV. *General Observations on the Organic Structure of the generically well-characterized Infusorial Forms of the Chalk Marl.*

Although I have prepared, as an Appendix to these communications, a more special description of the new animal forms in question, yet it appears to me advantageous to collect together a general view of the organic structure of these minute bodies, which have become so important from their relations.

When I published my first communications on the species of the genus *Dictyocha*, I remained in doubt whether they were really animals, or only fragments of animals, or parts of sponges. At all events, they were very regularly shaped corpuscles, consisting of silica, easily recognised by their remarkable form, which appeared to characterize the chalk marl, and not altogether unnaturally to join on to the forms of *Arthrodesmus* and *Xanthidium* of the Infusoria*. Careful attention to all circumstances had guided the first opinion with tolerable correctness. The knowledge of the living animal *Dictyocha Speculum* has not changed the view then entertained. They are evidently polygastric animalcules of the family *Bacillaria*, which are inwardly coloured green by numberless green granules, or according to analogy, bear egg-sacs full of green ovules, and have a very slow creeping motion. Scattered minute vesicles in their interior indicated, as distinctly as in many other *Bacillariæ*, the polygastric structure of the intestines. External locomotive organs could not be detected; it was however ascertained that the six long spines of the circumference are not inclosed by the soft body, but that this merely fills the inner space of the small hard basket-shaped portion of the animal, at the borders of which these spines project.

The genus *Coscinodiscus* was, in 1837, in my first memoir on its existence in Oran and Zante†, immediately placed in the family of the *Arcellinæ*, and the first form characterized as *Arcella? Patina*. But even in 1838, the further examination of the fossil fragments had led me to believe that probably two of these round plates might always belong to one individual animal. They were therefore subsequently, although always with doubt, placed under the family *Bacillariæ*, first with a mark of interrogation as *Gallionella*‡, then in the neighbourhood of the *Gallionellæ*§. The finding of four existing species has set aside all doubt and fully confirmed the latter view. These forms, in the living state, do not like *Gallionella* constitute long filaments resembling articulated

* Both in the illustrated work on Infusoria, concluded in 1838, p. 165, and in the memoir on the Chalk, printed in 1839, p. 73, the nature of these bodies remained doubtful, but they were in 1837 (Report of the Acad. 1837, p. 61) asserted to be nearly allied to the genus *Arthrodesmus*. These forms of the section of the *Desmidiaceæ* among the *Bacillariæ* have, by their siliceous shield, the peculiar interest, that they present a new obstacle to our viewing the soft-shelled *Desmidiaceæ* as plants on account of their organization being difficult to recognise and most of them having but very little motion, in as far, namely, as the other silica-shelled bodies, the *Naviculaceæ*, exhibit very distinct animal characters.

† Report of the Acad. 1837, p. 60. *Die Infusionsthierchen*, p. 134, published 1838. ‡ *Ibid*. p. 111. § *Ibid*. p. 172.

chains. They have a rather rapidly-closing perfect self-division, and are characterized thereby. They are only found as single free discs. Each pair of round plates connected by a broad ring forms an individual; and in the living specimens self-division was observed on the margin, which, according to the analogy of the *Gallionellæ*, is longitudinal division, with the appearance of horizontal division. At the outermost border of each disc was perceptible a circle of numerous minute apertures in the shell. A very finely divided, much-folded body, probably the ovarium, of a brownish or greenish colour, filled at times the entire inner space; but generally in the centre of the disc was a more transparent, usually ill-defined spot, comparable to the cicatricula in the egg, as in *Navicula*, which appears to be the chief part of the true animal body whence the other organs proceed to the periphery. A glandulous globular body, sometimes more sometimes less developed in the interior, appearing gray by transmitted light, was a singly-developed sexual gland, a large contractile simple oval vesicle once observed was the sexual vesicle, and small scattered transparent cells were analogous to gastric cells. Motion and locomotive organs were not distinguished.

The systematic character of the fossil genus *Actinocyclus* had, even in 1837*, been so correctly ascertained from its fragments found in Oran, that the inspection of the living species has not altered it. The only doubt that remained as to the fossil forms, was, whether in the living state they did not constitute chains, of which the contrary is now certain. In structure the *A. senarius* almost entirely resembles *Coscinodiscus*, the only difference being, that the inner space is divided by the radiating septa into six equal parts, and the ovarium into just as many larger folds, divided again into many other folds. Here also were perceived the brownish or green eggs which filled the disc, a transparent colourless spot in its centre, and scattered transparent cells (gastric cells). No gland, however, was distinguishable, nor locomotive organs. The organization of the fossil *Gallionella sulcata* appeared quite similar to that of the other *Gallionellæ*.

V. *On the Locomotive Organs of a large* Navicula *from Cuxhaven.*

The opinions of observers being still divided as to the true nature of the *Bacillariæ*, which become more and more important in geological researches, and the reasons advanced by me for their animal nature not having been attended to by some modern ob-

* *Berichte der Akademie*, 1837, p. 61.

servers, although the knowledge of the organic development of these forms is of essential importance for the establishment of the characters of the species, I think it will be useful to make more generally known, as early as possible, my late researches on their organization. The visible admission of indigo into the gastric cells of the *Naviculæ* by central exterior orifices, which I mentioned in 1838 and had then observed in *Closteriæ* and *Desmidiaceæ* and which I have succeeded in preserving as preparations for general inspection, is indeed so decisive a proof of the animal nature of all these forms, that a sight of them is all that is requisite; and the giving a bluish colour to some of their parts by tincture of iodine does not at all prove the vegetable nature of these bodies, since the possible reactions of this substance on organic matters are by no means fully known, even were microscopical observers always trustworthy chemists, and the colours produced determinable with sufficient accuracy, which frequently is not the case. Among the subordinate characters by which plants and animals are easily distinguished, may be considered not motion merely, but rather external organs of locomotion; and the more so, when these may be exserted and retracted.

At Cuxhaven there remained, in small pools left by the ebb of the tide, little bodies, which in form very much resembled *Navicula* (*Surirella*) *elegans* and *striatula*, but were essentially distinguished from the latter by their much larger size and the different sculpture of the shell. These animalcules, resembling a ribbed oval case of glass, and which belong to the largest species of *Navicula*, notwithstanding their size, exhibited great mobility, and I succeeded in discerning in them organs of motion far more distinctly than I had hitherto been able to do in other larger forms. These organs were likewise in shape and length very different from what I had hitherto observed in other *Naviculæ*. Instead of a snail-foot-like expanding sole, where the ribs or cross-bands of the shell join on to the ribless lateral part of the shield, there were long projecting minute filaments, which the animal shortened or lengthened at will, or drew in entirely. An animal $\frac{1}{18}$th of a line in length, had on each side as many as twenty-four for each of the two plates, or, in all, ninety-six such organs of locomotion; four also were visible on the broad frontal portion. The nutritive orifice appeared to be at the broad end. Whether other *Surirellæ* possess similar organs of locomotion in definite number is not yet ascertained: nor have I been able to determine whether these organs are merely supernumerary collateral filaments, cirrhi, together with a minute foot, such as the other *Naviculæ* have. Longitudinal fissures and central apertures on the broad side of the shell are not present; but more than six, and to the number of ninety-six, lateral apertures for the cirrhi are discernible. Perhaps this form may constitute the type of a distinct group of the *Bacillariæ*. I have besides recently convinced myself that the *Naviculæ* in general differ considerably in structure, as I have recognised distinctly the six round shell-apertures in many species also, as well as in *Nav. viridis*; but in some gaping fissures near them, and in others as yet undescribed gaping fissures without round apertures. These possibly generic or subgeneric distinctions may explain differences of opinion of various observers, who, from different objects, may have inferred a diversity of relations. The above species I have named, from its transparent large shield, *Nav. Gemma*.

VI. *Further communications on recent Organisms of the Chalk**, October 31st, 1839.

Part of the animals of the chalk marl are still living in Berlin, in the sea-water collected on the 22nd September at Cuxhaven; and by continued examination I have even discovered additional species. Of especial interest are two new large species of the genus *Actinocyclus*, one with 8 chambers and 16 rays (septa), the other with 9 chambers and 18 rays, which have been designated as *Actinocyclus sedenarius*, and *octodenarius*.

But besides this, a new phænomenon has occurred which awakens a still more general geological interest; for, besides the above-mentioned living siliceous-shelled Infusoria, two species of still living microscopic *Polythalamiæ* have also been found which bear decidedly the character of two of the most extensively diffused calcareous-shelled animalcules of the chalk,—namely, *Planulina* (*Rotalia*?) *turgida*, and *Textilaria aciculata*. Both forms were recognised in a few specimens, and afterwards in several, but unfortunately not early enough to submit to examination fresh and lively animalcules. In *Planulina* change of place has been observed, but the organs of locomotion remained

* This and the following additions I have considered advantageous to insert here, with the permission of the Academy, in order to render complete the scientific view of our entire knowledge of the subject up to the printing of the memoir. These observations were communicated on the 31st of Oct. 1839; those of the following section on the 16th of Jan. 1840, under which date they will be found noticed in the Reports of the Academy.

hid under the shell. In both forms, however, the animal contents of the minute shells, which are also more transparent and clear in their structure than the fossil, have been placed beyond doubt.

In my former memoir on the formation of chalk by microscopic animals, four species were arranged among the calcareous animalcules of that deposit, with a mark of doubt as to their being identical with living species, and an apology for the uncertainty from a want of knowledge of living specimens. They were *Globigerina bulloides*, D'Orbigny; *Globigerina helicina*, D'Orbigny; *Rosalina globularis*, D'Orbigny; and *Textilaria aciculata* of the same observer. With respect to the last species, I now entirely withdraw my doubt after these recent observations, and assert the forms of the chalk and of the present day to be identical; nor do I any longer think I require an excuse, if I likewise admit the other three as forms actually belonging to species of the present world, since a second fossil species could really be observed in a living state.

Thus, then, (microscopic) calcareous-shelled animalcules of the chalk still exist in a living state; and the whole number of the forms at present found to be identical* is according to observation 15, but probably to be provisionally fixed at 18 or 20, viz. 13 siliceous-shelled animalcules, 2 Xanthidia of the flint (*Xanth. furcatum*, and *hirsutum*), and 5 calcareous-shelled animalcules; at the same time, the circumstance should be well noted, that *many of these forms are exactly those of which the mass is composed, consequently the most numerous in individuals of the chalk formation, and not the more rare*, which appears to lead to a settlement of the still existing physiological difficulties.

VII. *Explanation of the Organization of several Polythalamia of the North Sea observed alive in Berlin*† (Jan. 16th, 1840).

An external animal, having the form of a *Sepia*, and which bore the minute shell frequently resembling a Cornu Ammonis as an internal bone in the back, had several years ago, after diligent observation, been ascribed by D'Orbigny to the Polythalamia, whose very minute and often microscopic calcareous shells constitute in inconceivable numbers, and in now nearly

[* Mr. E. Forbes (Feb. 1842) remarks as to the results of his dredging in the Adriatic, "Strange to say, the most characteristic shells in those depths are species known only in a fossil state hitherto."—*Annals of Nat. Hist.*, No. 57, for May 1842.—ED.]

† Report of the Academy, Jan. 16th, 1840. See note in preceding page.

1000 different known forms, the chief mass of chalk rocks and of many sea-sands. Dujardin, on the other hand, had subsequently denied to these animals all organic structure, and asserted them to be simple living and extensile slime, encased by a hardened external shell.

In my observations on the formation of the chalk from microscopic animals presented to the Academy in 1838 (where also historical notices relative to this subject are collected), the great influence of these minute bodies on such rock formations was explained. From the observation of a living species in the Red Sea, and by moistening the small dried bodies of several such forms taken from the sea-sand, and dissolving the delicate calcareous shell from the body in weak acids, by means also especially of the knowledge gradually acquired from rendering the shells transparent by the process there stated, they were classed with the Moss-coral animals (Bryozoa ?). Finally, the interest in these minute bodies was heightened from the circumstance of two of those forms (*Planulina* (*Rotalia* ?) *turgida*, and *Textilaria aciculata*), which help to constitute the great mass of the chalk by their incalculable number, having (in perfect opposition to existing general geological phænomena) been detected still living in Berlin in the sea-water collected at Cuxhaven in Sept. 1839. But further details of the organization could not be established.

From the importance which Nature imparts even to these minute organisms, which she has placed indeed, in individual energy, far below lions and elephants, but in their more general social influence far above them; and from the fluctuation in the opinions of naturalists on the true nature of these minute bodies, caused by the difficulty of examination, it may not be amiss to add, at the earliest opportunity, some recent observations to what I have before communicated. I have indeed the pleasure to exhibit alive to the Academy (Jan. 16th, 1840) ten such animalcules, in form resembling an Ammonite or Nautilus, of a size easily visible, together with drawings, and to solve all doubt as to the main points regarding the nature of these bodies.

The forms observed in October of last year (living chalk animalcules) were very minute, and exhibited, it is true, organic contents and locomotion, but no external organs. I was as little successful in clearly discriminating the internal organization. Those which I exhibit today are (not indeed forms observed as having existed at the period of the chalk formation, but) so large, that several parts of the organization may be immediately distinguished at first sight. Moreover numerous locomotive organs are very distinctly visible, although the motion in all the forms is exceedingly slow. I have gradually observed of these larger forms (even to one-fourth of a line in size) seventeen specimens (and recently seven more), all of which have been preserved alive in the sea-water at Berlin ever since the 22nd of September 1839. They belong to two different genera; eleven specimens to a hitherto undescribed large species of the already known genus *Geoponus* (*Polystomatium* without umbilicus), which I call *G. Stella borealis*, and six to a species equally large of the known genus *Nonionina*, which I shall term *N. germanica*. The two genera *Geoponus* and *Polystomatium*, were called by D'Orbigny *Polystomella*. They are represented in Plate V. and VI.

It is at once evident that there is no outer body surrounding the shell, but only an interior soft one. The supposition that *all* these little animals, as asserted by D'Orbigny, or even as had been observed only in *Sorites Orbiculus*, possess an exsertile head with a plumose feeling and prehensile apparatus, such as the *Flustræ* and *Halcyonellæ* have, was not found to be confirmed. All the animalcules, even the most developed, of the two genera *Geoponus* and *Nonionina*, are, like those of *Planulina* and *Textilaria*, without any prehensile apparatus at the head, and without any circle of feelers around the mouth. Each body is surrounded by the hard shell, has an ordinary simple aperture, and the numerous adhering bodies of *Geoponus*, whose social form (polypidom) resembles surprisingly the individual animals of *Nonionina*, have just as many visible simple apertures. On the other hand, the number of minute very extensile tentacula which at the same time effect the locomotion, and which project as it were from all parts of the sieve-like shell, evidently resemble the contractile fringes of the *Flustræ* and marine Gasteropodes. Their relationship to the pseudopodia [*Wechselfussen*] of the *Difflugiæ* of Infusoria is indeed very great, as Dujardin has correctly observed; but the rest of their organization, which this observer overlooked, removes them from the Infusoria quite as far as from a chaotic primitive substance. Great bundles of contractile filaments, arbitrarily ramifying (not as Dujardin asserts *actually*, but merely apparently confluent), appear frequently to project from the umbilical district, where there are perhaps distinct and larger contractile apertures.

The foremost and largest cell of all the animals, sometimes the

2–4 following ones, contain quite transparent parts only. In general, from the second cell of each little ammonite, all the hinder cells are filled with two differently-coloured larger organs. One of them is the generally greenish-gray very thick alimentary canal, which forms, like the whole body, a jointed chain, expanded in each joint, and connected by a narrow isthmus (probably the sipho) with the adjoining anterior and posterior ones. After dissolving the shell of the living animal by weak acid, various siliceous Infusoria swallowed as nutriment might be distinctly perceived in the alimentary canal of *Nonionina germanica,* even in the innermost divisions of the spirals. There is no polygastric structure of the alimentary canal, but it is a simple organ distended in the compartments of the body, consequently itself articulated with a single anterior aperture. Hitherto all the animals rejected coloured food. I never observed siliceous Infusoria in the intestines of *Geoponus,* but in these associated animals the space is certainly closed for each individual, and consequently much more confined than in the single animals of *Nonionina.* After dissolving the shell with acid, where Dujardin found only a body remaining behind in the *Rotalia,* I was enabled, by a very slow process, to discover in both also a completely spiral, articulated, inner body, the single articulations of which were connected in *Nonionina* by one, in *Geoponus* by eighteen to twenty tubes (*siphones*), as connecting parts of as many individual animals, lying close to one another in each articulation. Powerful acids destroy the shell so violently, that the delicate body is torn into numerous minute flakes. One drop of strong hydrochloric acid mixed in a watch-glass full of water, is just of the proper strength to dissolve in a short time the shells from the bodies of the animals placed in it.

Besides the alimentary canal, a yellowish-brown granular mass is perceptible in each articulation, up to the last of the spirals, the first excepted. In *Geoponus* it envelopes irregularly a great portion of the alimentary canal; in *Nonionina* it always forms a frequently globular reddish-yellow mass at the inner sides of the articulations nearest to the umbilical district. This part of the organization may probably be regarded, from its coarse granular consistency, as the ovarium.

Very surprising, moreover, was the occurrence of three specimens of *Nonionina* which bore pretty large petiolated membranaceous sacs with torn apertures fastened on to the back of their shell. These sacs appeared to be emptied egg-cells, similar to those which the marine Gasteropodes (*Strombus*, &c.) exhibit, clustered racemosely, and also some *Bryozoa* fastened singly to the exterior of their shell, which I also had observed in *Stylaria proboscidea*, but which have never yet occurred in any Infusorium. They are secreted small and soft; then swell very much in water (just as frog spawn), and harden. I have kept two of these forms with egg-cells well preserved, dried, in my collection.

Besides these positive characters, I have taken great pains to deduce with some certainty a negative one; it is the non-existence of pulsating vessels. In all mollusks, even in the very minute *Aggregati s. Ascidii compositi*, I have always distinctly observed these pulsations above many other far coarser parts of organization; but they are evidently wanting in the above-mentioned two genera of Polythalamia. Now this observed deficiency removes for the present all the Polythalamia decidedly from the proximity of the mollusks and articulated worms, and places them in the series of the *pulseless gangliated animals*, or *invertebrate vascular animals* (*Ganglioneura asphycta*), although the nervous mass and the vascular system have not yet been ascertained. The other characters already communicated to the Academy in 1838, together with the position in the natural kingdom there given, are only confirmed and established by these subsequent observations; and the notion, recently transferred from the Infusoria, which had outgrown these ideas, to the Polythalamia, of an animated simple organic substance occurring here and there, is hence likewise less and less supported by the experience which becomes daily more extended *.

* M. Dujardin has this last August, in a *Mémoire sur une classification des Infusoires en rapport avec leur organisation*, laid before the French Academy a new arrangement of the Infusoria, in which he has again placed the *Polythalamia* as *Rhizopoda*, in the same order with *Amœba* and *Actinophrys* of the Infusoria, and merely retained them in a separate family. But if the anatomical and physiological details of the different organs deserve the least attention as to their general arrangement and systematic distribution, and mere external relations of form be not alone considered as of value, then this classification, where peculiar, is often not to be considered successful. M. Dujardin has not demonstrated the polygastric structure of the *Rhizopoda*, indeed no structure at all; and that it is not polygastric, will again be evident from the investigation now communicated of other groups of these bodies. Moreover, this diligent author has given new names and characters to many things well known and characterized long before, which, at some other opportunity, will be more specially pointed out and adjusted.

[To be continued.]

SCIENTIFIC MEMOIRS.

VOL. III.—PART XI.

ARTICLE XIII. continued.

On numerous Animals of the Chalk Formation which are still to be found in a living state. By DR. C. G. EHRENBERG*.

VIII. *Latest additions to our knowledge of the forms of Recent Animals from the Chalk.*

[From the 29th of June, 27th July, and 13th August, 1840†.]

RECENT animals of the chalk marl have been found not only in the sea-water near Kiel and Cuxhaven, but likewise, as stated in my communications of the 29th June, very numerously in the sea-mud of Christiania in Norway, where no chalk rocks occur in the vicinity, and from whence M. Boeck has had the kindness to forward such sea sediment to Berlin. Besides very numerous specimens of *Dictyocha Speculum,* there were also in it very many of *Dictyocha Fibula,* a form which likewise had hitherto only occurred fossil in the chalk marls. At the same time there were also shells of *Coscinodiscus radiatus* of the Sicilian chalk, together with *Navicula viridula,* and *Synedra Gallionii,* which two latter species belong to the present world, and have never yet been observed as constituting the chalk‡. Besides these, there were also some hitherto undescribed sea Infusoria, which have been named *Navicula Entomon,* a form constricted in the centre, *N. Folium, N. norwegica,* and *N. quadrifasciata.* Of especial interest also were two still living stellate

* From the Transactions of the Royal Academy of Berlin for 1840. Published also separately in folio by Voss, of Leipzig.

† Monthly Reports of the Academy.

‡ *Navicula viridula* has, however, quite recently been likewise discovered by me in Greek marls, characterized by polythalamic calcareous-shelled animalcules as belonging to the chalk formation, which probably come from Ægina.

forms with five and six rays, which approach very near to *Dictyocha Stella* of the chalk marl of Caltanisetta, and together with this species form a distinct group in the genus *Dictyocha*, whose radiate skeleton of siliceous bars is not reticulately anastomosed. These two new species are, *Dictyocha* (*Actiniscus*) *Sirius* with six longish rays, and *D.* (*Ac.*) *Pentasterias* with five rays. Unfortunately, the greater number of these forms, from having been sent by way of Stockholm, as opportunity offered, and not having come to hand till six months afterwards, were dead when they arrived, and some few only still retained life and motion; yet even in the dead shells there could generally be detected, although altered in form, distinct remains of the ovaries and other organic details, which distinguished them very accurately from any fossil remains accidentally mixed with the mud.

From the memoir read on the 27th of July, it appeared that in the Peruvian and Mexican sea-water microscopic organisms are also met with still in a living state, which are partly identical with species from the chalk marls, and partly illustrate a hitherto obscure form occurring in them, namely by claiming for the present world an apparently extinct genus peculiar to the chalk.

This latter form was found on an alga at Callao, which Du Petit Thouars brought thence to Paris, and which Dr. Montagne had named *Polysiphonia dendroidea*, together with *Podosira moniliformis*. It had completely the zigzag and at the same time riband-like form of *Tabellaria vulgaris* (*Bacillaria tabellaris*), but was neatly divided in the interior by two curved septa in the longitudinal direction of each individual bar into three chambers. A very similar form, but as a single bar, was noticed in the Memoir on the Masses of existing siliceous Infusoria, 1837, as *Navicula africana* of the chalk marl from Oran, and more accurately described in the Memoir on the Formation of Chalk in 1838*. In this fossil state it had, however, as stated, always presented itself merely as single bars, whose form and large central aperture brought them nearest to the genus *Navicula*. In the living form from Peru it might be seen, even in the dried and remoistened state, that several such bars are connected together in the form of zigzag incised gaping bands, and that the curved lines in the interior of the individual bars were essential parts of the organization, as three green and granular inner sacs,

[* Full abstracts of these Memoirs by T. Weaver, Esq., were published in the Philosophical Magazine, Nos. 118 and 119; and in the Annals of Nat. History, vol. vii. p. 296.—Ed.]

such as had already been termed ovaries or egg-sacs in the allied species, were separated by these lines as by septa. Now, since this form constituted chain-like animal stocks, the central aperture of the individual bars, both in them and in the fossil bodies resembling them externally and internally, could not be the mouth aperture present in the *Naviculæ*; but it was evident that the fossil single bars had been produced from broken chains, which originally, like the living ones, had formed zigzag bands, a circumstance which had already been observed in the European *Tabellaria vulgaris**, which, however, does not show any interior division into chambers. Thus, then, the absence of inner chambers became an important character of distinction of these forms from the *Tabellaria*; and the chain and zigzag band formation, brought about by imperfect self-division, together with the consequent different position of the alimentary apertures, became likewise important characters for distinguishing them from the *Naviculæ*. This new generic type was characterized with the name of *Grammatophora*, and arranged near to *Bacillaria* and *Tabellaria*.

The species from Peru, *Grammatophora oceanica*, was, it is true, not found to be identical with the fossil *Grammatophora* (*Navicula*) *africana*; but recently *Gr. oceanica* has been found alive in the North Sea, as well as fossil in the chalk marl of Oran; and even the true fossil *Gr. africana* of the chalk marl has been found in a living state in the Cattegat.

There was found also on some Mexican Algæ, which Carl Ehrenberg had recently brought from Vera Cruz, a specimen of *Coscinodiscus eccentricus*, which had already been indicated as living in the North Sea, near Cuxhaven, and the fossil shells of which lie imbedded in the chalk marl of Oran in Africa. There were, moreover, on these Mexican Algæ, two distinct species of the new genus *Grammatophora*, which I have called *Gr. mexicana* and *Gr. undulata*†.

* In my work, *Ueber die Infusionsthierchen*, 1838, p. 199.

† In the communication made on the 27th of July, 1840, it was pointed out that the examination of marine forms from Peru and Mexico had led to the result, not unexpected, indeed, but hitherto uncertain, that there really are peculiar genera of Infusoria in other parts of the earth, while those previously enumerated have been likewise found in Europe, with the exception of a few species less accurately examined. *Podosira moniliformis*, a petiolated *Gallionella* from Callao, was represented as a very decidedly peculiar form, and the genus *Grammatophora*, comprising three species, was then also only known from America and the chalk marls of Africa. But since then this latter genus has been detected in numbers in the North Sea and Baltic; and thus, then, *Podo-*

A quantity of marine sediment, from the island Tjörn in the Cattegat, which Bishop Eckström of Gothenburg had collected in order to advance the objects of my pursuit, and forwarded to me at Berlin through the kindness of M. von Berzelius, was surprisingly rich in recent specimens of animals of the chalk formation. In this sediment of the North Sea were found not only all the siliceous-shelled animalcules of the chalk marls already observed at Cuxhaven, but also twelve other species which had previously been found in the marls of Caltanisetta in Sicily, and Oran in Africa. But most interesting was the occurrence of the living *Grammatophora* (*Navicula*) *africana* (known only as fossil from Oran), together with *Gr. oceanica*, recently obtained from Peru, which tends much to weaken the idea that there exist in other parts of the globe forms of Infusoria considerably and generically distinct from the Europæan*. There was also a new genus, a four-sided prismatic animalcule, which had been first detected in the chalk marls of Oran and Greece, resembling a *Staurastrum* in external form, but distinguished by the presence of a siliceous shell and four large apertures at the four corners. This form has been distinguished under the name of *Amphitetras antediluviana*. There was also a form quite similar to *Dictyocha Speculum*, only not smooth in its minute cells, but provided with short spines or teeth, which I had previously observed in the chalk marls of Caltanisetta and Oran, and which I have characterized as *Dictyocha aculeata*. Lastly, I found in vast numbers a series of eight of those species of the genus *Actinocyclus* which help to form the greater mass of the silica in the chalk marls of Oran and Caltanisetta, and which are readily and essentially characterized by the number of their rays; partly such as were only recently observed by me in those marls, and partly species which have already been noticed with 6, 7, 8, 9, 10, 11, 12, and 15 rays, which I shall name *Actinocyclus biternarius* (not *senarius*, which likewise occurred), *A. septenarius, octonarius, nonarius, denarius, undenarius, bisenarius* (not *duodenarius*), and *quindenarius*. All these species belong exactly to that division of the

sira moniliformis is still the only peculiar extra Europæan genus that has been accurately examined.

[* This view is also greatly confirmed by the recent researches of Dr. Cantor; he observes, in his remarks on the Flora and Fauna of Chusan, "that most of the forms observed at the island of Lantao, situated in the mouth of Canton river, and at Chusan, also inhabit Europe." Annals of Natural History, vol. ix. p. 361.—Ed.]

genus *Actinocyclus* with rays without septa, of which previously no living species was known, and which might have been regarded as especially characteristic of the chalk formation, but which in fact it is not.

Recent repeated examinations of the chalk marl have convinced me, then, that two long-known siliceous-shielded animalcules, *Striatella arcuata* and *Tessella Catena,* which are of exceedingly frequent occurrence in the water of the North Sea, and also abundant in that of Tjörn, are likewise present in small number in those marls; and also the centrally constricted *Navicula Didymus,* which I had first observed near Cuxhaven, was detected, on further research, as empty shells in the chalk marl from Caltanisetta.

I had, moreover, succeeded, up to August 1840, in detecting *Coscinodiscus lineatus* and three calcareous-shelled Polythalamia as still living, in the salt water from Cuxhaven, which had been preserved and frequently examined, viz. *Rotalia globulosa, R. perforata,* and *Textilaria globulosa,*—also species of those very genera, which, according to my previous memoirs, have essentially contributed to the formation of the chalk rocks.

These twenty recently-observed forms, then, added to the twenty species previously published in October 1839, increased our knowledge of still existing chalk animals to forty species.

I will now add some observations of the latest date. I have discovered among the living forms in the salt water from Tjörn *Actinocyclus quinarius* and *Coscinodiscus eccentricus,* together with *Grammatophora angulosa* as found at Oran. I have further detected, by a repeated examination of the chalk marl from Greece forwarded to me by M. Fiedler of Dresden, and especially of that numbered 5, which, as stated in his 'Travels' (i. p. 224), appears in Ægina to form the base and principal mass of a trachytic mountain, an unexpectedly great number of recent forms, and there associated with decided chalk animals. The composition of this Greek marl is perfectly identical in many characteristic forms with the chalk marl of Caltanisetta; and contains, together with these, others which coincide so perfectly with those from Oran, that even now I must consider that geognostical stratum as chalk. On a fresh examination I was at once surprised by the characteristic *Grammatophora undulata* of the Mexican sea-coast, and likewise by *Dictyocha (Actiniscus) Pentasterias,* but recently and first observed in the sea-water from Christiania. There were also found *Triceratium*

Favus, one of the characteristic forms first observed living near Cuxhaven, and which is represented on Plate VIII.; *Eunotia granulata* and *Cocconema lanceolatum*, common recent forms of brackish and fresh waters, and *Biddulphia pulchella*, a widely-diffused true marine form of the present day; also *Navicula quadrifasciata*, *norwegica*, and *Entomon*, together with *Coscinodiscus Oculus Iridis*, all species which I had found living in the waters of the Baltic and North Sea. Besides these ten bodies, only two of which had been observed in tertiary strata (they both also occur at Bilin), there were several other hitherto unknown minute shells, of which one, *Haliomma radians*, is of especial interest. In a bottle-full of turbid sea-water, obtained a few days ago from Cuxhaven, I discovered this form among many living chalk animals of species already described; and at the same time, in a living state, *Planulina* (*Rotalia*) *ocellata*, which had hitherto occurred only in the true white chalk and the chalk marls.

Thus, then, of the six peculiar genera of siliceous-shelled animals of the chalk formerly announced to the Academy, the genera *Actinocyclus*, *Coscinodiscus*, *Dictyocha*, and *Haliomma*, have been proved, at least as regards several species, to belong to the present world; and it is scarcely probable that the two genera *Cornutella* and *Lithocampe* will remain behind. The number of recent microscopic animals of the chalk, which in my first communication comprised thirteen species, amounts at present, according to observation, to fifty-seven species.

IX. *Conspectus of all the Forms belonging to the present world identical with those of the Chalk Formation.*

As in the commencement of this memoir only those observations respecting the occurrence of recent animals in the chalk were mentioned, which, notwithstanding the doubts cast upon them by the researches of Deshayes and Lyell in 1831, have again been maintained, it appears at present desirable to bring into a general view all the recent data which have come to my knowledge, more in detail than they have hitherto been, and to add to them my own latest observations.

M. Defrance in France, and Dr. Fitton in England, observed contemporaneously in 1824, that in fossil strata beneath the chalk there still occurred some recent species of animals. Defrance made known as such a *Trochus* and two or three *Terebratulæ*, among which was the *Terebratula vitrea* of the chalk deposit itself*. An accurate distinction of the *Terebratulæ* according to characters defining precisely their species, had then however not been attempted. Moreover, Defrance merely asserted the *Terebratula vitrea* of the chalk to be identical (*paroit être identique*) with the recent species, and with regard to *Trochus* and the other species, merely observed that, notwithstanding the great extent of his collection of fossils, they were the only ones occurring in and below the chalk with which he had become acquainted analogous to organisms still existing (*qui ont de l'analogie avec des espèces qui vivent aujourd'hui*).

Especially remarkable therefore were the cotemporaneous observations of Dr. Fitton, who made known several recent river-shells, especially *Paludina vivipara* (= *Vivipara fluviorum, Paludina fluviorum*), as occurring in great masses and entire beds in the Isle of Wight, in the stratum situated as supposed beneath the chalk, and which the English call Wealden clay after the Weald (Forest) of Sussex, where it likewise occurs†.

In the subsequent important works of Deshayes and Lyell, all these observations, after an exceedingly careful revision of fossil phænomena in the years 1831 and 1833, were characterized as uncertain and erroneous; and on the contrary, the already mentioned doctrine was asserted, that in the tertiary strata only of the earth's crust were still living forms with certainty to be found. Friedrich Hoffmann also, in his lectures of 1834 on the history of Geognosy, when speaking of the Weald clay and Dr. Fitton's observation of recent fresh-water shells in it, says, "there appears, however, to be some error here‡."

Thus then the cautiously advanced opinion of Von Buch, renewed in 1834, upon the identity of *Terebratula vitrea* of the chalk and that of the present world, became of fresh and especial importance, although it has also hitherto been disregarded in most recent works on the subject, from the predominant inclination in favour of Lyell's Eocene period; and the subsequent labours of Milne Edwards§ and Agassiz have attacked it in other directions. Let the following catalogue then awaken further interest in these inquiries.

From 1824 the following fourteen forms have been noted as

* *Tableau des corps organisés fossiles*, par M. Defrance. Paris, 1824, p. 63.
† Fitton in Thompson's 'Annals of Philosophy,' 1824, vol. viii. p. 374.
‡ F. Hoffmann's *Geschichte der Geognosie*. Berlin, 1838, p. 239.
§ *Annales des Sciences Naturelles*, 1836, vol. vi. p. 321.

occurring recent and beneath the tertiary formation; but almost all of which have been again called in question.

a. Polythalamia.

1. *Spirolina cylindracea*, Lam., from Paris, according to Soyer Willemet in Bronn's 'Lethæa.'

b. Corallines.

2. *Oculina virginea*, from the chalk of Rügen, according to Goldfuss and Von Hagenow, 1839.

c. Echinoidea.

3. *Clypeaster oviformis*, Lam., from Dax, according to Grateloup, 1836*.
4. *Spatangus acuminatus*, Lam., *ibid.*
5. ——— *canaliferus*, Lam., *ibid.*
6. ——— *gibbus*, Lam., *ibid.*
7. ——— *ovatus*, Lam., *ibid.*
8. ——— *punctatus*, Lam., *ibid.*

d. Mollusca.

9. *Cyclas cornea*, from the Weald clay, according to De la Beche, 1831†.
10. *Terebratula vitrea*, according to Leopold v. Buch, 1834.
11. ——— *Caput serpentis*? (= *striatula*), according to Leopold v. Buch, 1834.
12. ——— *al. sp.*, Leopold v. Buch, 1834.
13. *Paludina vivipara*, from the Weald clay, according to Fitton, 1824.
14. *Trochus*, according to Defrance, 1824.

To these species, only few of which are as it seems suited for safe comparisons, are to be added the following fifty-seven microscopic bodies, very accurately compared after numerous observations, and, from the peculiarity of their form, capable of being perfectly distinguished. In order to show at a glance the certain relation of the fossil form to the chalk, the white writing chalk is denoted by the letters W. Ch., and to each species one of the characteristic localities has been added. So also Ch. M. is used to denote Chalk Marl.

* I have since been able to compare this small work by Dr. Grateloup, which however bears a different title to that given by Bronn. It is called *Mémoire de Geo-zoologie sur les Oursins fossiles* (Echinides), par Grateloup. Bordeaux, 1836. It has merely been revised by Charles des Moulins.

† In De la Beche's Geological Manual, London, 1831, p. 296, there is a full catalogue of the Weald clay fossils.

a. Calcareous-shelled Polythalamia.

1. *Globigerina bulloides*, W. Ch., Denmark.
2. *Globigerina helicina*, W. Ch., Cattolica.
3. *Rosalina globularis*, W. Ch., Gravesend.
4. *Planulina* (*Rotalia*) *ocellata*, W. Ch., Cattolica.
5. *Rotalia globulosa*, W. Ch., Rügen.
6. ——— *perforata*, W. Ch., Cattolica.
7. ——— (*Planulina* ?) *turgida*, W. Ch., Gravesend.
8. *Textilaria aciculata*, W. Ch., Brighton.
9. ——— *globulosa*, W. Ch., Rügen.

b. Siliceous-shelled Infusoria.

10. *Actinocyclus quinarius*, Ch. M., Caltanisetta.
11. ——— *biternarius*, Ch. M., Caltanisetta.
12. ——— *senarius*, Ch. M., Caltanisetta.
13. ——— *septenarius*, Ch. M., Caltanisetta.
14. ——— *octonarius*, Ch. M., Caltanisetta.
15. ——— *nonarius*, Ch. M., Oran.
16. ——— *denarius*, Ch. M., Oran.
17. ——— *undenarius*, Ch. M., Oran.
18. ——— *bisenarius*, Ch. M., Oran.
19. ——— *quindenarius*, Ch. M., Oran.
20. *Amphitetras antediluviana*, Ch. M., Oran.
21. *Biddulphia pulchella*, Ch. M., Greece.
22. *Cocconema lanceolatum*, Ch. M., Greece.
23. *Coscinodiscus Argus*, Ch. M., Caltanisetta.
24. ——— *eccentricus*, Ch. M., Oran.
25. ——— *lineatus*, Ch. M., Caltanisetta.
26. ——— *minor*, Ch. M., Caltanisetta.
27. ——— *Oculus Iridis*, Ch. M., Greece.
28. ——— *Patina*, Ch. M., Zante.
29. ——— *radiatus*, Ch. M., Caltanisetta.
30. *Dictyocha aculeata*, Ch. M., Caltanisetta.
31. ——— *Fibula*, Ch. M., Caltanisetta.
32. ——— *Pentasterias*, Ch. M., Greece.
33. ——— *Speculum*, Ch. M., Caltanisetta.
34. *Eunotia granulata*, Ch. M., Greece.
35. ——— *Zebra*, Ch. M., Greece.
36. *Fragilaria rhabdosoma*, W. Ch., Gravesend.
37. ——— *striolata*, W. Ch., Gravesend.
38. *Gallionella aurichalcea*, W. Ch., Rügen.
39. ——— *sulcata*, Ch. M., Caltanisetta.
40. *Grammatophora africana*, Ch. M., Oran.
41. ——— *angulosa*, Ch. M., Oran.
42. ——— *oceanica*, Ch. M., Oran.
43. ——— *undulata*, Ch. M., Greece.

44. *Haliomma radians*, Ch. M., Greece.
45. *Navicula Didymus*, Ch. M., Caltanisetta.
46. ——— *Entomon*, Ch. M., Greece.
47. ——— *norwegica*, Ch. M., Greece.
48. ——— *quadrifasciata*, Ch. M., Greece.
49. ——— *ventricosa*, Ch. M., Oran.
50. ——— *viridula*, Ch. M., Greece.
51. *Peridinium pyrophorum*, Flints of the W. Ch., Gravesend, and in the North German Plain near Delitzsch.
52. *Striatella arcuata*, Ch. M., Oran.
53. *Synedra Ulna*, Ch. M., Oran.
54. *Tessella Catena*, Ch. M., Caltanisetta.
55. *Triceratium Favus*, Ch. M., Greece.
56. *Xanthidium furcatum*, Flints of the W. Ch., near (Delitzsch and) Gravesend.
57. ——— *hirsutum*, Flints of the W. Ch., near (Delitzsch and) Gravesend.

Of these 57 species, 30 belong to the undoubted chalk and Sicilian marls. The remainder from Oran, Greece (probably Ægina) and Zante, are it is true not from chalk marls geologically determined with equal certainty, but occur with such numerous animals decidedly of the chalk, both calcareous-shelled and siliceous-shelled, that even from these species the geognostic character is more firmly established.

X. *Characters and Descriptions of the Genera and Species of Animals mentioned.*

Although the number of the new genera and species of microscopic organisms has been considerably increased since my last communications, especially by the recent examinations of salt water, yet I have not been able to carry the inquiries necessary for their description so far as to include them all in this memoir, which, besides, would have too much extended it. I have therefore only given the descriptions of the 10 new genera and 42 new species mentioned in the course of this Memoir, together with the characters of the living animals of 21 forms, which hitherto had only been known fossil, and as empty shells. The other forms are already contained in my work on the Infusoria, or in the memoir printed last year on the formation of the chalk.

The 10 new genera and 40 of the new species belong to the class Polygastrica; two species belong to already known genera of the class Bryozoa. Of the 10 new genera only one (*Dinophysis*), and of the 40 species only 2, belong to the family Peridinæa; all the other Polygastrica belong to the family of the Bacillariæ, the rapidly increasing extent of which becomes daily more surprising and important. Both the Bryozoa belong to the order Polythalamia.

A. *Ten new Genera**.

I. AMPHITETRAS.—*Char. gen.* Animal polygastricum e Bacillariorum familia ejusque Naviculaceorum sectione, liberum, lorica simplici bivalvi aut multivalvi (silicea), cubica, imperfecta spontanea divisione in catenas non hiantes rectasque abiens, aperturis in utraque laterali opposita facie quaternis, ad angulos sitis.

Cum *Denticella Biddulphia* et proxime ad *Fragilariam* accedit, a *Staurastro*, cui forma similis, longius recedit.

The genus was first found in the chalk marls, and has since been observed alive in the Cattegat.

II. CERATONEÏS.—*Char. gen.* Animal polygastricum e Bacillariorum familia, ad Naviculacea accedens, liberum, spontanea perfecta divisione geminatum aut solitarium, nunquam cateniforme, lorica simplici prismatica bivalvi silicea, aperturis loricæ duabus oppositis mediis, margine solido rimam longitudinalem utrinque interrumpentibus.

Forma *Naviculæ*, apicibus in cornua longe attenuata productis.

Both known species of this genus have only been observed living in salt water, and are figured on Plate VIII.

III. DINOPHYSIS.—*Char. gen.* Animal polygastricum e Peridinæorum familia, liberum, loricæ (membranaceæ) sulco transverso ciliato et crista media plicata insigne, ocellis carens.

Forma *Vaginicolæ* liberæ, natura *Peridinii*.

It was remarkable that both species were found together with phosphorescent animals in sea water, and are probably themselves phosphorescent, which is likewise indicated by their yellow colour. The genus is not known fossil. Both species are represented on Plate VIII.

IV. EUCAMPIA.—*Char. gen.* Animal polygastricum e familia Bacillariorum, ejusque Desmidiaceorum sectione, liberum, lorica simplici univalvi, complanata, cuneata, in utroque medio latere excisa, divisione spontanea imperfecta in tænias planas articulatas lacunosas curvas, paullatim circulares, abiens.

[* We have not given the detailed descriptions of the genera and species, but merely the diagnoses, as they would have occupied too much of our space, and are too specially zoological for this work.—ED.]

Odontellæ curvatæ *Eucampiæ* formam referunt, sicut *Fragilariæ* curvatæ *Meridii* fere characterem præ se ferunt.

The only known species of this genus is represented on Plate VIII.

V. GRAMMATOPHORA.—*Char. gen.* Animal polygastricum e familia Bacillariorum, sectione Naviculaceorum, liberum (sæpe implexum, nec affixum) lorica simplici bivalvi (silicea) et prismatica, spontanea imperfecta divisione in catenas dehiscentes flexuosas abeunte, Tabellariam æquans, ab eaque dissepimentis duobus internis, tres loculos longitudinales formantibus, diversa.

Tabellariam refert plicis duabus internis siliceis, sæpius ad scripturæ modum flexuosis, insignem.

Five species of this genus are known, of which three have occurred in Oran and one in Greece in a fossil state. All three from Oran have been observed alive in the North Sea, one of them likewise in the Baltic, in the Mediterranean, and on the coast of Peru: the Greek form has been detected alive on the Mexican coast. The fifth species has been found only in the living state from the bay of Mexico.

VI. LITHODESMIUM.—*Char. gen.* Animal polygastricum e Bacillariorum familia, ejusque Desmidiaceorum sectione, liberum, lorica simplici univalvi triangula (silicea), spontanea imperfecta divisione in bacilla rigida triangularia recta abiens.

Characteres *Desmidii*, sed lorica triangula silicea sub cute sponte divisa.

The only species of this form inhabits the North Sea; a representation of it is given on Plate VIII.

VII. PODOSIRA.—*Char. gen.* Animal polygastricum e Bacillariorum familia et Echinelleorum sectione, affixum, lorica pedicellata bivalvi subrotunda (silicea), spontanea imperfecta divisione monilia formans.

Gallionellis affinis formæ pedicellatæ, affixæ.

The only species of this genus is from the Ocean near Callao in Peru. It is the only extra European genus which after careful examination has retained its peculiarity.

VIII. TRICERATIUM.—*Char. gen.* Animal polygastricum e Bacillariorum familia, ejusque Naviculaceorum sectione, liberum, lorica bivalvi triangula (silicea) in utroque latere tridentata vel corniculata, spontanea divisione longitudinali sub cute multiplicatum.

Biddulphiam triquetram aut *Desmidium* siliceum refert.

Three species of the genus are known, of which two have occurred alive in the North Sea, and one only in a fossil state in the chalk marls of Greece. One of the recent species has likewise been found fossil in the same chalk marls. Two species are figured on Plate VIII.

IX. TRIPODISCUS.—*Char. gen.* Animal polygastricum e familia Bacillariorum, ejusque Naviculaceorum sectione, liberum, lorica bivalvi rotunda (silicea) in utroque latere tribus processibus appendiculata, sponte longitudinaliter dividuum.

Coscinodiscus corniculis seu pedicellis utrinque tribus, et *Triceratium* rotundum *Tripodisci* characterem enuntiant.

Only one large species from the North Sea known, which is represented on Plate VII.

X. Zygoceros.—*Char. gen.* Animal polygastricum e Bacillariorum familia et Naviculaceorum sectione, liberum, lorica bivalvi, compressa, naviculari (silicea), utrinque corniculis duobus perforatis insigni, sponte longitudinaliter perfecte dividuum.

Biddulphiam liberam, perfecte sponte dividuam, hinc nunquam concatenatam nec affixam æquat.

Both known species inhabit the North Sea, and are figured on Plate VIII.

B. *New Fossil Species of known Genera, and now for the first time observed Living.*

a. Calcareous-shelled Polythalamia.

1. *Geoponus Stella borealis*, n. sp. Pl. V. fig. *a–g*. G. Testulæ (compositæ) superficie non striata, lævi, foraminibus minimis subtiliter punctata, animalculis (et aperturis frontalibus) aucto sensim numero, vicenis.

   In the North Sea near Cuxhaven, and in the canal of Christiania. Size of the shells $\frac{1}{10} - \frac{1}{4}'''$.

2. *Nonionina germanica*, n. sp. Pl. VI. fig. 1. *a–g*. N. Testulæ (simplicis) superficie non striata, lævi, foraminibus minimis subtiliter punctata, animalculi (unici) apertura sinistra (unica) parva.

   North Sea, on the German coast near Cuxhaven. Size of shell $\frac{1}{20} - \frac{1}{6}'''$.

3. *Planulina ocellata*. Syn. *Rotalia ocellata*, 1838.

   Recent near Cuxhaven; fossil in the chalk near Caltanisetta in Sicily.

4. *Rotalia globulosa*, Pl. VI. fig. 3. Syn. *Rotalia globulosa*, 1838.

   Recent near Cuxhaven in the North Sea; fossil very numerous in the white chalk of Russia (Wolsk), Poland, Prussia, Denmark, England, France, and Sicily; in the chalk marls of Greece, Zante, Sicily, and Oran; likewise in the compact chalk of Egypt and Arabia.

5. *Rotalia Stigma*, Pl. VI. fig. 2. Syn. *Rotalia Stigma*, 1838.

   Recent near Cuxhaven; fossil in the white chalk of Cattolica in Sicily and in the chalk marls of Caltanisetta.

6. *Rotalia turgida*, Pl. VI. fig. 4. Syn. *Planulina turgida*, 1838.
Recent near Cuxhaven; fossil in the white chalk of England, France, Prussia, and Denmark; in the chalk marls of Oran, and in the compact chalk of Egypt and Arabia. Size $\frac{1}{48}'''$.

7. *Textilaria aciculata*, Pl. VI. fig. 5. Syn. *Textularia aciculata*?, 1838.
Recent near Cuxhaven; fossil in the white chalk of Russia, Denmark, England, and Sicily, and in the chalk marls of Greece, and also in the compact chalk of Egypt and Arabia. Size $\frac{1}{30}'''$.

8. *Textilaria globulosa*. Syn. *Textularia globulosa*, 1838.
Recent in the North Sea; fossil in the white chalk of all European countries, from Wolsk to Ireland; likewise in the chalk marls of Sicily, Oran, and Greece, and in the compact chalk of Egypt and Arabia. Size $\frac{1}{36}'''$.

b. INFUSORIA with siliceous loricæ.

9. *Achnanthes pachypus*, Montagne MS. Syn. *Achnanthes brachypus*, Montagne, *Annales des Sci. Nat.* 1837, vol. viii. p. 348. A. bacillis striatis minimis, bis latioribus quam longis aut ovatis, singulis mediis levius inflexis, a dorso apice utroque rotundatis, pedicello brevissimo crasso.
Dr. Montagne first observed this new species on a Conferva from Callao in Peru, which he calls *Conf. allantoides*, brought to Europe by D'Orbigny. Breadth of the bacillæ $\frac{1}{144}'''$.

10. *Actinocyclus biternarius*, n. sp. A. sepimentis carens, disci subtiliter punctati radiis senis.
Fossil in the chalk marl of Oran and Caltanisetta; recent in the North Sea, near the island Tjörn. Diameter $\frac{1}{84} - \frac{1}{48}'''$.

11. *Actinocyclus* (*Actinoptychus*) *senarius*, Pl. VIII. fig. 1. *a–e*. Syn. *Actinocyclus senarius*, 1838.
Fossil very numerous in the chalk marl of Oran, Caltanisetta, and Greece; likewise recent, very numerous in the North Sea near Cuxhaven, Christiania, and Tjörn. Diameter $\frac{1}{96} - \frac{1}{36}'''$.

12. *Actinocyclus septenarius*. Syn. *Actinocyclus septenarius*, 1838. A. sepimentis carens, disci subtiliter punctati radiis septem.
Fossil in the chalk marls of Caltanisetta, Oran, and Zante; living in the Cattegat. Diameter of the discs $\frac{1}{88} - \frac{1}{36}'''$.

13. *Actinocyclus octonarius*. Syn. *Actinocyclus octonarius*, 1838.
Fossil in the chalk marls of Caltanisetta and Oran; recent in the Cattegat. Diameter of discs $\frac{1}{80} - \frac{1}{48}'''$.

14. *Actinocyclus nonarius*, n. sp. A. sepimentis carens, disci subtiliter punctati radiis novem.

Fossil in the chalk marls of Oran; recent in the Cattegat near Tjörn. Diameter $\frac{1}{60}$ — $\frac{1}{34}'''$.

15. *Actinocyclus denarius*. Syn. *Act. denarius*, 1838.

Fossil in the chalk marls of Oran; recent in the Cattegat near Tjörn. Diameter of discs $\frac{1}{60}$ — $\frac{1}{48}'''$.

16. *Actinocyclus undenarius*, n. sp. A. sepimentis carens, disci subtiliter punctati radiis undecim.

Fossil in the chalk marls of Oran and Zante; recent in the Cattegat near Tjörn, and in the bay of Christiania, Norway. Diameter of discs $\frac{1}{48}$ — $\frac{1}{40}'''$.

17. *Actinocyclus bisenarius*, n. sp. A. sepimentis nullis, disci subtiliter punctati radiis duodecim.

Fossil in the chalk marl of Oran; recent in the Cattegat near Tjörn. Diameter of the fossil $\frac{1}{72}'''$, of recent $\frac{1}{43}'''$.

18. *Actinocyclus* (*Actinoptychus*) *duodenarius*, n. sp. A. septis internis in duodecim loculos divisus, disci subtiliter punctati radiis totidem.

Observed only recent in the water of the North Sea near Cuxhaven. Diameter $\frac{1}{48}$ — $\frac{1}{40}'''$.

19. *Actinocyclus quindenarius*, n. sp. A. sepimentis nullis, disci subtiliter punctati radiis quindecim.

Fossil in the chalk marl of Oran; recent near Tjörn. Diameter of the fossil $\frac{1}{48}'''$, of the recent $\frac{1}{48}$ — $\frac{1}{40}'''$.

20. *Actinocyclus* (*Actinoptychus*) *sedenarius*, n. sp. Pl. VIII. fig. 2. A. septis internis in sedecim loculos divisus, disci subtiliter punctati radiis sedecim.

Recent near Cuxhaven. Diameter of the shells $\frac{1}{24}$.'''

21. *Actinocyclus* (*Actinoptychus*) *octodenarius*, n. sp. Pl. VIII. fig. 3. A. septis internis in octodecim loculos divisus, disci subtiliter punctati radiis octodecim.

Recent near Cuxhaven. Diameter $\frac{1}{20}'''$.

22. *Amphitetras antediluviana*, n. sp. A. testula singula cubica ubique cellulosa, cellulis in faciebus lateralibus radiatis, angulis varie productis.

Fossil in the chalk marls of Oran and Greece; recent near Tjörn. Diameter $\frac{1}{72}$ — $\frac{1}{36}'''$.

A few days ago I found in the Greek chalk marls a second species of this genus, which has on the lateral surfaces parallel rows of cells, and may be distinguished as *A. parallela*.

23. *Biddulphia pulchella*, Syn. *Conferva biddulphiana*, Engl. Bot.; *Diatoma biddulphianum*, Agardh.; *Biddulphia pulchella*, Gray; *Biddulphia australis*, Montagne. B. testulæ quadratæ compressæ processibus lateralibus ternis quinisve parvis, obtusis.

Fossil in the Greek chalk marls; recent in the Baltic and North Sea, in the Mediterranean and near Cuba. Diameter $\frac{1}{24}'''$.

24. *Ceratoneïs Closterium*, n. sp. Pl. VIII. fig. 7. C. setacea, cornibus longissimis, singulis corpore medio duplo longioribus et leviter conniventibus lunata.

North Sea near Cuxhaven, and in the Baltic near Wismar. Length of the entire animal $\frac{1}{24} - \frac{1}{18}'''$, of the body without the horns $\frac{1}{96}'''$.

25. *Ceratoneïs Fasciola*, n. sp. Pl. VIII. fig. 6. C. lineari-lanceolata, sigmatoides, cornibus corpore brevioribus, diversa directione curvis.

Recent near Cuxhaven and Tjörn, and near Wismar in the Baltic. Length $\frac{1}{36}'''$.

26. *Cocconeïs oceanica*, n. sp. C. testula elliptica suborbiculari, dorso levissime convexa, exterius lineis concentricis simpliciter curvis exarata, non undulata nec transverse striata.

In the Peruvian Ocean near Callao. Length $\frac{1}{96}'''$.

27. *Coscinodiscus Argus*. Syn. *Coscinodiscus Argus*, 1838. C. testulæ cellulosæ cellulis majoribus, in centro et margine paullo minoribus, ordine radiato sæpius interrupto.

Fossil in the chalk marls of Caltanisetta and Oran; recent near Cuxhaven. Diameter of the fossil $\frac{1}{72} - \frac{1}{24}'''$, of the recent $\frac{1}{48}'''$.

28. *Coscinodiscus eccentricus*, n. sp. C. testulæ cellulis parvis in lineas curvas a centro aversas dispositis.

Fossil in the chalk marl of Oran; recent near Cuxhaven, near Tjörn in the Cattegat, and near Vera Cruz. Diameter $\frac{1}{72} - \frac{1}{36}'''$.

29. *Coscinodiscus lineatus*, Pl. VII. fig. 4. Syn. *Cosc. lineatus*, 1838. C. testulæ cellulis parvis in series transversas parallelas rectas dispositis.

Fossil in the chalk marl of Caltanisetta, recent near Cuxhaven. Diameter of the fossil $\frac{1}{96} - \frac{1}{40}'''$, of the living $\frac{1}{96} - \frac{1}{72}'''$.

30. *Coscinodiscus minor*, Pl. VII. fig. 2. Syn. *Cosc. minor*, 1838. C. testulæ cellulis parvis sparsis, statura minor.

Fossil in the chalk marls of Caltanisetta, Oran, and Zante; recent near Cuxhaven. Diameter $\frac{1}{96}'''$.

31. *Coscinodiscus Oculus Iridis*, n. sp. C. testulæ magnæ cellulis majusculis, radiantibus, in extremo margine et prope centrum minoribus, mediis nonnullis maximis, stellam centralem efficientibus.

Fossil in the chalk marls of Greece, and recent near Cuxhaven. Diameter of disc $\frac{1}{20}'''$.

32. *Coscinodiscus Patina*, Pl. VII. fig. 3. Syn. *Cosc. Patina*, 1838, ex parte. C. testulæ magnæ cellulis mediocriter magnis in lineas circulares concentricas dispositis, ad marginem decrescentibus.

In the chalk marl of Zante, and recent near Cuxhaven. Diameter $\frac{1}{72} - \frac{1}{20}'''$.

33. *Coscinodiscus radiatus*, Pl. VII. fig. 1. Syn. *Cosc. Patina*, 1838, ex parte. C. testulæ magnæ cellulis mediocriter amplis, in lineas e centro radiantes dispositis, ad marginem minoribus.

Fossil exceedingly frequent in Oran, and also in the chalk marls of Caltanisetta and Zante; recent very frequent near Cuxhaven, and in the Baltic near Wismar. Diameter $\frac{1}{72}$ — $\frac{1}{20}'''$.

34. *Dictyocha aculeata*, n. sp. D. cellulis senis in annulum, spinis longioribus sex inæqualibus margine radiatum, conjunctis, calathum utrinque apertum referentibus, ipsisque cellulis singulis intus spinulosis.

Fossil in the chalk marls of Caltanisetta, Oran, Zante, and Greece; recent near Tjörn. Diameter of the fossil $\frac{1}{192}$ — $\frac{1}{96}'''$, of recent $\frac{1}{120}$ — $\frac{1}{96}'''$ (without the spines).

35. *Dictyocha Fibula*, Pl. VIII. fig. 16. Syn. *Dict. Fibula*, 1838. D. cellulis quaternis in formam concavam rhomboidem aut quadratam conjunctis, angulis spinosis.

Fossil in the chalk marls of Oran and Caltanisetta; living in the North Sea near Christiania and Tjörn, and in the Baltic near Wismar. Diameter $\frac{1}{96}$ — $\frac{1}{48}'''$.

36. *Dictyocha* (*Actiniscus*) *Pentasterias*, n. sp. D. cellulis destituta, centro solido concavo, radiis siliceis quinis stellam referentibus.

Fossil in the chalk marls of Greece; recent in the port of Christiania. Diameter $\frac{1}{96}'''$.

37. *Dictyocha* (*Actiniscus*) *Sirius*, n. sp. D. cellulis destituta, centro solido, radiis siliceis sex acutis basi alatis, stellam æmulantibus.

North Sea, near Christiania in Norway. Diameter $\frac{1}{96}'''$.

38. *Dictyocha Speculum*, Pl. VIII. fig. 4. Syn. *Dict. Speculum*, 1838. D. cellulis senis, in annulum, spinis longioribus sex, inæqualibus, margine radiatum conjunctis, calathum utrinque apertum æquantibus ipsisque cellulis intus inermibus.

Fossil in the chalk marls of Caltanisetta, Oran, Zante, and Greece; recent in the Baltic near Kiel, and in the North Sea near Cuxhaven, Christiania, and Tjörn. Diameter $\frac{1}{72}'''$.

39. *Dinophysis acuta*, Pl. VIII. fig. 14. D. lorica ovata urceolata granulosa, fronte quasi operculata plana, loricæ posteriore fine subacuto.

Recent in the Baltic near Kiel. Diameter $\frac{1}{48}'''$.

40. *Dinophysis Michaëlis* (= *D. limbata*), Pl. VIII. fig. 15. D. lorica ovata urceolata granulosa, fronte quasi operculata plana latiore, loricæ posteriore fine rotundato.

Recent in the Baltic near Kiel. Length $\frac{1}{48}'''$.

41. *Eucampia Zodiacus*, Pl. VIII. fig. 8. E. lorica crystallina lævi parum latiore quam longa, ovario dilute flavo.

Recent near Cuxhaven. Diameter of the single animalcules $\frac{1}{96}'''$.

42. *Gallionella sulcata*, Pl. VII. fig. 5. Syn. *G. sulcata*, Infusionsthierchen, 1838.

Recent near Cuxhaven; fossil in the chalk marls of Caltanisetta, Oran, Zante, and Greece.

43. *Grammatophora africana*. Syn. *Navicula africana*, 1838. G. bacillis a dorso quadratis aut oblongis, a latere navicularibus obtusis, plicis internis in quovis dimidio latere tribus undulatis.

Fossil near Oran in chalk marl; recent near the island Tjörn, and near Heligoland in the North Sea. Length $\frac{1}{192}$ — $\frac{1}{40}'''$.

44. *Grammatophora angulosa*. G. bacillis a dorso quadratis aut oblongis a latere navicularibus obtusis, plicis internis in quovis dimidio latere singulis acute angulosis.

Fossil in the chalk marl of Oran; recent near Tjörn. Length of fossil $\frac{1}{76}'''$, of recent $\frac{1}{96}'''$.

45. *Grammatophora mexicana*. G. bacillis a dorso quadrangulis, a latere linearibus apicibus subito constrictis obtusis, plicis internis in quovis dimidio latere mediis rectis prope apicem demum uncinatis.

Among Confervæ near Vera Cruz. Length $\frac{1}{80}'''$.

46. *Grammatophora oceanica*. Syn. *Bacillaria adriatica*, Lobarzewski. G. bacillis a dorso quadrangulis, a latere navicularibus aut linearibus apicibus paullatim decrescentibus obtusis, plicis internis in quovis dimidio latere mediis rectis prope apicem demum uncinatis.

Fossil in the chalk marl of Oran; recent near Callao in Peru, near Vera Cruz in Mexico, near Tjörn in the Cattegat, near Wismar in the Baltic, and in the Mediterranean. Length of fossil $\frac{1}{60}'''$, of recent $\frac{1}{192}$ — $\frac{1}{30}'''$.

47. *Grammatophora undulata*. G. bacillis a dorso quadrangulis, a latere linearibus undulatis quater constrictis obtusis, plicis internis in quovis dimidio latere duabus undulatis.

Fossil in the chalk marls of Greece; recent among Confervæ near Vera Cruz. Length $\frac{1}{72}'''$.

48. *Haliomma? radians*. H. articulis exterius non discretis, forma globosa et subovata foraminosa silicea, cellulis undique e nucleo medio obscuro radiantibus.

Fossil in the Greek chalk marls; recent near Cuxhaven. Diameter $\frac{1}{48}'''$.

49. *Lithodesmium undulatum*, Pl. VIII. fig. 13. L. corpusculis concatenatis continuis maxime pellucidis lævibus, lateribus duobus undulatis, tertio plano ejusque margine bis exciso, angulis obtusis.

Recent near Cuxhaven. Length $\frac{1}{40}'''$.

50. *Navicula (Pinnularia) Didymus*. N. testula striata a latere lineari integra utrinque truncata, a dorso media constricta utroque fine

suborbicularis, hinc tanquam discis duobus coalitis constans, striis in centesima lineæ parte viginti tribus.

Fossil in the chalk marl of Caltanisetta; recent near Cuxhaven and Wismar. Length $\frac{1}{96}-\frac{1}{40}'''$.

51. *Navicula (Pinnularia) Entomon.* N. testula striata ab utraque facie media constricta, apiculo laterali medio nullo, pinnulis (striis) latioribus.

Recent in the port of Christiania; fossil in the Greek marls. Length of the recent $\frac{1}{36}'''$, of the fossil $\frac{1}{24}'''$.

52. *Navicula (Surirella) Folium.* N. testula ovata ampla obtusa turgida parumper compressa, media apertura nulla, pinnis angustis in vicesima quarta lineæ parte 24.

Recent in the bay of Christiania, and near Tjörn on the coast of Gothland. Length $\frac{1}{43}'''$.

53. *Navicula (Surirella) Gemma,* Pl. VIII. fig. 5. N. testula ovato-oblonga ampla magna turgida, media apertura nulla, pinnis gracilibus in vicesima quarta lineæ parte 16.

Mouth of the Elbe near Cuxhaven. Size $\frac{1}{24}-\frac{1}{18}'''$.

54. *Navicula (Pinnularia) norwegica.* N. testula a latere lineali angusta utrinque truncata, a dorso late ovata utrinque acuta, marginis limbo anguste striato, area ampla media lævi.

Recent in the port of Christiania. Length $\frac{1}{30}'''$.

55. *Navicula (Pinnularia) quadrifasciata.* N. testula a latere lineari angusta truncata, a dorso late ovata utrinque acuta, marginibus limbo lato et tænia duplici media longitudinali angustiore striatis.

Recent in the port of Christiania and near Tjörn; fossil in the Greek marls of the chalk. Size $\frac{1}{36}'''$.

56. *Peridinium pyrophorum.* Syn. *Per. pyrophorum,* 1836. Infus. 1838.

Recent near Kiel; fossil in the flints of Delitzsch, Gravesend, &c. Size of the recent $\frac{1}{48}'''$.

57. *Podosira nummuloides.* Syn. *Trochiscia moniliformis,* Montagne, 1837. *Melosira hormoides,* Mont. MS. 1838. P. corpusculis globosis a latere leviter compressis discretis subtilissime punctatis, ovariis vesiculosis virentibus, pedicello corporis diametro breviore.

Recent on *Polysiphonia dendroidea,* Montagne, near Callao in Peru. Size $\frac{1}{72}'''$.

58. *Triceratium Favus,* Pl. VIII. fig. 10. T. testulæ lateribus triquetris planis aut leviter convexis, angulis obtusioribus superficie cellulis sex angulis magnis favosa, dorsi angulo medio lævi.

Recent near Cuxhaven; fossil in the chalk marls of Greece. Diameter $\frac{1}{20}'''$.

59. *Triceratium striolatum*, Pl. VIII. fig. 9. T. testulæ lateribus triquetris convexis, angulis subacutis, superficie subtilissime punctato-lineata, dorso cingulo medio lævi.
Recent near Cuxhaven. Size $\frac{1}{24}'''$.

60. *Tripodiscus Argus*, Pl. VII. fig. 6. T. testulæ magnæ orbicularis compressæ valvulis leviter convexis, cellularum in series radiantes dispositarum margine, interdum et interstitiis, punctatis asperis, processibus lateralibus brevibus hyalinis.
Recent near Cuxhaven. Diameter $\frac{1}{18}'''$.

61. *Zygoceros Rhombus*, Pl. VIII. fig. 11. Z. major, testula turgida a latere rhomboide angulis rotundatis, superficie subtilissime striata et granulosa, dorsi angulo medio lævi.
Recent near Cuxhaven. Diameter $\frac{1}{24}'''$.

62. *Zygoceros Surirella*, Pl. VIII. fig. 12. Z. minor, testula compressiore a latere lanceolata, apicibus constrictis obtusis, superficiei lineis granulatis in medio conniventibus magis distinctis, dorsi cingulo lævi latiore.
Recent near Cuxhaven. Size $\frac{1}{60}'''$.

XI. *Summary of Results considered with relation to the present state of our knowledge.*

It should be the endeavour of one who has collected new facts not merely to bring them accurately and clearly under review, and to compare them according to his own idea of them with the existing state of science, but also to elucidate the conclusions which directly and necessarily result from them. This additional task is generally very difficult, sometimes leading to the discovery that what had been supposed new was not so, or not of sufficient importance for such an extended investigation; sometimes rendering a fresh and more profound examination requisite, or misleading to an evident exaggeration of the facts discovered and to conclusions which they do not justify, and is therefore very frequently avoided from motives of fear or convenience. To leave this to others lightens indeed the labour of the task; but this alleviation at the same time lessens the value of isolated observations, and throws a doubt upon the care employed in making the comparison.

Along with the general view of the facts advanced, I have also aimed at forming comparisons and conclusions, not in order to veil any erroneous view of the facts, but to render it the more conspicuous where it had gained ground; and on the other hand to render the truth discovered more striking, and thus to awaken a more general and active interest for this kind of inquiry. I shall only draw such conclusions as are most obvious; since the further we depart from actual observation, the more we deviate into the field of uncertain speculation, which when constructive, instead of being completive merely, becomes the very opposite of philosophical inquiry, and is just as feasible for anybody as for the philosopher himself. I would desire therefore that my conclusions should always be less, rather than more, than the observations might warrant me to draw.

1. There are numerous animals of the chalk or secondary formation of the earth which are still found living, and precisely such as do not, either from great variation of form within generic limits, or from the simplicity of their exterior, leave any uncertainty in determining their specific difference.

2. Of the animal forms which constitute the greater mass of the white chalk, those which preponderate in number of individuals are identical with living species; and hitherto all the principal species which form the rocks have been observed alive even in the short time during which the inquiry has been proceeding.

3. The principal number of species, and the great mass of individuals of these recent forms, are microscopic Infusoria and calcareous-shelled Polythalamia, scarcely or not at all perceptible to the naked eye, which nevertheless form so incalculably great a volume of the solid portion of the earth, that the few species asserted to be still living, from other groups of animals of higher organization, even if they were all decidedly identical, bear not the slightest comparison with the number and mass.

4. The microscopic organisms are, it is true, far inferior in individual energy to lions and elephants; but in their united influences they appear far more important than all these animals.

5. The fifty-seven recent species of the chalk in Europe, Africa and Asia do not live solely or principally in southern latitudes, as has been shown with respect to the recent larger forms of the so-called Eocene formation, but have been observed living both in those and in northern latitudes. These recent species also are not rare nor isolated, but fill in incalculable numbers the seas of northern Europe, and are not wanting on the tropical coasts of the American ocean.

6. The idea that the temperature and constitution of the atmosphere and oceans were essentially different at the period of the chalk formation, and adverse to the organized beings at present existing, naturally acquired more probability and weight

the more decidedly different all the creatures of that period were from those of the present time; but loses more and more in importance the less the chalk proves to be a chemical precipitate, and the more numerous the forms agreeing with those of the present day become by renewed inquiry Nay, there is not the least doubt that the perfectly ascertained identity of a single species of the present day with one of those of the chalk, renders doubtful the necessary transformation of all the others subsequently to the formation of the chalk rocks; how much more so when these are numerous, and such as form masses! The size appears to be of no importance, as the small organisms have already been shown to agree with the large with regard to the effect of external influences upon them.

7. The period of the dawn of the organic creation coexistent with ourselves, can only be admitted as being anterior to and below the chalk formation, if indeed, which is questionable, such a distinction can be made; or the chalk, with its rocks, covering far and high the superficies of the earth, forms part of the series of recent formations, and, since of the four as yet well-established great geological periods of the earth's formation, the quaternary, tertiary and secondary formations contain recent organisms, it is as three to one more probable that the transition or primary formation is not differently circumstanced, but that, from the gradual longer chemical decomposition and change of many of its organic relations, it is more difficult to examine and determine.

Paludina vivipara and *Cyclas cornea* of the Weald clay, and the recent *Trochus* below the chalk, according to Defrance, as well as the confirmation of the occurrence of *Terebratula Caput Serpentis* in the upper Jura by Von Buch, together with my observations of microscopic yet nevertheless peculiar Polythalamia in the flints of the Jura, are additional positive indications of the inconceivable extent of similar organic relations, the further investigation of which is one of the important questions to be determined in the present age.

8. It cannot be denied that the notion hitherto frequently asserted, that all recent organisms, including man, are the descendants and perfected stages of metamorphoses of Trilobites and Ferns, has something in it opposed to sound sense; when therefore the direct inquiry leads powerfully to a different point of view it has much in its favour, even though it be reserved to a future period to explain the vast connexion of the phænomena.

9. Since now Polythalamia, and other forms identical with chalk animals, exist which are not endowed with spontaneous division, this faculty of the Infusoria, and their general nature, are not the sole causes to which the indefinite duration of the species is owing.

10. In consequence of the mass-building Infusoria and Polythalamia, the secondary formations can now no longer be distinguished from the tertiary; and in accordance with what has been above stated, masses of rock might be formed even at the present time in the ocean, and be raised by volcanic power above the surface, the great mass of which would, as to its constituents, perfectly resemble the chalk. Thus then the chalk remains still to be distinguished by its organic contents as a geological formation, but no longer as a species of rock.

11. The power so conspicuous in the organic beings under consideration is, according to experience, so immensely great, even in its influence on the inorganic, that with the concurrence of favourable circumstances they alone might give rise to the greatest changes in the distribution of the solid of the earth in the shortest space of time, especially in the water; and the ascertainable extent of such influences, however great, remains constantly small in comparison to those that are possible, consequently do not give by their magnitude any certain measure of periods of time.

12. The correctness of the above expositions is not founded on individual opinion formed from hasty inspections of petty objects; but the microscopic objects on which the opinions are based (though fading from our notice as individuals, yet by their number forming mountains and countries) are accessible to any comparison in distinct preparations, made according to the methods already described; and almost all the forms here mentioned, especially all the more important ones, have been carefully preserved by me, and laid before the Academy.

13. *Thus then there is a chain, which though in the individual it be microscopic, yet in the mass a mighty one, connecting the organic life of distant ages of the earth, and proving that it is not always the smaller or most deeply lying which is the base and the type of those which are larger and nearer the surface on our earth; and moreover, that the dawn of the organic nature coexistent with us, reaches further back into the history of the earth than had hitherto appeared.*

EXPLANATION OF THE PLATES.

The four Plates annexed to this Memoir, containing all the living microscopic marine animals that have not been previously figured, magnified 300 times in diameter, are moreover intended to afford three views of a more general nature.

In the first place, they will give a more extended view of hitherto doubtful and unknown organic beings, of numerous living native Polythalamia, while the structural relations previously communicated in the Memoir on the formation of Chalk were taken from dead animals, and, with the exception of a single foreign species, only illustrated the dead structure, not the living form.

Secondly, they will place in view now living forms of calcareous-shelled and siliceous-shelled animalcules, which can be regarded in no other light than as identical with those species that form in masses the chalk and chalk marl of the secondary formation of the earth.

And thirdly, they will give an idea of a portion of the rich booty yielded by a single pail-full of sea water in microscopic and hitherto quite unknown forms, almost all of great importance in geological inquiries.

The two first Plates contain only calcareous-shelled animalcules, *Polythalamia*; the other two only siliceous-shelled animalcules, *Infusoria.*

The Plates were executed at a time when only thirteen living species of chalk animalcules were known, and contain therefore only these.

PLATE V.

The first and second Plates contain six species of native and recent calcareous-shelled animalcules belonging to the group Polythalamia, of which four species appear to be identical with those forming the mass of the chalk.

The whole of the first Plate is occupied by the representation of *Geoponus Stella borealis*, as representative of the family of the *Helicotrochina*, in the living state, with its organic detail. It has been found alive only near Cuxhaven, and is not known from the chalk.

The great similarity of this composite animal to the single-bodied animal of the *Nonionina*, represented on the second Plate, is highly remarkable.

Fig. *a.* The creature magnified 290 times, with numerous projected very delicate locomotive organs whilst in motion, and in the greatest observed development and projection of the soft parts from the shell.

At first view it will be seen that the calcareous shell of the animalcule is no interior part, like the dorsal plate of the naked *Sepiæ*, but is an external envelope like a snail's shell. From the character of the septa which divide the inner space of the shell into several chambers, results further an important distinction from all molluscous shells; but a great similarity with the many-chambered house of the *Nautilus* and similar shell-bearing *Sepiadæ* is evident.

On the other hand, an important distinction in the formation of these minute Polythalamia and of the many-chambered house of the *Nautilus* will be immediately recognised; in the microscopic animalcule the body fills distinctly all the single cells, the four front ones with transparent colourless, the hinder ones with less transparent coloured parts; while in the true Nautiloidea all the posterior compartments are, as successively quitted dwellings, empty, and the first front one alone receives the entire animal.

α. The siphons or tubes of connexion of the individual animals in the shell.

β. The filamentoid cirrhi, which may be lengthened, shortened or ramified at will, effecting locomotion, and which are still more local organs than the pseudopodia of the *Amœbæ* or *Arcellinæ* among Infusoria.

δ. Bands of aggregated large apertures near the septa of the chambers.

ε. Numerous small apertures over the entire surface of the shell penetrated by exceedingly minute tubes.

Fig. *b*. A similar animal, magnified 100 times, from the left side. The four anterior cells are likewise filled with colourless, the remainder with coloured parts, which cannot optically be distinctly separated. However, in all the figures the dark portions of the coloured body are granular, and belong probably to the ovarium, while the fainter coloured appear to belong to the organs of nutrition.

Fig. *c*. The same creature from the right side.

Fig. *d*. The same, front view, on the narrow side.

α. The anterior apertures of the individual animals.

Fig. *e*. A similar creature, seen from the right side, after removal of the calcareous shell by means of weak acid. It is remarkable, that the first cell, in the natural state apparently empty, likewise exhibits as its contents a gelatine quite solid, and somewhat condensed by the acid.

γ. The conoid connecting parts of the chambers, the true siphons, of which the sheaths designated by *α* in fig. *a* are those of the shell.

Fig. *f*. The contents of a single chamber (front view) separated from the other parts by too violent a removal of the shell by a somewhat strong acid. It may be compared with the anterior part of fig. *d*. The connecting parts of the single chambers *γ γ γ* appear like dentoid cones, each of which marks the connexion or the termination of an individual.

Fig. *g*. The similar dissolved chamber, which consists of several adherent or not perfectly separated animals, each of which has its conoid embouchure or connecting tube with the other joints at *γ γ γ*: side view.

PLATE VI.

This Plate contains five native (German) recent calcareous-shelled Polythalamia, represented in the living state, of which four likewise occur in the chalk in such quantity that they belong to those forming the masses. The first and largest form has however not yet been observed in the chalk.

Especially remarkable is the formation of externally affixed ovaria in the first form, and the presence of external organs of locomotion, as well as of distinct internal recipient nutritive organs, and of evident organs of generation.

Fig. 1. *Nonionina germanica*, from the North Sea near Cuxhaven, as representative of the organic relations in the family of the *Rotalina* of the Polythalamia, with its projecting locomotive organs, its body enclosed in the shell and contemporaneously filling all the chambers, without exsertile head and without tentacula, magnified 290 times.

This form, although very similar externally to that on Plate V., is, as it seems, totally distinct from it in its entire formation and import. The first form was a composite animal or polypidom of from eighteen to twenty animalcules, which, closely connected laterally with each other by as many connecting parts of the chambers, formed apparently an inseparable regular whole, similar to a *Nautilus*. This second form is only a single animal, which represents a whole, quite similar to the conglomerated one, only simple. The former is a corallidom, this a coral individual. This view is evidently proved by the simple connecting part of the chambers situated to the left.

The structure and internal organization of this more simple form is far more easily seen than that of *Geoponus*.

a. A living animalcule in the act of creeping, seen from the right side. The soft, transparent, large, very contractile pseudopodia, difficult to be observed, are frequently longer than the diameter of the shell. The first chamber of the shell is colourless, but entirely filled with a crystalline, transparent, soft body-part; all the chambers, beginning with the second, contain besides this a patch of granular mass of brownish-yellow colour. The whole surface of the shell is pierced with very minute pores, which form as many outlets of minute tubes of the shell.

b. A similar animal, magnified 100 times, with numerous exserted pseudopodia.

c. The same, seen from the left side, with numerous developed locomotive organs.

d. An outline view of the same animal in front.

e. A similar animal, after removal of the shell by weak hydrochloric acid. The soft residue is the peculiar body which extends even to the first apparently empty cell. The parts performing the functions of nutritive canal now become evident from their being filled with siliceous-shielded *Naviculæ*. Nearly as far as the umbilicus of the spiral there are apparent in the separate chambers swallowed Infusoria of the family *Bacillariæ*; and the simple connecting tube of the single chambers indicates the only way by which these solid parts can have gradually advanced into the otherwise everywhere closed interior. The intestinal canal therefore appears as an articulated wide tube, the joints of which are connected by narrow intermediate parts.

In the interior of the joints near the wide nutritive organ, and outside of it, beginning with the second chamber, is situated a parcel of granular yellow mass, which therefore cannot be received nutriment, and consequently must be regarded as an ovarium provided with coloured eggs, especially considering the surprising increase of these animalcules, and that the organs for these functions cannot therefore be hidden. The entire surface of the soft body freed from its shell is shagreened, and these minute projections correspond distinctly with the fine tubes and pores of the shell, and are therefore undoubtedly the retracted pseudopodia or cirrhi.

f. A single joint of the body, detached by violently removing tne shell with too concentrated an acid. It corresponds to the contents of one chamber, side position, and with empty shells of *Bacillariæ* in the interior.

α to *β*. The space occupied by the nutritive canal. Between *β* and *γ* is situated the ovarium, near the intestinal canal.

γ. The connecting canal of the members or sipho, in place of which in the first joint the mouth aperture is situated.

δ. A space on each side, in which other very transparent parts of the organism are probably situated, but which hitherto have not been seen.

g. An animal with its shell, and an affixed petiolated ovarium. This state has been thrice observed on different individuals; each time, however, the sac had been already emptied, and exhibited large torn apertures both in front and behind. One had two apertures in front, and a third behind. In one were granules, which appeared like bad eggs. It appears therefore that in each such sac a number of eggs occur. The form, mode, and place of adhesion were alike in the three cases. I have also recently detected similar sacs in *Geoponus*.

Fig. 2. *Rotalia Stigma* (not *perforata*), recent from the North Sea near Cuxhaven, in two specimens, magnified 300 times.

The name *R. perforata*, engraved on the plate instead of *R. Stigma*, is an error, which should also be corrected in pages 349 and 353 of the text. Both perforated and nearly allied fossil species have already been figured on pl. iv. to my Memoir on the Chalk of 1838, and may be easily compared. The apparent differences of the present and of the former two drawings are, I am convinced, not attributable to different specific characters. Other forms, occurring in the marls of Caltanisetta, are more and more closely related to the recent, and evidently belong to the same species; the fluctuation in the formation of which will be apparent from the two individuals here represented; and a comparison of the three representations there given of different individuals of the true *perforata* from Caltanisetta, Gravesend, and Denmark, exhibit fluctuations in the formation. The large cells in connexion with minute apertures have principally guided me in determining the species.

a. A larger specimen, seen from the left side, and has eight joints or chambers.

b. A smaller one, from the same side, with only four joints.

c. The same, from the right side.

The ovarium surrounding the intestinal canal alone, it seems, colours all the cells, beginning with the second.

Fig. 3. *Rotalia globulosa*, recent near Cuxhaven, magnified 290 times in diameter.

a. Viewed from the right side.

b. Viewed from the left side.

The ovarium again appears to be the principal organ, covering the intestinal canal, and all the other inner parts, with its brown-yellow mass, beginning with the second cell.

Fig. 4. *Rotalia turgida* (*Planulina? turgida*), from the same locality, and equally magnified.

a. Viewed from the right side. The colouring of all the cells, beginning with the second, by the prevalent ovarium, is likewise here evident.

b. Outline view of the narrow surface in front.

Fig. 5. *Textilaria aciculata*, in similar circumstances as the former, recent, and magnified to the same extent.

a. Side view from the left.

b. View of the narrow ridge in front, in outline.

The two first chambers are, as in the *Rotaliæ*. the first only, and in *Geoponus* the four first, filled with colourless parts; the others are characterized by their coloured contents, which perfectly resemble the ovarium surrounding the intestinal canal of the other forms.

PLATE VII.

This and the following Plate contain twenty-two species of siliceous-shelled recent Infusoria of the Ocean, of which nine species have been recognised to be perfectly identical with the forms which constitute, by their innumerable quantity, the chalk marls of Sicily, Oran, Zante, and Greece.

This third Plate contains, among six allied forms of the present world, five forms which likewise belong to the secondary formation (fig. 1 to 5).

Fig. 1. *Coscinodiscus radiatus*, the principal form of the polishing slate of Oran and of the chalk marl of Caltanisetta, recent near Cuxhaven, magnified 300 times.

a. Broad lateral view. The cells are most directed in alternating rows, radiately towards the centre; at the margin smaller, and in the centre itself homogeneous and irregularly aggregated. In the centre of the disc is situated in the interior a green granulate body, which is comparable to the ovarium of the *Gallionellæ*. Marginal apertures are indicated in fig. *d*.

b. The same, in outline, half turned.

c. The same, from the narrow side, where it is evident how each two plano-convex shells form a single coin-like animal, as in *Gallionella*.

d. A young specimen.

It is to be observed, that the two specimens here figured are extremes of the variation in form, the large one by its large, the small one by its small acute-radiate cells. The first approaches closely to *C. argus*. The natural specimens exhibit, by the arrangement of the cells, curved lines varying on changing the light, which could not be represented in the drawing.

Fig. 2. *Coscinodiscus minor*, a very frequent form of the chalk marl of Sicily, living, from Cuxhaven, magnified 300 times.

a. Broad side.

b. Front view of the narrow surface.

c. Half turned (reverted).

In the interior the ovarium is evident as a green mass. The remainder of the inner soft body is transparent.

Fig. 3. *Coscinodiscus patina*, a form occurring more rarely in the chalk marls and polishing slates of the chalk, recent near Cuxhaven.

a, b, c. Discs of various sizes, seen from the broad surface. In fig. *b* the coloured brownish-yellow ovarium is evident.

c. The form *b* seen from its narrow surface, in which it is evident that it was an animal in the act of longitudinal division, and that two ovaria are situated behind each other.

d. The same, half reverted, in outline.

e. A smaller disc.

f. Young healthy state of the same animal, with fully developed ovarium as yellow numerous globules, then at *t* with a glandular body which may be compared to the male sexual gland, and at *v* with a large contractile vesicle in the interior.

g. The same, from its narrow side.

h. The same, half reverted, in outline.

Fig. 4. *Coscinodiscus lineatus*, living with the former, and occurring frequently fossil.

a. Empty shells, with their characteristic straight-lined arrangement of cells.

b. Narrow surface of the same, in the act of spontaneous division, consequently double animalcule.

d. Half reverted double animalcule. These three with their yellow ovarium quite similar to that of the *Gallionellæ*.

e. Narrow surface of another individual preparing to divide, with green contracted ovarium.

Fig. 5. *Gallionella sulcata*, already figured as fossil form of the chalk marl of Oran in my large work on Infusoria, but here represented as living from Cuxhaven.

a. An eleven-jointed chain of as many animalcules, of which several are filled with the ovarium of brownish yellow colour, some are empty.

b. A single animalcule from the broad side.

c. The same, half reverted.

d. Another chain of 7½ joints, with contracted ovarium of greenish colour.

e. The broad lateral surface.

Fig. 6. *Tripodiscus germanicus*, a recent form from Cuxhaven, not known in the fossil state, which belongs to a peculiar genus.

a. Half reverted side view.

b. Broad lateral surface with three bright spots.

c. Narrow ventral surface.

In the interior of all the figures the ovarium is evident as a green mass.

The horns, three on each side, and evident in all, constitute the generic character, and connect this round form very closely with the apparently much deviating trilateral *Triceratium* of the following Plate.

PLATE VIII.

Sixteen forms of siliceous-shelled Infusoria from the Baltic and North Sea are here represented, of which four are perfectly identical with those met with generally in vast numbers in the chalk marls. These are the remarkably formed *Actinocyclus senarius*, *Dictyocha Fibula*, *Dict. Speculum*, and *Triceratium Favus*.

The other forms represent but a small part of the riches of the pail of sea-water collected at Cuxhaven, in which the above also occurred.

Fig. 1. *Actinocyclus senarius*. The fossil shells from Oran were represented already in 1837, in my work on Infusoria, pl. xxi. fig. 6: here the living animal is figured.

a. View of the narrow ventral surface.

b. The same animal from the broad lateral surface.

c. Empty shells of a dead animalcule.

d. Broad lateral surface of another specimen.

e. The same half reverted, in which the central diagonal line, which has remained somewhat too light, is to be regarded as reflexion of light, not as a septum.

The polypartite ovarium is easily distinguishable from its partly more yellow, in part more green colour. In empty shells three spaces (fields) appear to be situated alternately deeper.

Fig. 2. *Antinocyclus sedenarius*, known only recent from Cuxhaven, has 16 rays and as many chambers; but the septa of the chambers form as many again, therefore 32 rays. The structure of these forms is, as long as they are filled with the living animal, indistinct, but very evident in empty shells. At the margin are 16 apertures corresponding to the septa of the chambers.

a. Empty shells.

b. Shell filled with the animal. The ovarium is greenish. The organic contents rendered the determination of the number of rays and the number of marginal apertures, which appeared to be far greater, difficult.

c. View of the narrow side.

Fig. 3. *Actinocyclus octodenarius*, likewise known only recent, with 18 rays and chambers.

a. Narrow ventral surface.

b. Broad lateral surface, with the green-coloured ovarium grouped in the circle.

c. Empty shell of the same.

Fig. 4. *Dictyocha Speculum*. Fossil very frequent in the chalk marl of Caltanisetta and Oran, and found living near Kiel and Cuxhaven. The fossil form, but a thick-celled variety, was figured in my Memoir on the Chalk. There are in those marls far more frequent forms which perfectly resemble the living here represented.

a. The animalcule from the broad side, coloured green from the ovarium with which it is filled after the first observation and drawing made in Kiel.

b. The same, from the narrow surface.

c, *d*, *e*. Three other individuals with emptied ovaria, with larger or smaller, more elongate or rounded central cell, and with longer or shorter marginal spines. The forms *d* and *e* are perfectly identical with the fossil specimens from Sicily, both in size and in form.

Fig. 5. *Navicula* (*Surirella*) *Gemma*. Compare Section V. This form of large Navicula, observed only in the recent state from the North Sea, has admitted the following details to be recognised, which

assist to establish more and more firmly the animal nature of these creatures:—

a. View of the side surface β β β cirrhi.

b. View of the ventral or dorsal surface; *a a*, probably two apertures.

c. View of the expanded ventral surface when preparing for spontaneous division.

d. Smaller and younger specimen from the side.

e. The same from the ventral or dorsal surface.

In all these forms, very minute, slowly moveable, retractile cirrhi projected at the sides, perceptible but with great trouble on account of their transparency, and at the extremities of the bands or pinnæ appeared to be apertures. The radiation of the cirrhi could be traced to the centre of the inner body (fig. *a*, *b*, *d*). At the thick extremity appeared to exist near *a* two larger apertures (for the mouth and the eggs?). Locomotion by means of the cirrhi was very distinct, and brought to mind the feet of the Sea-stars.

The green colouring in the interior filled two very minute curved laminæ, around which were numerous vesicles of various sizes.

In the dorsal and ventral view could be distinguished a saddle-formed very transparent body in the centre, which extended with two arms to the anterior two apertures and surrounded these. Of these places (*a a*, fig. *b*) probably one was the mouth aperture, the other the aperture for the ovarium.

In fig. 5 *a*. were observed three still larger (glandular?) transparent bodies, which might easily be sexual glands.

In fig. 5 *c*. is the ovarium contracted out of its two-folded laminæ into two agglomerated masses, and the very transparent saddle-formed body was not perceptible.

Fig. 6. *Ceratoneïs Fasciola*, a form not known as fossil, but numerous in the sea near Cuxhaven.

a. Ventral or dorsal surface.

b. Another similar form.

c. Narrow lateral surface of the same species.

The two vesicles of fig. *b*, situated near the beak, appear to be comparable to the sexual glands of the *Naviculæ*, the 4 to 6 vesicles in the centre round the mouth aperture might be ventral cells. In fig. *a* the circular part appears to be comparable to a closing muscle around the mouth, as in fig. 5 *b*, *a*. The interior green parts are the egg-plates.

Fig. 7. *Ceratoneïs Closterium*, living near Cuxhaven, not known fossil.

a. Ventral or dorsal surface with yellowish ovaria.

b. The same view of another specimen with greenish ovaria.

c. Somewhat narrower lateral surface of fig. *a*, without central line.

Fig. 8. *Eucampia Zodiacus*, from Cuxhaven.

a. A gradually-grown chain-like curve of eight individuals by imperfect self-division, with the tendency to further increase in a circular spiral.

b. Two connected individuals.

c. The peculiar form of an individual from its broad side.

The yellow agglomerated ovarium contains larger vesicles, perhaps large gastric cells. From fig. *c* it is evident that the ovarium originally forms in the young state a manifoldly divided mass, which is probably similar to that of fig. 12 *a*, and of fig. 1 *d*.

Fig. 9. *Triceratium striolatum*, from Cuxhaven.

a. Dorsal or ventral surface.

b. Half reverted from the side.

c. Side surface of the same.

The brownish-yellow ovarium presents itself everywhere divided into numerous parts.

Fig. 10. *Triceratium Favus*, likewise from Cuxhaven.

a. Side view.

b. Ventral or dorsal surface.

Fig. 11. *Zygoceros Rhombus*, from Cuxhaven.

a. Side surface.

b. Dorsal or ventral surface.

Fig. 12. *Zygoceros Surirella*, occurring with the previous species.

a. Side view.

b. Dorsal or ventral surface.

The still young state of the ovarium is apparent in this form in its natural expanded state.

Fig. 13. *Lithodesmium undulatum*, from Cuxhaven.

a. A chain-bar of four individuals, seen from the broad basal side slightly reverted, in which the two incisions at the lateral margin are most distinct; they probably lead to apertures.

b. Two chain-joints seen from above, where the striping and indentation of the two upper surfaces are evident. The green ovarium is in both, although agglomerated, yet still swelled.

c. A side view of a single individual.

Fig. 14. *Dinophysis acuta*, from Kiel, a probably phosphorescent sea animalcule, seen from the side, characteristic from the circle of cilia situated close to the flat front in a groove and by the lateral frill. The cilia could not be so distinctly seen that they could be represented, but their vibratory action was evident, as in the *Peridinii*.

Fig. 15. *Dinophysis Michaëlis* (not *limbata*), likewise observed and drawn in Kiel.

Fig. 16. *Dictyocha Fibula*, after one of several specimens which have been observed in Berlin with many other recent and living Infusoria in the sea-water from Christiania. It is evidently the same creature which occurs so frequently in the chalk marl of Caltanisetta.

Article XIV.

An Inquiry into the Cause of the Electric Phænomena of the Atmosphere, and on the Means of collecting their Manifestations. By M. A. Peltier.

[From the *Annales de Chimie et de Physique*, Avril, 1842.]

Introduction.

1. THE different branches of the physical sciences which constitute meteorology are composed of observations very difficult to be collected, by reason of their number and of the fixed hours which they often require; therefore observers have divided the work amongst themselves according to their taste, their locality or their particular position. The great number of meteorologists who are occupied with these questions have brought forward those masses of magnetic, barometric, thermometric, hygrometric and aërian observations, which together form the immense collections under the burden of which those who wish to reduce them and obtain from them useful laws are overwhelmed.

2. When we consider the observations of the aqueous and igneous meteors unprejudiced and without a preconceived theory, we are surprised at the unsatisfactory nature of the explanations which are given of them, and at the blanks which have only been filled up by hypothetical and often contradictory assertions. All that relates to electric phænomena appears still to belong to the infancy of science, since we are obliged to have recourse to the creation of substances endued with forces, instead of attributing meteors to a general cause which governs all natural phænomena. This also proves, and for the thousandth time, how difficult it is to divest ourselves of anterior ideas which we have been in the uncontrolled habit of using, and which have served to form the scientific language which we use.

3. There are three difficulties to overcome, when we wish to reconsider an ancient science the theory of which has been generally received. The first is to bring into doubt the received ideas respecting the cause of the phænomena; the second is the language which they have produced and which must be changed; the third, in short, is to present a new theory which answers to all the known phænomena; a theory which will always be re-

ceived with repugnance, whatever its merits may be, because it forces upon us an effort of the mind, which the faith we had in the affirmations of the ancient theory relieved us from.

4. In a special work, I will state why I can neither admit the two fluids of Dufay nor Franklin's single fluid. I will then show that the cause of the electric phænomena is like those of light and heat, a modification of the universal fluid which fills space. I will indicate what is the modification which it receives, in order to produce these new manifestations; a modification quite different from those of light and heat; then only shall I be able to propose a nomenclature appropriate to the true causes; but till then I cannot dispense with the use of the customary language which has been created for theories which everywhere totter, and which have no support in facts. In the infancy of a science, as many forces are created as there are phænomena to explain, as in mechanics a motive force was given to each effect which we desired to produce; but progress in the one science as well as the other, shows this multiplication of forces to be unnecessary, and reduces the different effects to a single force. The terms *vitreous* and *resinous*, *positive* and *negative*, which I am still obliged to use, do not present to my mind any of the meanings which the theories from which they have originated attribute to them; they will have, in my opinion, no other value than that of indicating the different degrees of the same state, setting out from a point of equilibrium, deprived of electric manifestation. This is not the time to prove in detail why I consider the *resinous state* as the real electric phænomenon, and why the *vitreous state* is but the absence or diminution of it. Without entering into these considerations in the present memoir, we shall see that all the phænomena which will occupy our attention agree in this, that by admitting this assertion the natural phænomena arrange and transform themselves without effort. We shall employ the words *resinous* and *vitreous* in preference, as the least significant, meaning by the first a *more resinous* tension than the point of equilibrium, and by the other a *less resinous* tension than that which this same point possessed.

5. I will again mention what I have already said several times; it is that our instruments for measuring electricity only indicate the differences of the same condition, and not contrary conditions, nor absolute quantities. Their function is limited to tell,

that setting out from the point where an instrument, an electroscope for example, has been in a state of equilibrium, (or, let us say, that setting out from the point where the stem has been put into communication with the armatures and the plate, and the electric state in the stem has afterwards been increased or diminished,) it is what it was before, *plus* what has been added to it, or *minus* what has been taken away from it. The electric sign will then depend on the starting-point of the instrument, on a point of equilibrium which may vary excessively, and which as much exists at the centre of an atmosphere at its maximum of *resinous* tension as at the minimum of the same tension.

6. In the course of this memoir I shall frequently speak of the gold leaf electroscope, although it is not the instrument which I employ; it has neither the precision nor the extent of indication requisite when we would interrogate phænomena so variable in their electric tension as are those of the atmosphere. I employ the index electrometer which I made known (*Ann. de Chim. et de Phys.*, 1836, t. lxii. p. 422) and which I have reduced to the proportions of an ordinary electroscope. I have modified it only by curving the fixed conductor, in order to bring it back to the centre by prolonging it vertically two decimeters, and by surmounting it with a hollow metallic ball eight centimeters in diameter. I shall describe it more in detail in the work which will contain the applications and the tables of observations. This memoir treating only of the general principle, I shall more frequently mention the gold leaf electroscope as being more known to the reader.

7. We know that electrometers consist of two distinct parts; one which must always communicate with the earth, the other which is insulated from it, and carries with it moveable bodies, as gold leaves, a needle suspended on a pivot, &c. The sum of electricity which constitutes their equilibrium may be altered in two ways. 1st. It may be increased by giving to the indicating element a greater *resinous* tension. 2nd. It may be diminished by taking from this *resinous* tension. In the first case the sign is *resinous in plus*, and it preserves the term *resinous*; in the second case it is *resinous in minus*, and it bears the name of *vitreous*, although it in no way differs from the first case, excepting that the *resinous* tension is less than that of the point of equilibrium.

8. Rheometers (multipliers) in like manner indicate only the difference of the contrary propagations; they are also subject

to the inconvenience of requiring a continued unintermitting propagation, and their sensibility is very inferior to that of the static electrometers, as I have proved in a memoir published in the *Annales de Chimie et de Physique*, t. lxvii. p. 422.

These preliminary indications were necessary before entering upon the subject of this memoir, 'The cause of the Electric Phænomena of the Atmosphere, and of the Means of observing their Manifestations.'

Chapter I.

Of the Means of ascertaining the Resinous Induction of the Earth.

9. De Saussure* refers the first analogy made between electricity and lightning to Gilbert, a view which Wall† reproduced in 1708, Grey in 1735‡, Nollet in 1746§, Hales|| and Barberet¶ about the same time; and lastly, Franklin and his successors.

Franklin had only indicated the means of investigation, but he had not yet made any experiment, when D'Alibard** made his at Marly-la-Ville the 10th of May 1752, with a fixed apparatus, and Romas†† in the same year with a paper kite. The number of the fixed apparatus increased, being more convenient than the moveable ones. Beccaria‡‡, Le Monnier§§, Ronayne||||, Read¶¶, Schubler***, &c. made use of fixed apparatus; whilst Romas, the Prince Gallitzin, Mussenbrock†††, Van Swinden, the Duke of Chaulnes, Bertholon‡‡‡, Beccaria, Franklin§§§, and Cavallo||||||, joined the paper kites to the fixed apparatus.

* *Voyage dans les Alpes.* Note du § 648 A.
† Phil. Trans., vol. xxvi. No. 314, year 1708.
‡ Phil. Trans. abr. vol. viii. p. 401.
§ *Leçons de Physique*, 1re édit. du 4e vol. p. 314, year 1748.
|| Considerations on the Physical Cause of Earthquakes.
¶ *Dissertation sur le rapport qui se trouve entre les Phénomènes du Tonnerre et ceux de l'Electricité.* Bordeaux, 1750.
** *Traduct. des Lettres de Franklin*, by D'Alibard, 2e édit. ii. 99, and that of Sarbon Duboury, 1re part, 105.
†† *Mem. Sav. Etr.* t. ii. ann. 1755, p. 393.
‡‡ First Letter on Atmospheric Electricity. Turin, 1775.
§§ *Mém. Ac. Sc.* 1752. 233.
|||| Phil. Trans. 1772, p. 137.
¶¶ *Id.* 1791, 185; 1792, 225.
*** See his *Météorologie* and the Journal of S. X. B. Weigger, viii. 131; xi. 337; xix. 1.
††† *Acta Acad. imperat.* Petropol. 1778. 1st part, p. 76 of the history.
‡‡‡ *Electricité des Météores de Bertholon*, 1 CC.
§§§ Franklin's Works, Letter of the 19th Oct. 1752.
|||||| Treatise on Electricity, 4th part, ch. 1 and 2.

10. The results of the numerous experiments which have been made on atmospheric electricity have often been contradictory; Romas, the Prince Gallitzin and Mussenbrock remarked that the electric signs varied with the progress of the kite; a fact which De Saussure and Erman showed with the portable electroscope by raising or lowering it. On the other hand, Beccaria, Read, Schubler complained of the want of agreement of the fixed apparatus. Numerous anomalies and still more numerous absences of indication constantly occur and disturb the received theory, and have determined most observers to occupy themselves no longer with electrical manifestations. What appears the most contradictory is the absolute silence of the fixed apparatus, when the least elevation gives powerful signs of electricity in the moveable electroscope; and the progress in a contrary direction of the electric and the hygrometric signs observed by Mr. Clarke of Dublin*.

For if vapour were vitreous, as it is said to be, the instruments should act with greater energy when the atmosphere contains much, and the wind which strikes the exploring wires ought perpetually to charge them. All these difficulties should long since have witnessed against the received theory, and have caused the experiments to be renewed without any preconceived notion. This is what we have done, and of which a detailed account will be given in the treatise upon electric Meteorology that we are preparing, and of which the present Memoir is but an extract. We have reviewed the facts and have drawn the deductions which appeared to come out in the most rational manner from them, without troubling ourselves with those which had been drawn before us.

11. We shall first notice and reproduce Saussure's† and Erman's experiment‡, an experiment which alone sufficed to prove the error of the ancient theories and to open a new path for meteorology; but these celebrated physicists were not able or did not dare to place themselves in opposition to the received theories; but one of them was content to connect it with them by impossible suppositions, the other to remain in uncertainty and silence.

* Athenæum, 14th March 1840, No. 646.

† *Voyage dans les Alpes*, § 791 and following.

‡ *Annal. Phys.* Gilbert, year 1803, t. xv. p. 385–418; *Journ. Phy.* An. 12, A. 59, p. 98–105.

12. A common electroscope (Wood-cut, fig. 1.) must be taken, furnished with a stem not exceeding 40 centimeters, and terminated by a polished ball of metal from 7 to 8 centimeters in diameter. We shall presently say why we terminate the stem by a large polished ball instead of terminating it by a point. This ball may be of glass or card, covered with tin foil in order that it may be lighter. If,—under a clear sky, and in a place which is quite open, commanding the neighbouring trees and buildings, in short, higher than all the surrounding bodies which rest upon the earth,—if in this position the stem and the plate of the instrument are made to communicate in order to put them in an equality of reaction, it is then in equilibrium, and the gold leaves fall perpendicularly and mark zero. As this communication between the stem and the plate may be established at all heights, an electrometer may therefore be placed in equilibrium at any stratum and stand there at zero. Thus in equilibrium, it may be presented to the agitations of the atmosphere during entire hours without the leaves manifesting the least change in their equilibrium, if it be always kept at the same height*. This will no longer be the case if it be in the vicinity of a body projecting above the ground; this body would possess a tension so much higher as it is more elevated and more pointed. It sometimes increases to so great a degree that the induced electricity radiates from its asperities under the form of a luminous brush, which phænomenon has been called *St. Elmo's fire*. Horizontally receding from or approaching to a body like this gives the same result as vertically receding from or approaching to the ground.

13. Instead of remaining in the stratum where the instrument has been rendered neutral, if the weather be dry, cold, and the sky perfectly serene, it is sufficient in our climate to raise it two decimeters in order to have 20 degrees of divergence with the gold leaves, but this divergence is much more considerable if the temperature has been for several weeks from 10 to 15 degrees below zero; the elevation of a single decimeter is sufficient to project the gold leaves against the armatures. Beneath a clear sky the electric sign is always vitreous. If during the day much vapour has been formed, it is necessary, in order to obtain the same intensity of action, to raise the instrument so much higher as the air contains more. Having obtained this

* See the article 'Atmosphere' of the Supplement of the *Dict. des Sc. Nat.*, and my communication to the Acad. des Sc. of the 8th February 1841.

manifestation of *vitreous* electricity, if the instrument be lowered, in order to place it again at its first height where the neutralization took place, the leaves fall again to zero. If we descend below this point, by a quantity equal to that which at first surpassed it, the leaves again diverge, but then their sign is contrary, it is *resinous*. If the instrument be replaced at the point of departure, it again falls to zero. Thus, above this point it gives a *vitreous* sign; below it gives a *resinous* sign; and it again becomes neutral by being replaced at the point of departure.

14. By changing the point of neutralization, that is to say, neutralizing the instrument in a stratum above or below the first, the signs for the given heights are made to vary. For example, in the stratum of air where the instrument diverged *vitreously* it may be made to diverge *resinously*; for this it suffices to neutralize it above this stratum of air, and afterwards to lower the instrument to it. In like manner it may be made to indicate *vitreously* in the stratum of air where it had before indicated *resinously*; it will suffice to neutralize it below this stratum, then to raise it again to its height. In this experiment the air plays no part; the elevation of the instrument, its depression, and its horizontal movement are not able to make it take nor to make it lose any electricity; there is a difference in the distribution, but not in the quantity; all takes place as if under the induction of an electrified body; all is transitory, nothing is permanent.

I terminated the rod with a large polished ball, in the first place to render the phænomenon of induction more intense, and to leave no doubt as to the cause; secondly, in order not to complicate it with the radiation of electricity accumulated at the upper extremity.

Æpinus had already remarked, in the winter of 1766-67, during a frost of 24° R. below zero, and which lasted several weeks, that all bodies having lost a great part of their humidity by this severe and desiccating cold, they had become insulating, and it was sufficient to rub them slightly to obtain bright sparks. "However it may be," said he, "the air is not more spontaneously electric during these great frosts than during less degrees of cold; by itself it does not give electricity, but bodies take it with less friction*."

15. Instead of a polished ball, if the rod be provided with a

* See Guthrie's Memoir, Trans. Soc. Roy. of Edin., vol. ii. pp. 213–244.

bundle of points, to the phænomenon of induction there is added another which has always been the cause of mistaking the first. With points or sharp edges, by which the electricity may escape, that which is drawn to the extremity of the stem, when it is raised or lowered, does not remain there coerced; it flows away towards space, and only leaves in the stem the electricity accumulated at the other extremity. After this loss by the radiation of the points, if we replace the instrument at the point of its equilibration, this preserved electricity remains dominant; the leaves then diverge with a permanent electricity. In this case it is no longer the primitive phænomenon, it has ceased to be simple; a second phænomenon has taken place which has changed its nature by giving free passage to that which is attracted. If instead of points we place at the end of the rod a lighted wick, as Volta did, the progress of the instrument will be still more rapid; the electricity of induction will radiate more easily, and the instrument will directly attain its maximum of permanent tension. The more powerful the induction is, the greater will be the radiation and the more will the leaves diverge.

16. These two means have each a double inconvenience: in the first place neither of them exhibit the phænomenon in its simplicity, since their indication is similar to that of an electricity received from without, while they have only lost that which was repelled by the terrestrial induction to the upper extremity of the stem; secondly, the radiation of the points and of the German tinder in combustion depends on the humidity of the air, the rain and the force of the wind; they have besides the inconvenience of not preserving any electric tension worth notice during great humidity: the indication of the instrument is not the measure of the electric induction, but of that which has not been able to pass off by the moist air. These means being in entire subordination to the conducting power of the atmosphere, they are not adapted to collect and measure the variety of the electric tensions occurring every moment.

17. Besides, in all these experiments the instrument is only subjected to electric inductions, and the result is always conformable to the laws of the unequal distributions of the electricity of the body, of the radiation of that which is attracted towards the points or towards the flame, and of the conductibility of the ambient air. To make the conclusion more evident, I have produced in a small chamber the same series of experiments;

and I thus demonstrate that all the effects observed under a serene sky are really owing to the inductions of an electric body, and not to the contact of an electrized fluid.

18. The first care which should be taken in this kind of experiment, is to assimilate as near as possible to the circumstances which accompany the induction of the terrestrial globe, in order to have identical results, which could not be obtained if certain conditions were neglected.

One of the first circumstances we must take into account, is the infinite smallness of our instruments compared with the size of the globe which modifies them by induction. Secondly, their elevation above the ground may be regarded as infinitely small, and the action of the globe envelops them as a point placed at its surface. To approximate to these conditions, we must not then place the instrument on a globe, nor even on a plane surface; of whatever extent it may be, its limits will always be too near to the instrument, and the extreme rays of its induction will be far from having the same inclination as those of the terrestrial globe. The instrument, however small it may be, will be very much elevated compared with the extent of the surface of the body, and its rays will be too nearly vertical.

19. The experiment may be made in two ways, either by reproducing the vitreous induction of the celestial space by means of an insulated globe suspended from the ceiling; or by acting on the surface of an insulated globe resinously electrified. The first way is the most simple and easy: we place ourselves under this globe with a small electroscope, having a rod of two decimeters terminated by a well-polished ball or by a point, and under this globe is performed what we should do under a serene sky (12–15). Exactly the same results are produced, whether with or without radiation. But those persons who are prepossessed with the ancient theory, might reproach me with materializing the celestial space; with making it a ponderable body charged with a special electricity. This would be to revive the hypotheses of the last century, which made space to be a vast receptacle of electricity, from whence the earth was supplied by means of its conducting vapours. It is to prevent such objections that we reproduce the same facts in the way we shall now describe, although in reality, for those who are familiar with the science of electricity, the preceding experiment is quite sufficient.

20. In order to come as near as possible to the terrestrial conditions, and to cause the electrometer to be affected by the induction of the greatly inclined electric rays, I take a copper globe, 40 centimeters in diameter, fig. 2. *a a*, having an opening *b* of 16 centimeters. This globe rests on cakes of resin *c c*, in order to keep it insulated.

A small cord *d* is attached to a bobbin *e*, which turns with difficulty, and which is fixed on an immoveable rule *f*; the small cord traverses a pulley *g*, suspended from the ceiling; in the eye at its extremity passes a hook *h*, sealed in a stick of gum-lac *i*; at the bottom of this stick of gum-lac is fastened a screw *j*, from which is suspended a very small electroscope *k* by its rod *q*. To the plate *l* is attached a metallic wire *m*, which always touches the bottom of the globe by its inferior extremity, furnished with a ball of copper, *n*.

21. By means of the revolving bobbin *e* and of the cord *d*, which is rolled upon it, the electroscope *k*, which is insulated at its upper part by the stick of wax *i*, may be raised or lowered. By means of a metallic wire *o* which is held by a long and slender handle of gum-lac *p*, the rod of the electroscope *q* may be made to communicate with the globe, and thus occasion perfect equilibrium of electric tension, without either the experimentalist or the terrestrial globe playing any part in it. The electroscope is lowered until the stem from *q* to *j* is in the interior, it is held in contact with the globe by means of the conductor *o*, then a powerful *resinous* electricity is communicated to the globe. Like the rod, the gold leaves *z z* and the armatures *s s* are in equilibrium of reaction, the gold leaves do not diverge. The communicating wire is removed by its insulating handle *p*, and the rod of the instrument is then insulated.

22. By means of the bobbin *e* the electroscope is raised one or two centimeters; the leaves immediately diverge, and then indicate a *vitreous* tension, if the globe is *resinous*. If the electroscope is again lowered, the leaves return to zero. When the electroscope has been raised and when the leaves diverge *vitreously*, if the rod *q* is put in communication with the globe, a new equilibrium takes place, the leaves again fall to zero; but if, setting out from this point, we lower it to that which it occupied before, the leaves then diverge and are *resinous*. Thus, when a passage has been given to the free electricity of the rod *q*, it is the *vitreous* electricity of the leaves which has flowed off,

the other has remained in the upper part, repelled by that of the globe. When the instrument is afterwards lowered, the globe acting with greater force on the upper end of the rod, forces the *resinous* electricity which had been accumulated there to distribute itself towards the inferior end, where the action is increased in a less proportion. It would seem at first that it is the contrary that should take place; that the *resinous* tension of the globe repelling the electricity of the same name should force it to escape and retain the other. This is what would take place if the rod were made to communicate with another body; but its communication with the globe itself, which is *resinous*, retains repelled that of the same name, and neutralizes the *vitreous* portion which is free.

23. When the rod q is touched with an insulated proof plane, it does not show a preference for either electricity; it carries off that which is repelled by the globe and not that which it retains by its induction. It is then the resinous tension which diminishes and the vitreous tension which augments in the rod, which is contrary to the effect at the first contact, and which shows its opposition when the electroscope is lowered.

In the first case, the *resinous* electricity, retained less at the upper extremity, distributes itself over the rod, and the leaves diverge with the appropriate sign; in the second case, the leaves which were very divergent approach so near that they come into contact.

24. This result may be produced as often as desired, and the electroscope may be neutralized at different heights, so that the instrument may give contrary signs at a given height. I suppose that the electroscope has been neutralized, the ball t being even with the opening B; if it be raised three centimeters, the leaves will diverge vitreously: at this height, if the rod be made to communicate with the globe, the positive electricity will neutralize it in the globe and the leaves will fall to zero. If the instrument be raised three more centimeters the leaves will again diverge *vitreously*; and if we again establish the communication of the rod with the *resinous* globe, this *vitreous* electricity will be there neutralized, as in the preceding case, and the *resinous* alone will remain repelled at the furthest extremity. If we then lower the instrument the three last centimeters, to replace it in the zone of its second equilibrium, and where its first divergence has been *vitreous*, the leaves then diverge *re-*

sinously, they indicate the contrary sign to that of the first experiment, and where it did not give any indication in the second. The same happens in this case as in the first; the repelling action of the globe increasing faster on the further extremity of the rod which is lowered than on the inferior extremity, this force repels a part of the electricity which was accumulated in it towards the inferior part, until there be equality of reaction. It is this quantity, proportionally more repelled from the superior part, which makes the leaves diverge *resinously*.

25. In order better to understand the preceding demonstration, a portion of the atmosphere must be divided into equal and superposed strata, having for thickness the length of the stem of the electrometer intended to measure their different electric tensions. The action of the electricity, decreasing as the square of the distance, reacts more powerfully on the lower than on the upper end, and the natural electricity of the stem distributes itself equally from one stratum to the other.

If we cause these different strata to be traversed by this stem, kept insulated and free from the approach of any terrestrial body, its electricity distributing itself, according to the difference of the reactions, upon both extremities, this difference will diminish on receding from the ground. But the law of the square of the distance is not applicable to bodies of very different magnitudes and very near each other; and besides, an electroscope is not made of a single insulated stem, it does not alone advance in space; on the contrary, the lower extremity which supports the gold leaves has before it metallic armatures and a copper plate below, which communicates directly with the earth. This plate and these armatures touching the ground by intermediate conductors, preserve the entire terrestrial reaction; it is even augmented there, as occurs around the projections of electrized bodies. It follows that the lower part of the insulated stem is always subjected to the whole terrestrial tension, and that it undergoes all its *resinous* repulsion, whilst the upper part is only subjected to it laterally in proportion to its distance. The terrestrial repulsion remaining the same for the lower part of the stem, and diminishing for the upper part, the *resinous* electricity is accumulated in this latter extremity, and is there more abundant to the detriment of the lower part. Comparing these tensions to the point of equilibrium, the upper extremity is *resinous*, the lower *vitreous*, and the degree of their tensions depends on the difference of the reactions upon the extremities.

26. To facilitate reading the electrometer I make the experiment in an inverse manner. In weather when the air is charged with *resinous* vapours, when it is necessary to raise the instrument one or two meters, it would not be convenient, and frequently even it would not be possible, to read accurately the number of degrees which are indicated by the gold leaves or the index of my electrometer. To this difficulty are to be added those which depend on the wind, the rain, the heat of the sun, and especially that which arises from the electric tension which the head of the observer assumes in the open air. To avoid these inconveniences I deposit and read off the charged electrometer in the chamber below the terrace on which I operate.

When I wish to interrogate the electric tension collected in the atmosphere, I ascend to the terrace, place the instrument upon a stand raised 1 meter 50 centimeters, and neutralize it by touching the lower part of the stem; I then descend, and place the instrument on the stand which is intended for it; all this is done with great rapidity, and does not require eight seconds.

27. While neutralizing the instrument the arm must be raised as little as possible, for if it be raised so high as to touch the globe, the hand, becoming resinous by induction, would repel the *resinous* electricity of the ball; it would there neutralize the vitreous portion which it would attract, and the instrument would be *resinously* charged at the moment of removing the hand. The stem must therefore be touched as low as possible, and with a slender body, as a metallic wire, in order to avoid the induction of the mass of the hand on the remainder of the stem. Being neutralized before its elevation, the instrument on lowering gives signs of *resinous* electricity, whilst on raising it, it gives *vitreous* signs, which is what we have demonstrated above. When we thus operate it is necessary to recall this change of sign to mind, in order not to attribute a contrary electricity to the atmosphere. A *vitreous* tension is to be noted when the electrometer gives a resinous sign on descending, and a *resinous* tension of the atmosphere is indicated if the instrument brought down into the chamber gives a vitreous sign.

28. There is one more observation which we ought not to omit, it is that the divergence of the leaves or the deviation of

the index of my electrometer is less considerable when the instrument descends than when it is raised; the difference is very obvious, and the amount ought to be stated in order to ascertain the direct tension for the purpose of forming correct tables. The cause of this difference is that the action of the globe increases more on the lower than on the upper end of the stem, as may be seen in the table.

Equal and superposed strata.	Value decreasing as the square of the distance.	Ratio between the superficies of each stratum.
8	0·0156	1 to 1·245
7	0·0204	1 to 1·358
6	0·0277	1 to 1·444
5	0·040	1 to 1·560
4	0·0625	1 to 1·777
3	0·1111	1 to 2·250
2	0·2500	1 to 4
1	1	

The difference obtained with an instrument held in the hand is less than the table indicates; and, in fact, the armatures communicating with the ground preserve an almost equal action on the gold leaves during the descent of the instrument. It is necessary to make some previous experiments in order to deduce the proportion from them, if it be desired to take them into consideration in tables of atmospheric electricity. When I develop these principles I will give examples of the formation of these tables and of the corrections to be made to the rough observations.

We see by what precedes, that in nature the same takes place as under the induction of an electrified body; in the atmosphere we obtain only an unequal distribution of electricity arising from the *resinous* action of the earth. These experiments well understood, those of Saussure and Erman no longer present any difficulty, we find in them all the results of electric induction, and not those of communication and distribution.

29. If it be desired to keep to the simplicity of the primitive fact, a greater length than five decimeters must not be given to the stem of the electrometer; we even prefer to give it only three or four; by taking a longer stem the sensibility of the instrument diminishes. The reason of this is simple: the electricity of induction, coerced at the extremity of the stem, allows that of the contrary name to distribute itself over the remainder of the

length; the longer this stem is, the less will be the portion which will return to the gold leaves, and the less will they diverge; it is therefore in vain to take poles to study this phænomenon, the maximum of which manifests itself with a rod of three or four decimeters in length, terminated by a ball seven or eight centimeters in diameter. By employing masts, balloons or kites, it is not the electricity of induction in the electrometer, but its radiation which is augmented, and consequently the electricity repelled alone remains in the conductor, and becomes permanent if it be insulated, or gives a continuous current by its passing off to the ground if it be not. The employment of poles has led to another error which ought to be noticed. When the extremity of a wire is held to the end of a pole, and it is inclined without being raised, two contrary manifestations may happen: if it be elevated above all surrounding terrestrial bodies, the extremity of the pole is lowered by inclining it, and the electroscope then gives a *resinous* sign. If, on the contrary, a portion of the building be near, from which the end of the wire recedes on inclining the pole, it is really receding from the ground, and then the sign is *vitreous*.

Chapter II.

Of Vapours produced at a High Temperature.

30. The preceding experiments have been made leaving out of consideration bodies the presence of which might complicate the result; we have supposed them to take place under a perfectly serene sky and in the midst of an atmosphere which contains but little aqueous vapour. We must now mention what are the changes which arise when the atmosphere becomes charged with vapour.

31. One fact respecting the electric state of the atmosphere has been proved to be nearly general, viz. that its induction diminishes when the quantity of elastic vapour augments; the electrometer must be raised much higher in order to obtain a divergence equal to that of an experiment made under a serene and dry sky. According to the humidity of the air, it must be raised one, two, or ten meters, to obtain a sign of electricity, which is easily obtained by raising it two decimeters under a clear sky. This difference in the electric manifestations is not peculiar to the moveable instruments. The fixed apparatus are also affected

by vapours, but a contrary effect is produced in them; they are completely inactive under a clear and dry sky, and they give signs of electricity when the air is become moist, when the vapours produced in the day-time begin to be condensed, when in short the air becomes a better conductor and facilitates the electric radiation. Humidity of the air is for the fixed apparatus what the flame of Volta is for electroscopes—it is the means of losing the induced electricity accumulated at the extremities. These instruments would in fact be well adapted for ascertaining the quantity of water contained in the air, if the supports could remain perfectly insulated; if, becoming moist, they did not in every part allow the escape of the electric quantities which the radiation leaves free. But why is not aqueous vapour indifferent as the air is? Why does it modify the induction of the globe? Why does it alter the energy of this inductive action even sometimes so far as to change the signs of our instruments? These are the questions we are about to consider.

32. Since the experiments of Volta, of Laplace, and of Lavoisier in 1781, and according to much later experiments, it has been said that the electricity of vapours arose from chemical segregation; that the vapours of saline solutions carried off *vitreous* electricity and left to the liquids *resinous* electricity. We have repeated these experiments, we have analysed them, and we have already announced that their results are inapplicable and contrary to the natural phænomena*. The experiment, it is well known, consists in making red-hot a capsule of metal (platina is preferable), into which is poured a few drops of a saline solution. During the time that the liquid remains in a globular form, the electroscope, whether provided or not with condensing plates, gives no sign of electricity, although the drop of water be reduced to half. The same thing happens when the drop of water suddenly moistens the capsule of platina and becomes a mass of vapour. In one, as in the other case, there is no sign of electricity, notwithstanding the great quantity of vapour which rises. Between these two phases of the phænomenon, when the vessel has its temperature lowered between 140 and 110 degrees, and when some small parts of the saturated drop begin to touch the metal, the vapour is there pro-

* *Comptes Rendus, Ac. Sc.* 1841, t. 12, p. 307, and that of the 30th November 1840.

duced at a very elevated temperature, it thereby acquires a tension of several atmospheres, breaks the aqueous envelope which surrounds it, and escapes suddenly*. Other contacts form other vapours, the high tension of which occasions a series of explosions which separate the vapour instantly from the liquid by projecting it to a distance. It is only at this moment, it is only when these continued explosions take place, that the index of the electrometer is acted upon, and deflected in proportion to the temperature at which the vapour has been produced, and to the decrepitations which have resulted from them. Before, as well as after this series of explosions, whatever may be the quantity of vapours produced, there are never any signs of electricity, if foreign causes are carefully removed.

33. This result is easily understood. Whenever there is a chemical action, whether a combination or segregation, an electric phænomenon undoubtedly accompanies it. We have said and proved by experiments, that there is never any change in the molecular relations of a body without production of electricity, whatever may be the nature of these changes; but it is not sufficient that an electric phænomenon be produced, it is necessary that the unequal distribution which constitutes it be preserved, that the neutralization be not re-established, that, in short, there be an insulation, an obstacle which is opposed to the re-establishment of the normal equilibrium. We know therefore that in the evaporation of solutions electricity is produced; but it is not sufficient that there be separation of electricity, there must be preservation, and this cannot be unless at the moment of its production there be a perfect insulation between the two tensions, in order that the retrograde neutralization may not take place. These conditions cannot exist but when the vapour is formed at a high temperature; the tension increasing with it, the vapour escapes with force, it projects itself to a distance, and leaves suddenly a great space, which insulates it from the liquid. But when the vapour forms slowly, when nothing separates it instantaneously from the liquid, so that it rises from it tranquilly, the electric recomposition takes place before it has been sufficiently removed to constitute an insulating space;

* When the liquid is turbid, or has been darkened by a solution of indigo, the contact commences at a much higher temperature; I have found it several times above 200°. The projections are then so violent that it is difficult to protect oneself from them, and I have seen the index of my electrometer blown into the air.

the liquid and the vapour are then in the neutral state. This is the reason we cannot collect nor retain the cause of the electric phænomenon when the temperature of the vessel is below 110 degrees. Guthrie, in his curious dissertation on the Climate of Russia*, makes a very judicious observation: "Messrs. Volta and de Saussure," says he, "consider that the electricity of the atmosphere is due to the vapours which rise from the ground, and which are positive; but in our rigorous countries the vapour is reduced to its extreme minimum, and nevertheless the air is strongly electrical." We will not dwell on the error which belongs to the epoch, of attributing to the air that which is a product of induction; but with the exception of this error, Guthrie's reasoning is unanswerable.

34. From what precedes it will be easily understood, that in the combinations as well as in the segregations which take place in organized bodies, it is not possible that the ambient air should take and preserve free the static electricity which is produced; the conductibility of the surrounding substances does not permit the contrary tensions to remain in the presence of each other without being neutralized, and they are mistaken who would refer to the atmosphere the electricity which a metallic conductor collects and transmits to a Volta's condenser; they forget that the electric recomposition is very much easier in the midst of these humid substances than the preservation of their insulation. It is then neither in evaporation, properly so called, nor in the chemical actions of the assimilations and of the segregations of living bodies, that the cause of the electricity of vapours must be sought out. It must be looked for in the phænomenon as it takes place in nature; the power of nature herself must be used, without adding to it any of our complications.

35. This is not the place to describe the experiments which have determined us to consider electric phænomena as being always accompanied by ponderable matter, and to admit with Fusinieri, that all radiation takes place only by means of the transport of the molecules of bodies. We must however recollect, that electric phænomena are never manifested to us without matter; that there is no static phænomenon without a body coercing and preserving the electricity; that there is no dynamic phænomenon without a conducting body; that wherever there is an electric phænomenon, there is a ponderable body. The only

* Transactions of the Royal Society of Edinburgh, vol. ii. p. 224.

question which leaves any doubt, is that of knowing whether the spark is or is not accompanied by terrestrial matter, and whether the electric radiation can be made without the transport of this matter. Not being able to quote actually all the experiments which have determined us to adopt this opinion, we will call to mind that the radiation between two bodies takes place the more easily, as they are more volatile; that balls of platina must be held nearer to one another in vacuum than balls of zinc *, to have a constant current of light; that the radiation is still more easily made with mercury, and incomparably better with water. Besides, the experiments we are about to give, and which depend only upon meteorology, will be sufficient to render it probable that in this phænomenon ponderable matter is invariably transported, and that the general phænomenon only exists in consequence of this.

36. The preceding experiments have proved that the earth is a body charged with *resinous* electricity, or to express it more logically, that it possesses the cause of the phænomena to which we have given this name for more than a century. The celestial space, not being a material body, does not possess this power of coercion, it is not in the same state of *resinous* electricity; and it is this state of negatively *resinous* which has been named *vitreous*. It is not, as we have already said, a special, real state, caused by a peculiar substance or a peculiar modification; it is only the absence of the *resinous* state, or this state at a less degree. It is merely a difference and not a particular state. To avoid circumlocution, we have retained the word *vitreous* to express this difference in minus of *resinous* coercion. In a special work I will hereafter give the proofs which induce us to reject this nomenclature as defective and likely to retard the progress of the science of electricity.

Chapter III.

Of the Presence of Vapours in the Atmosphere.

37. We have seen (§ 32) that the vapours produced solely by the influence of the air and of heat below 110° did not

* See *Annali di Chimica di Brugnatelli*, t. xviii. p. 136. *Giornale di Physica de Pavia*, 1825, p. 450; 1827, pp. 353, 448. *Annales des Sciences*, 1831, pp. 291, 365. Phil. Mag. Oct. 1839. *Bibl. Univ. de Gen.*, 1840, t. xxv. p. 426. *Mem. di Zantedeschi*, Venezia, in 4to, 1841, and the Experiments of Messrs. Breguet and Masson, 1841.

possess free electricity; we are now going to resume these experiments by placing ourselves in the circumstances which exist in nature in order to obtain the same effects. Two kinds of evaporation take place in the atmosphere; that which is effected at the surface of water and humid ground, and that which is effected when opake clouds return into the state of elastic vapour. To imitate the first, I fill a platina capsule with distilled or common water, and I place it upon a thermoscopic tripod, insulated on cakes of resin, and I connect the tripod with an appropriate rheometer; I allow the spontaneous evaporation to continue for some time, until the rheometer remains constant during the cooling process. I then separate the rheometer in order to insulate the tripod, and I keep the capsule during a few minutes in a rather strong *resinous* tension. Afterwards, on re-establishing the communication with the rheometer, the latter indicates a higher degree than that of the spontaneous evaporation; which proves that during the action of *resinous* electricity the evaporation has been more considerable. The needle afterwards gradually returns to the primitive degree, and thus completes the demonstration. The same fact may be verified in another manner. The capsule is placed on an insulating body, and a body charged with *vitreous* electricity is suspended above it; under the induction of this body the water of the capsule quickly becomes *vitreous*, which indicates that the vapour which rises from it is *resinous*, and that it is the material vehicle which carries to the *vitreous* body the contrary tension by which it is neutralized. The *vitreous* tension of the water is so great that it must not be examined without a proof plane, for if the electroscope were brought into immediate contact, the gold leaves might be destroyed by it.

38. I have recently made an electric hygrometer, founded on the proportionality which exists between the loss of electricity and the quantity of vapours contained in the atmosphere: I shall elsewhere give a description of it. I will merely state, to render the experiment to which I am about to refer intelligible, that it is nothing else than my index electrometer surmounted by a metallic tuft, to which a constant charge of electricity is given. I exposed this hygrometer in the open air upon a very elevated place; Saussure's hygrometer indicated 75°, vapours disturbed the aspect of the sky, and it was necessary to raise the electrometer one meter to have a divergence, which was ob-

tained by raising it two decimeters beneath a perfectly clear sky. The hygrometer was 30′ losing the quantity of electricity necessary to reduce it from 60° to 50°, although it was surmounted by a large tuft of very fine copper wire. On moistening this tuft, or by covering it with a wet cloth, it required only 10′ to allow the same quantity to pass off. Another time, the temperature and Saussure's hygrometer being at the same degrees as in the preceding experiment, but the electrometer indicating *very resinous* vapours in the air, the index of the instrument, instead of proceeding towards 0°, augmented its deviation by several degrees. Lastly, in another experiment, made in the midst of a strongly *vitreous* fog, the index rapidly descended to 0°.

39. The vapours produced at the surface of the globe, under a clear sky, are necessarily *resinous*, since they are formed at the surface of a body which possesses a considerable resinous *tension*, and in presence of the celestial space which does not possess it. These vapours preserve this tension during the whole time they float in the atmosphere, and are insulated from bodies less *resinous* than themselves, or from conductors to the ground. They also rise to a height above that which they would attain by their gravity, and they do not stop until the point where their specific lightness, added to the electric repulsion of the globe, is in equilibrium with gravitation. The *resinous* vapours are maintained at a higher elevation than the neutral vapours, and still more than those which are in the *vitreous* state. The mutual repulsion which the particles of *resinous* vapour exert upon each other as insulated electric bodies, being augmented by that of the terrestrial globe, they are caused to recede further from each other; they are, in fact, more dilated than consists with the mere state of elastic vapour; they weigh less upon other bodies, and we shall see, in another memoir, that it is to these electric differences of the vapours that the horary and accidental oscillations of the barometer are owing.

40. The first effect of such an atmospheric state is to place us in the centre of an homogeneous induction, *resinous* towards the earth, and also *resinous* towards the vapours which are above us; and our instruments, which only indicate differences, being immersed in a uniform inclosure, obey in a less degree the *resinous* tension of the earth, as the atmosphere contains more of this primitive vapour. The indication of the electroscope will

then diminish in the course of the day proportionally to the quantity of vapours which have been disengaged under the opposite tensions of the earth and of space; and the variations of the instrument, if other causes did not concur, would serve to indicate hygrometric changes. Baron von Humboldt says, in the *Tableau Physique des Régions Equinoxiales*, in 4to, 1807, p. 100, "In the lower equinoctial regions, from the sea to 200 meters, the lower strata of the air are slightly charged with electricity; it is difficult to find signs after ten o'clock in the morning, even with Bennet's electrometer. All the fluid appears to be accumulated in the clouds, which causes frequent electric explosions, which are periodical, generally two hours after the culmination of the sun, at the maximum of heat, and when the barometric tides are near their minimum. In the valleys of the great rivers, for example in those of the Magdalen, the Rio Negro and the Cassiquiaré, the storms are constantly towards midnight. In the Andes, the height at which the electric explosions are stronger and make more noise is between 1800 and 2000 meters. The valleys of Caloto and Popayan are known by the frightful frequency of these phænomena." We quote this passage to show that in the regions where the electric phænomena are more considerable and more numerous, the electrometer indicates nothing; but it will now be understood that this absence of sign arises from its being always placed, in these regions, at the centre of a sphere of *resinous* vapours, produced every day by the high temperature.

41. When the atmosphere is thus charged with *resinous* vapours, it is evident that in raising oneself above the ground the instrument is disengaged from these vapours, and that the opposition of tension must re-appear; the inferior induction again takes its *resinous* superiority, and the superior induction loses it, and becomes *vitreous* by opposition. In atmospheric experiments, the number of decimeters or of meters which it is necessary to raise an electrometer in order to have an equal divergence, is the measure of the *resinous* vapours existing in the upper atmosphere; and if in very dry weather elevating the electrometer a decimeter is sufficient to obtain a divergence of five degrees, and it should be necessary another day to raise the same instrument sixteen decimeters to obtain the same divergence, the upper atmosphere contains four times more of this elastic vapour. The *vitreous* induction of

space decreases as the square of the resinous inductions which are interposed between our instruments and it.

42. The indication of the electrometers depending on the point of their neutralization and on the purity of the atmosphere, it would be necessary to fix the place of this point of neutralization, and to decide once for all on the moment which ought to be assumed for the maximum of resinous tension of the globe. To have the absolute measure of the terrestrial tension a complete absence of resinous vapour would be required; that is to say, it would be necessary to operate at the pole itself, when all the vapours have disappeared, and to measure, at the winter solstice, the divergence of a standard electrometer which has been raised one decimeter. If we possessed an experiment thus made, all other electrometers might be regulated by this standard electrometer, as barometers are regulated by a standard barometer. This means not being practicable, that must be chosen which is the most so for each climate. Thus in Russia, at Kasan for example, a frost of $-25°$ C., which has lasted twenty days, might be taken; at Berlin a frost of the same duration of 15° C. might be taken. At Paris it would be necessary to profit by a winter which would give during ten successive days a cold of 10° C. with an easterly wind. Severe and long frosts are too rare at Paris to serve as starting-points; it is better to adopt a less degree of cold, but which more frequently occurs, unless for the purpose of comparing the electrometers with those of Berlin, St. Petersburg or Kasan. It is obvious that the lower the temperature is, the less interposed vapour there will exist, and the nearer we shall be to the absolute tension of the earth.

43. During all these regular and successive changes, the fixed apparatus indicate nothing; they have always time to put themselves in equilibrium. These instruments are inactive unless their length is considerable, unless the electricity of induction, radiating from an extended surface of great curvature, as is the case with metallic wires, leaves in the instrument only the repelled electricity. When the exploring wire is very long, the radiation is sufficient to produce a continued current and cause the rheoscopes to act. These instruments, like static electrometers, indicate differences only; the current will also diminish in proportion to the resinous vapours which the atmosphere, by which the instrument is surrounded, may contain, and it is neces-

sary, by a greater elevation, to cause it to extend beyond this electrically homogeneous envelope. There is another cause of electric indication, which does not seem to have been sufficiently guarded against, viz. the chemical action of vapours on the oxidizable wire or rods of which the apparatus consists. An iron bar, like that of a lightning conductor, gives a continued electric current; a moist copper wire, twenty to thirty meters long, likewise gives one. These currents are always negative from top to bottom, and they increase in proportion to the humidity which has been deposited. Another cause of error is that which arises from the metals which enter into the construction of the building. When the walls and the supports are rather damp and they have become conductors, the oxidation which these metals undergo furnish a chemical current to the neighbouring conductor which carries it to the ground, and it is then difficult to say what the direction of the current will be: it will depend on the moisture of the vicinity to which the supports are attached. Of the electric apparatus which I possess, there is one which constantly gives an electric current, because it is formed of a bar of iron and of a zinc vane painted in oil colour. In damp weather the needle of the rheometer ascends to 80°, without there being any electric action of the atmosphere. We must therefore take care not to register such results as atmospheric products. For more security we must only expose to the air a tuft of platina wire, and cover the conducting wire with silk, covered over with an oily varnish, and insulate it in the best manner possible. With these precautions, the height of our buildings is hardly ever sufficient to enable us to obtain an electric current beneath a serene sky, a current which in these circumstances is always *resinous* from bottom to top.

44. When by cooling, these *resinous* vapours are condensed and have formed opake clouds, they preserve the quantity of electricity which they possessed; but this electricity is no longer distributed with the primitive uniformity which it had in elastic vapour not at its point of saturation. The molecules gathered then into small spheres or *vesicles* no longer retain their entire electric tension; in this new state they conduct better, and the electricity is differently distributed among them; their agglomeration into small masses or cloudy flakes gives them the periphery of a body, as well as preserving the individuality of the constituent parts. There are then two perfectly distinct

distributions in opake vapours; the electricity which proceeds to the surface of the masses, as it does to the surface of bodies; and that which is retained around these *vesicular* particles, sufficiently insulated from each other to maintain a portion of their primitive electric tension. The result of this better conductibility is, that by the *resinous* induction of the earth, the lower side of the cloud will be less charged with this same electricity, the upper side will be more charged, and our instruments will give better indications beneath an opake cloud than when they were enclosed in the elastic vapour which served to form it. On account of its importance we insist upon, and we return to this first modification of the elastic vapours: at first all are equally *resinous*, and the electric manifestations of our instruments immersed within them, diminish more and more. In consequence of their transformation into opake vapours, called vesicular, they by forming masses approach the nature of conducting bodies; a part of their electricity is coerced at the periphery, the other remains coerced around each vesicle. By the induction of the earth, the external portion of this electricity is repelled towards the upper part, and renders the lower part less *resinous*, which gives to our instruments the possibility of manifesting an electric difference. It is in these changes of conductibility and of electric distribution, that we must seek to explain the anomalous indications of our atmospheric apparatus. When we apply these principles to the entire range of aqueous meteors, it will be seen, with the aid of the barometer, the indications of which will then be better understood, that the transformations may be easily followed and more readily predicted.

45. When a new elevation of temperature makes this cloud again pass into a state of elastic vapour, the upper vapours which arise are more charged with *resinous* electricity than those which were primitively formed; the intermediate position of the cloud between the earth and the celestial space is very fit for the unequal distribution of *resinous* electricity; the superior part possesses a much higher tension than the inferior portion, since its resinous electricity is repelled by the terrestrial induction. The last vapours produced will consequently be in a less *resinous* state than the first; they will be *vitreous* as regards them. From this new evaporation results masses of elastic vapour of different electric tensions; the superior masses are

more *resinous*, the inferior *less resinous*; that is to say, there are transparent clouds differently charged with electricity, and floating in the atmosphere at different heights. When a lowering of the temperature condenses these transparent vapours, the superior masses form *resinous* clouds, the inferior masses form *vitreous* ones; the superior, repelled by the earth, maintain themselves at a greater elevation than accords with their specific weight; the inferior descend lower than they would by their weight alone, being drawn near by the attraction of the globe. In fact when they are superposed they attract each other, and as all the portions of these masses are moveable and partially preserve their independence, their position and their form vary incessantly, and, according to the resultant of these different forces which are present, the phænomena are perpetually altered.

46. This transformation of elastic vapour into opake vapour and of opake vapour into elastic vapour is reproduced a certain number of times, according to the atmospheric circumstances of temperature, wind and humidity. These secondary clouds, *resinous* in the superior regions and *vitreous* in the inferior regions, repassing into the state of elastic vapour under the same inductions, *vitreous* above and *resinous* below, form clouds highly charged with electricity; the *resinous* tension of the superior clouds becomes greater in proportion as they rise in the atmosphere and are impelled by the terrestrial repulsion beyond the point which is assigned to them by gravitation.

These masses of vapour, which pass a third time to the elastic state under the contrary inductions of the earth and of space, the first becoming more and more *resinous*, the others less *resinous*, are reconverted into opake clouds at the first cooling, in order to reproduce the same series of phænomena, by a succession of alternate condensations and evaporations. The new products being charged more energetically, if we follow the same mass of vapour in the series of numerous transformations which take place during a great part of the year, like those of the *cumuli* during the heat of the day, to form again during the cool of the night; or again, if we follow the *cumuli* formed by the vapours of the day, and which dissolve themselves in the strata of dry air high in the atmosphere, we shall see that the ponderable quantities ought to diminish in this succession of transformations, that the vapour in ascending becomes rarefied at the same time that it charges itself more powerfully with *resinous* electricity; that each transformation leaves behind a quantity which the electricity repelled by the superior induction had received in deposit, and that the ascending portions have a tension which increases with the number of the transformations, which tension they retain on account of their perfect insulation, and the electric energy of which augments to a degree that nothing here below can reproduce, too much surrounded with moist air and with inducing and conducting terrestrial bodies.

47. There are days very favourable to make these observations, it is when small *cumuli*, sufficiently thin to allow their internal motions to be distinguished, are spread over the sky. On examining attentively what takes place, the following is observed. Independently of the motion of translation of the entire mass, each part of the cloud changes its position with respect to the other parts, whilst the evaporation is taking place: these motions are more extensive, as the evaporation proceeds more rapidly; but they are not the same throughout the mass. Towards the edge which receives the direct rays of the sun, the evaporation being there greater, the last opake vapours become strongly vitreous, and they are seen to descend towards the earth, to pass even below the mass of the cloud, and to keep itself there; whilst on the opposite side the vapour extends and disperses itself until all is transformed into elastic vapour, but without that great agitation and strong repulsion from above to below. When, by a particular disposition in the upper part of the cloud, the solar rays are received and readily absorbed, and there thence results a prompt evaporation, a vaporous eruption is seen to proceed from below the *cumulus* and to extend itself before the mass. These last portions are not vaporized so quick as the first, and if we compare the rapidity with which the *cumulus* at first diminished, with that of the last thin and semi-transparent portions, we soon recognise the whole influence of the vitreous electricity of the latter, which far from assisting the temperature to vaporize these flakes, opposes it by the repulsion of similar tensions.

48. We may imitate one of these transformations, and judge by the result obtained of the powerful tension that the insulated clouds may acquire, when the vapours which constitute them have undergone several such changes. A watch glass *a* (fig. 3.), or a glass capsule, is pierced at the bottom by a small hole through

which a copper wire *b* passes. This copper wire is soldered to a support of the same metal *c*, which rests upon cakes of resin *d d*. Some water saturated with soap is poured into this capsule, and with a very small tube we produce a large cloud *e e*, formed of minute bubbles which rise and keep their position above the capsule. At some distance from the cloud of soap bubbles is suspended an insulated ball of copper *f*, which communicates with an electric machine. When we turn the machine, we see the extreme bubbles disappear shooting towards the *vitreous* ball, and the remainder of the cloud itself becomes strongly *vitreous*; which shows, that all the parts which shot away and were dissolved were *resinous*. We might take a capsule alone without any metallic support; but the tension of the remainder of the cloud increases much less, unless it be made of very large size, in order that the *vitreous* electricity be far enough off not to retard the *resinous* radiation towards space. When the cloud is small, and when we do not afford a means to the *vitreous* tension of removing to a distance from the portion in front of the electrified body, we have only a feeble result; whilst with the copper foot, which facilitates this separation and is a substitute for the greater size of the cloud, a single turn of the machine is more than is necessary to give to the remainder a tension capable of destroying the gold leaves of an electroscope placed in immediate contact.

49. Not to leave any doubt respecting the induction of the globe and of space, an analogous experiment must be repeated during very hot, clear and dry days, like those which took place from the 10th to the 15th of September of the year 1841. An insulated disc is placed in the open air communicating with an electrometer, and as much disengaged as possible from all lateral induction of the buildings; a small syringe is filled with distilled or common water, which has no influence on the result, and a small stream is projected vertically, so that it falls in small drops on the insulated disc. In favourable days like those I have just mentioned, the short time that the water takes to pass through the curve of its projection, is sufficient for it to fall again charged with vitreous electricity. If the atmosphere is less warm, and the evaporation is less rapid, it is rare that this time suffices to give the necessary tension to the water. The concurrence of a quick evaporation is necessary, under the induction of the ground and of space, for so short a time to suffice for

this electric production. It is evident, as M. Belli * has stated, that since the water falls vitreous, it is because the vapour which it has furnished was resinous. It is not the atmosphere which furnishes this electricity, for if the upper rod of an electrometer be made to pass through the same arc, the index will return to 0 after having deviated during the elevation of the point. In a very favourable day, the wet plate is sufficient for the phænomenon; the electroscope gives a slight *vitreous* indication. Secondary causes, such as oxidation, which makes the plate resinous, and the imperfection of the insulation during evaporation, do not allow of so great a manifestation as when the drops of water fall suddenly with their electric charges; but neither experiment leaves any doubt respecting the cause of the electricity of the vapours of the atmosphere.

50. There is a notable difference between the results of Nature and those of our experiments, principally in the succession of the transformations which we have related. In Nature, the superior empty space is not a body; the electric vapours do not become neutralized by contact with it; they are diffused, preserving all their electricity until the superior attraction is counterbalanced by their gravity. Space then is not a receptacle of *vitreous* electricity, since it enables the vapour to preserve its high *resinous* tension; it is only deprived of an action similar to that of the globe. It is otherwise with the last opake vapours which pass to the elastic state, and which are vitreous; if they do not come into contact with the earth to be there neutralized, it is because their specific lightness counteracts the terrestrial attraction; but when they can approach the ground or trees, they discharge themselves and become neutral; they no longer preserve any of the vitreous tension which they possessed. We see that in these transformations, produced by the elevation and depression of temperature of each day, the vapours are divided more and more into masses electrified to different degrees; that those most elevated in the atmosphere are the most *resinous*, and possess the greatest tension; that their elevation beyond the point assigned by their weight increases as the square of this tension, whether we consider the terrestrial repulsion alone, or combined with the superior attraction of space.

51. Among the intermediate clouds, there will be some less *resinous* than the globe; these will be attracted by the earth and

* *Bibl. Univ.* 1835, t. vi. p. 148.

will approach it: there will also be some as *resinous* as the globe; these will only obey their specific weight: finally, there will be some more *resinous* than the globe, and these will be repelled by it. This repulsion, joined to the diminution of the molecular gravity, carries them far beyond the limits which they would have attained without this succession of electric inductions, according to the transformations which they undergo.

52. The elastic vapours dispersed in the atmosphere cannot follow the ordinary laws of their repulsive forces; subjected to the electric element, they are not regularly distributed: the inequality of repulsion, according as more or less electricity has been able to coerce itself in different parts, will group the vapours into small masses, which are called flakes in the opake vapours. In the state of transparent vapour, the molecules more distant from each other, more insulated, and more independent, retain a greater proper electric tension; all are enveloped by a more extended electric atmosphere, which acts on its own account. The feeble conductibility of the mass does not allow the extreme molecules to have the superiority which the opake vapours take; the mass, as a body, can have but a feeble electric atmosphere, which increases with their density. To find these distinct masses of elastic vapours, forming transparent clouds, and carrying within them an electric tension different from the neighbouring masses, paper kites or captive balloons must be raised, and be caused to ascend sufficiently high to reach the region of *resinous* strata.

53. In dry and very clear weather, it is very seldom that we can meet with masses so powerfully charged that their action on the wire of the kite is able at first to destroy the effect of the inferior mass, and afterwards to give a *plus* result. The presence of a transparent *resinous* cloud is often not suspected, but by seeing the *vitreous* current diminish, and by seeing it again assume its first intensity after some moments of feebleness. But in slightly damp weather it sometimes happens that the superior masses are lowered, so that they float in regions accessible to our kites. I have several times found these *resinous* masses powerful enough at first to neutralize entirely the *vitreous* current which the radiation had produced in the inferior strata, then to give a *resinous* current of from 20 to 30 degrees. At certain periods after a continuation of hot days, which has produced a succession of opake and elastic transformations, such as takes place

towards autumn, and such as took place after the heats of the month of May of the year 1841, we find these resinous transparent clouds nearer the surface of the ground; and hardly have the vapours slightly condensed into some opalescent *strati*, when already the *resinous* induction is felt on the surface of the ground itself, and reverses the sign of the electroscope. The storms which arise from these *resinous* clouds are always more unpleasant to our organization than other storms; we are charged with *vitreous* electricity summoned by induction, and it radiates from all our extremities; whilst in the normal state it is *resinous* electricity which is summoned towards the head and towards the limbs we raise; they are charged with a sufficient quantity of electricity of induction to make the electroscope which we approach diverge.

54. The semi-conductibility of the clouds gives them distinct atmospheres—an exterior and a great number of others interior; all act at a distance like static tension, but on account of their semi-conductibility, all these tensions cannot unite in a simultaneous discharge. When the exterior electric atmosphere has been removed by an instantaneous discharge, all the small masses or flakes of vapours which have their own atmospheres, can only supply gradually to the periphery what it has lost; in the same way the electric atmosphere of the flakes being diminished, can only be formed again gradually by means of a new supply arising from the insulated particles that have preserved a part of their electric atmosphere. Fixed and greatly elevated apparatus are well adapted to render manifest the partial exchanges which follow the exterior discharge of the cloud, principally the gold-leaf electroscope, a much more sensible instrument than the rheometers. The leaves are seen to start, to open or close suddenly, or strike the armatures without any explosion being heard. The discharge of the periphery occurs only subsequently to a certain number of these partial discharges, after which the exchanges of the interior re-commence.

55. Much remains to be discovered respecting the distribution of electricity in vapours, and principally respecting the part which it plays in their condensation, their agglomeration and their grouping. We should deviate too much from the limits of a memoir were we to enter upon this subject, which we reserve for another work; we shall merely state that we have always observed that clouds strongly charged with *resinous* elec-

tricity have a leaden-blue colour, whilst those which are strongly *vitreous* are white, and adapted to reflect red. When we see a cloud with its head leaden-blue and its tail gray, we are certain of finding *resinous* signs before and *vitreous* behind, a distribution which is imposed upon it by the induction of a heap of *vitreous* clouds which precede it. The flakes dispersed in the atmosphere which are coloured orange-red possess a great vitreous tension. If this state presents itself after rainy days, it is a sign of amelioration, it is an indication that the vapours are less near the opacity which precedes their resolution into rain. The contrary is the case after fine days, when this tint of the vapours is a sign of bad omen and a commencement of condensation. It is known that many physicians admit an intermediate state between pure elastic vapour and opake vapour. The Count de Maistre* and Professor Forbes of Edinburgh† are among those who maintain this opinion.

56. When the series of vaporous transformations under the influence of the temperature and the electricity of the globe shall be well understood; when we shall have seen with what facility the opake clouds pass to the state of transparent clouds, and *vice versa,* always in presence of the earth powerfully charged with *resinous* electricity and of the celestial space not possessing the same resinous tension; when one single experiment shall have been made in order to be assured how quickly vapour is produced under electric induction, then only will be understood the different phænomena which may result from these masses of opake or transparent vapours, all charged with different degrees of *resinous* electricity, some possessing enormous tensions, others possessing less tensions, all tending to neutralize themselves, and only finding obstacles in the distances maintained by the difference of their gravity. We shall understand that when the superior transparent or opake clouds descend towards the earth with their powerful tension, they must produce great atmospheric perturbations, consequently energetic attractions and repulsions. When to these phænomena are added those of temperature, which condense or dilate these masses, which cause them to approach or recede according to their densities, we shall understand that electric phænomena may be produced at very different heights between these transparent or opake masses,

* *Bibl. Univ. Genève,* 1832, vol. li.

† Phil. Mag. 1839, vol. xiv. p. 419, and vol. xv. p. 25.

phænomena which will vary with the conduction of the vapours, some producing instantaneous discharges, others exchanges of some duration, according to the mass of electricity of the periphery and the interior conductibility. It is only when the development of these transformations has been followed, when the known laws of electric induction and those resulting from changes in temperature shall have been applied to them, that we shall be able to form an idea of the igneous phænomena which are perceived at such different heights and under such different atmospheric aspects. These are subjects to which we shall return in future memoirs, when we shall follow in detail the connexion of these phænomena, supporting ourselves by numerous facts and by positive experiments.

57. Rains, like the storms which follow these evaporations, are of two kinds, according to the position of the clouds; the first is that which arises from the condensation of the inferior vapours, which have become *vitreous* by means of the successive evaporations. These opake vapours, attracted by the terrestrial globe, form a stratum of reddish fog, possessing a very powerful *vitreous* tension. On approaching the ground these vapours gradually lose this great electric tension, either by radiation or by contact with terrestrial bodies, on which they are deposited under the form of dew*. If they are accumulated into distinct clouds, they form below vitreous storms, which discharge themselves upon the ground by lightning or by sudden local perturbations of the atmosphere; after these discharges the rain falls during some moments, sometimes during several hours, and then calm is restored. These kinds of storms are uncommon, and are never very intense, neither does the rain last long. The proximity of the ground, the density of the air and of the elastic vapours of the lower strata, and the agitation of the intermediate air, all combine to diminish gradually their vitreous tension, which is not the case in the second species of storm. Besides, vitreous storms do not affect disagreeably organized bodies; they exaggerate their normal state. The head of man, the summit of plants are more resinous by induction, but in fact they possess an electric state of the same order as the natural state. After this local disturbance it very often happens that fine weather is re-esta-

* See the observations of Achard in the *Nouv. Mém. de l'Acad. Roy. de Berlin*, 1780, p. 14—23. He has anticipated a part of these facts.

blished until an overcharge of inferior vapours again causes the same order of phænomena to re-commence.

58. The case is different when the resolution of the vapours is of the second kind, when it is the superior clouds which have descended after their condensation. The storms which determine this depression of the superior vapours are always very violent, the rains are considerable and often of long continuance. It is not only the inferior stratum, it is a mass more or less thick of condensed vapours descending from the high regions, which dissolves into rain, after having discharged their powerful *resinous* tension, either by sudden agitations of the air, or by lightning between clouds of opposite tension or with the ground. In these storms the wind is always more violent, more sudden, and more capricious than in vitreous storms. Considerable showers follow these discharges, and very often the weather remains rainy until the atmosphere has lost its superabundance of vapours, or favourable winds have carried into other regions the long rains which follow the lowering of the superior vapours. During these storms organized beings have their summit in a *vitreous* state, that is to say, below the normal state; this state, contrary to that which is natural to us, causes an uneasiness which it is difficult to define, and may produce hurtful effects on nervous and sanguine temperaments. In the natural state, or even when it is *resinous* with exaggeration, the superior extremities form the resinous pole of a current, whilst in the contrary state they form its vitreous pole. Lastly, if the vapours possess a high electric tension, if they approach sufficiently near to plants to permit the exchange to be made with the ground through their intermediation, the evaporation from them may be accelerated so as to destroy the vegetation and render the leaves yellow, as is seen after waterspouts, strokes of lightning, and even dry and red fogs*. Resinous storms are also very frequently formed in the lower stratum of the atmosphere; but this subject is too extensive to be treated incidentally here; we reserve it for a special work.

59. Wishing to restrain myself in this first Memoir to the indication of the means adapted to ascertain the resinous induction of the globe and its most immediate effects, I have been obliged

* *Observations et Recherches Expérimentales sur les Trombes*, §§ 168, 178, p. 159.

to pass over in silence the phænomena which acknowledge as their cause the changes of temperature; these are always regular, and are never subject to abrupt transitions. The trade winds, the monsoons and gales, testify this regularity of ascending or descending variations of temperature. The sudden instantaneous winds are produced by ruptures of electric equilibrium. It is the prompt attraction and repulsion of the air, intermediate between the clouds and the ground, which cause these sudden agitations, in order to neutralize their electric tensions. They produce those sudden storms and aërial gyrations which I have described in my work on Waterspouts*. In my treatise upon *Meteorology* now preparing I shall unite the proofs of the facts advanced in this Memoir; however, what I have stated will suffice to make known how this new mode of considering the subject facilitates the interpretation of many aqueous and igneous phænomena, which have been hitherto inexplicable.

Summary.

60. 1st. Ponderable matter alone has the power of coercing the cause of electric phænomena; and the phænomenon which produces this coercion is that which has been improperly called *resinous electricity*, and still more improperly *negative electricity*. §§ 4 to 36.

2nd. Pure space, deprived of ponderable matter, not coercing this cause in a special manner, cannot react with an equal force against the resinous action; this negation of *resinous reaction* is called *vitreous electricity*, or still more improperly, *positive electricity*. § 36.

3rd. The earth, as a ponderable body, possesses a powerful *resinous* tension, and the celestial space which surrounds it, not possessing this state, is in the state of *minus resinous* or *vitreous*. §§ 14, 36, 40, &c.

4th. The earth, like every electric globe in the midst of a free space, has its tension at the surface, and this tension may increase or diminish in certain points, according as the bodies placed above have a lesser or greater tension, that is to say, according as these bodies are *vitreous* or *resinous* in relation to the electric mean of the globe.

5th. Every body situated at the surface of the earth partakes

* Chap. ii. first part, p. 67.

of its *resinous* tension; this tension increases as the body projects more in space. Thus mountains, buildings, and even organized beings, have stronger *resinous* tensions than the ground on which they rest. § 12.

6th. When we insulate a body, after having placed it in communication with the ground, it is in equilibrium of reaction in all its parts; and according to the distance of each of them; the inferior portions are less *resinous*, the superior portions are more *resinous*. In this state of distributive equilibrium, if this body possess moveable parts like the gold leaves of an electrometer, they will not indicate any preponderating action. § 12.

7th. If beneath a dry and clear sky we remove this body or this electrometer from the surface of the ground, or from an elevated body which is attached to it, the reaction of the globe no longer acting in the same proportions on the length of the insulated rod, the *resinous* electricity is differently distributed on it; it increases towards the upper part, it diminishes towards the lower part, and the moveable leaves attached to the latter separate from each other in order to approach the bodies attached to the ground or possessing the same tension with it. This *minus resinous* tension which the gold leaves possess is called *vitreous*. §§ 12 and 13.

8th. If we lower the electrometer, the first equilibrium is again produced; the leaves are at zero. §§ 12 and 13.

9th. If we cause it to descend below the point of equilibrium, the reaction of the globe increasing more on the upper part than on the lower, the *resinous* electricity is repelled from it; it becomes dominant in the lower portion, and the leaves diverge *resinously*. Thus it is not the atmosphere which acts on the electrometer, but the resinous tension of the earth. §§ 13, 14, 25, 28.

10th. To avoid complication in this experiment, the upper extremity must be terminated by a polished ball, in order to avoid the radiation and to increase the effects of induction. In this state the electrometer may remain twelve hours exposed to the air and to the winds, without manifesting the least electricity. § 14.

11th. Since there is no electric phænomenon without ponderable matter, the radiation between two bodies differently electrified takes place so much better as these bodies, or one of them, evaporates more easily; consequently the water at the surface of the resinous globe evaporates better under this electric induction. The vapours carry with them a resinous tension equal to that of the surface of the liquid, and they disperse in the atmosphere according to their specific weight and their repulsion as bodies charged with the same electricity. § 35.

12th. The vapours thus dispersed react from above downwards upon the electroscope; they place it in a *resinous* inclosure, and the instrument in rising or in lowering undergoes but feeble differences of reaction, or indeed sometimes does not undergo any appreciable change; it is only by an ascension with a paper kite or a balloon that the extremity of the instrument can pass beyond this inclosure of homogeneous reaction. Assuming as a starting-point the induction obtained beneath a clear sky during a prolonged frost of 10° C. below zero, the diminution of this induction will indicate the surplus of resinous vapours contained in the atmosphere. § 13.

13th. Fixed apparatus of small extent are useless in dry and clear weather; they cannot render manifest electricity of induction, since they remain at the place of their equilibrium; and they cannot radiate that retained on their surfaces, because the dry air is a good insulator. When they are of considerable length and are very high, the extent of the wire making up for the feebleness of the local radiation, the apparatus loses its electricity of induction and becomes charged with permanent electricity. § 43.

14th. When the air is rather damp the radiation is favoured, and continuous currents may be obtained with shorter lengths of wire. § 43.

15th. These electric phænomena arising from atmospheric inductions, must not be confounded with those which arise from the oxidation of the conducting wires immersed in a damp medium. This cause of error has often been the means of attributing to the atmosphere what belonged to a chemical action. § 43.

16th. When by a depression of temperature the first vapours are condensed, they form opake clouds, and the electricity which they have carried away is distributed according to their groupings and the ambient inductions. The induction of the earth renders these clouds more *resinous* in the upper than in the lower part; the electrometer will then diverge more beneath these vapours accumulated into clouds than when they were equally disseminated. § 44.

17th. When the temperature rises and these primitive clouds return to the state of elastic vapour, it is under the *resinous* induction of the earth below and under the *vitreous* induction above. The first vapours which arise from them are more strongly *resinous*, whilst the latter are less so. The atmosphere then contains masses of *resinous* elastic vapours, and masses of *vitreous* elastic vapours. In a word, there are then transparent clouds, some charged with *resinous* electricity, others charged with *vitreous* electricity, and intermediately some neutral spaces, which are the bright ones. The limits of these transparent clouds may be found by means of a kite or a balloon. §§ 45, 46, 52, 53.

18th. The condensation of these transparent clouds forms secondary opake clouds charged with their respective electricities. A new elevation in the temperature produces a new division in the electric charges. The first vapours produced are more *resinous* than the preceding ones, the last are less so; they are *vitreous* in relation to the superior vapours and *resinous* in relation to the inferior. It is after these successive condensations and evaporations that the superior vapours acquire stronger and stronger *resinous* tensions, and that those near the ground become more *vitreous*. Intermediately there are some of different degrees, which are maintained separate by the difference of their specific weight. § *id.*

19th. In proportion as the vapours, by this series of transformations, are charged with an increasing electricity, their molecules repel each other more; they are also more repelled from the earth or attracted towards space; they rise to heights greatly superior to those which correspond to their specific weight. These opake or transparent clouds, charged with different degrees of *resinous* electricity, (some of which, in relation to the tension of the globe, which serves as a standard, are *resinous*, and others *vitreous*,) neutralize each other when they approach, in consequence of the condensations which the depressions of temperature and the diminished terrestrial repulsion cause them to undergo. When there are other strata of clouds interposed, exchanges are made, which vary with their mode of agglomeration and their conductibility. These exchanges produce sudden explosions, if the periphery of the clouds contains much free electricity; or rather continuous *explosions* if the conduction is feeble, and if the discharge takes place along the contiguous masses. §§ 54, 55, 56.

20th. A discharge at one point being followed by a new equilibrium, provokes others; it is thus that meteors succeed each other, and may become numerous at certain epochs; it is thus that simultaneous meteors which succeed each other at short intervals are observed in countries distant from each other, meteors which have been frequently taken for the same, on account of the apparent simultaneousness of their existence.

21st. After these restorations of equilibrium, or electric discharges, the vapours being less repelled, gravity regains its influence; they descend, become condensed, and at last dissolve into rain.

22nd. Rain proceeding from *resinous* clouds is more abundant than that proceeding from *vitreous* clouds; the winds are likewise more sudden and violent, and it is under the induction of masses of *resinous* clouds that tempests and inundations arise.

Fig. 1.

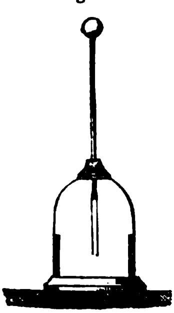


Fig. 2.

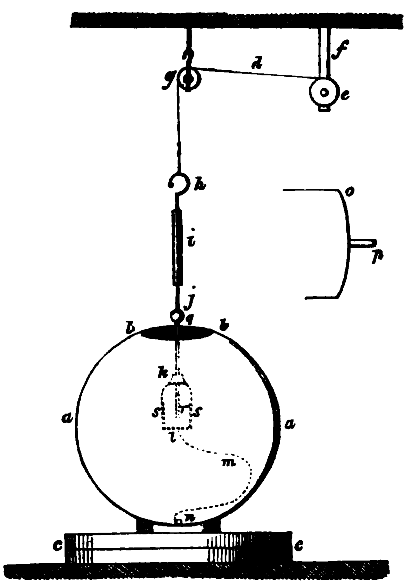
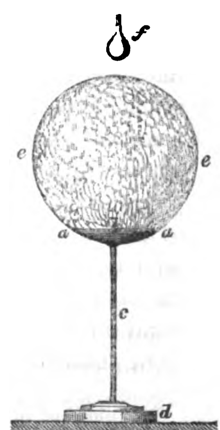


Fig. 3.



Article XV.

On the Cause of the Differences observed in the Absorbing Powers of Polished and of Striated Metallic Plates, and on the application of these Principles to the Improvement of Calorific Reflectors. By M. Melloni.

[*Annales de Chimie et de Physique*, 3ième Série, vol. i. p. 361. March 1841.]

In my last memoir* I often treated of plates whose temperature became gradually raised in consequence of their power of absorbing calorific radiations. Under this head I particularly pointed out, that metals, and other substances capable of being made smooth and shining, were yet frequently so prepared as to present dead and completely unpolished surfaces.

This was an essential condition in our experiments; for it was necessary to distinguish between the dispersion and reflection, properly so called, in order to follow out, with any chance of success, the objects we then had in view. If, instead of using exclusively plates that are dead and uneven in surface, we experiment sometimes with polished and sometimes with unpolished surfaces, the calorific absorptions, compared with the nature of the plates, afford very curious results, of which we shall give a rapid sketch.

A disc of brass, the surface of which is yet rough and granular, becomes more strongly heated under the action of calorific radiation than a well-polished disc of the same metal. On the other hand, a metallic vessel of a rough surface, filled with hot water, cools more rapidly than a vessel made of burnished metal. These experiments have induced many experimental philosophers to admit that minute points and asperities in the surfaces of bodies increase both their absorbing and emissive powers. I have already endeavoured to prove, in a note communicated to the Academy†, that the emissive power of bodies does not depend on the degree of polish or of roughness communicated to their surfaces.

[* Published in the *Annales de Chimie et de Physique*, vol. lxxv. p. 337, for Dec. 1840. A translation of this Memoir will appear in a future Part.—Ed.]

† See *Comptes Rendus des Séances de l'Académie des Sciences, année* 1838.

We shall now see that it is the same with their absorbing power. But before entering on the experimental proofs, it is essential to guard against any mistake as to the exact meaning of what we have just advanced. Our proposition does not bear upon the fact itself, which we in no way dispute, but upon the explanation hitherto given of that fact. Thus, if the polish of a metallic body be destroyed by friction, with emery, or with a file, so as to render its surface rough and dull in place of its former smoothness and brilliancy, the proportion of heat absorbed by this body in a given time, when exposed to calorific radiation, is unquestionably altered: this alteration may even be such that the metal becomes heated doubly or trebly what it was before; and yet we maintain that the roughness or polish have no influence whatever in producing the phænomenon, and that the change effected in the absorbing power of the metallic surface has an entirely different source. The following experiments prove this.

If two small copper discs, the one striated or unpolished, the other polished and shining, be successively placed before a good thermoscope, each of the discs being blackened on the side turned towards it; and if we then cause exactly the same calorific radiation, concentrated by means of a lens of rock salt, to act on their anterior surfaces, we shall observe the effects which are above stated, viz. that the roughened disc becomes more intensely heated than the polished one. The result is the same if we act on polished and unpolished discs of steel, tin, silver, gold, or any other metal reduced into plates by means of the hammer or the flatting-mill. But if the experiments be repeated on two plates of tinned iron, one of which has been subjected to strong and quick blows of the hammer and the other left in its natural state, we shall find that the latter, which has an even and reflecting surface, becomes far more intensely heated than the former, whose surface is less shining and full of unevennesses. Again, if we take two plates of silver or of gold that have been melted and slowly cooled, one of the plates possessing the fine polish which can be imparted by means of oil and of fine charcoal, while the other, having been similarly polished, is afterwards unpolished by a series of lines traced with the diamond, we find, to our surprise, that the results are exactly contrary to those which occur under the ordinary circumstances; that is to say, the striated plate becomes less heated than the shining and polished one*.

But if, by destroying the polish, we can in one case increase the absorbing power, and in another diminish it, it is evident that the formation of points and asperities, through which a larger quantity of heat might be introduced, is not, as it has been generally supposed, the cause of the differences observed between polished and rough surfaces, but that this difference is rather to be attributed to changes in the hardness or in the elasticity of the superficial layers of the metal; for there is no doubt that the operations by means of which we render the plate rough or shining, necessarily produce at the same time displacements of the molecules; displacements which in some cases cause the integrant particles permanently to approach nearer, in others to recede from each other, and which increase the hardness and elasticity of the metal more or less, according to its previous consistence, and to the method adopted for imparting to the surface a more or less decided roughness or polish.

As to the precise kind of effects respectively produced, it is clear, from what we have just said, that the absorbing power diminishes as the hardness or elasticity of the plate increases. Thus, hammered tinned iron, compressed by the percussion of the hammer, possesses a feebler absorbing power than when in its natural state. This inferiority is not the result of a more perfect polish, for we may easily give the hammered disc a polish very inferior to that of the unhammered one, without rendering by this the absorption superior to that of the unhammered disc. It is therefore really the greater hardness which diminishes the absorbing power of the hammered plate.

Polished copper prepared by the flatting-mill having by this means been in reality compressed, increases in absorbing power when it is subsequently striated; because the furrowed lines ex-

* It is necessary to use gold or silver, because in the case of copper or of any other oxidizable metal the striated surface would become coated with a film of oxide far more rapidly than the smooth surface. This would greatly increase the absorbing power, and it would be difficult to distinguish between this and the effects resulting from the respective influence of the polish and the striæ. For the same reason the surfaces of the gold or silver must on no account be roughened by means of emery or of a file, which, even after repeated washings, would always leave more or less abundant traces of heterogeneous matter incrusted on the metal, and would produce on the unoxidizable surface the same effect which oxidation would occasion on a plate of copper or any other metal alterable by the air.

pose the softer parts of the interior, and allow the remaining compressed superficial layers, of which the particles were previously restrained by their mutual compression, to expand and dilate themselves in the solutions of continuity which have been made on the surface.

The plate of gold or of silver which has been fused and slowly solidified, and has received a slight polish, loses on the contrary in absorbing power when it is striated; because the point of the diamond compresses a portion of the soft metallic surface, and thus imparts to it a greater degree of hardness.

The influence which the degree of hardness or elasticity of metallic plates exerts on the calorific absorption, is especially evident from the following fact related to me by M. Saigey, and confirmed by M. Obelliane, Préparateur de Physique to the Ecole Polytechnique and to the Faculté des Sciences at Paris. Dulong had two large conjugate mirrors constructed of cast metal perfectly tempered, and on experimenting with this apparatus he was greatly astonished at finding it less effective than another much smaller pair of mirrors which had been worked by the hammer, and had long been among the instruments of the latter establishment. There was at that time no means of accounting for this singular anomaly, which was merely suspected to result from a difference in the quality of the copper used in the manufacture of the two apparatus. Every one can now at once perceive it to be an immediate consequence of our principles. The cast mirrors had necessarily been less compressed, and were therefore less hard and elastic than the mirrors that had been prepared with the hammer; they would consequently absorb much more heat, and give a feebler reflection. Thus, to secure good calorific reflectors, it is not merely sufficient to polish their surfaces, but the metallic plate from which they are formed must be powerfully compressed, so as to communicate at the same time to the metal a regular surface, the highest possible polish, and a great degree of hardness and elasticity*.

* The great influence which the hardness or the elasticity of the superficial layers seems to exercise on the calorific reflection of metals, an influence far more decidedly marked than in the analogous case of light, is undoubtedly closely connected with the nature of heat itself. And it is much to be wished that this latter should become the subject of a profound investigation from those mathematicians, who now study, under all possible forms, the vibratory motions of the fluid whence the phænomena of light, heat, and chemical action, manifested by the rays of incandescent bodies, are conjectured to be derived.

This result, which might have been deduced by analogy from our first experiments on the emissive power of polished and striated surfaces, had not escaped the penetration of M. Saigey, who has since then applied the principle very successfully in the construction of conjugate mirrors, and of other apparatus intended for the reflection of heat.

The new theory, which takes from the points and asperities their supposed influence on calorific absorption, and attributes it to variations in hardness or in elasticity, is strikingly confirmed by the constancy of absorbing power remarked in all substances which are incapable of retaining any state of compression impressed on their superficial layers by any mechanical means whatsoever. Thus we find that a disc of marble, jet or ivory absorbs the same quantity of heat in their natural state, as when they have either received the highest possible polish, or been roughened by means of coarse sand or emery; because in these classes of substances the methods which either develope or destroy asperities and inequalities of surface, do not in a permanent manner affect the hardness or the elasticity of the superficial layers. Finally, I may remark, that in the course of my various experiments I have never been able to detect any difference in the amount of heat acquired by bodies exposed to calorific radiation, when their surfaces have been successively painted over with colouring matter of various degrees of fineness. In this case, as in that of the discs of marble, jet or ivory, there is a greater or less variation in the regular distribution of the superficial points and asperities, but without any appreciable alteration in the state of hardness or of elasticity.

When I showed the insufficiency of the received theory as to the action of superficial asperities on the radiation of bodies, it was objected that irregularities of surface would necessarily cause the quantity of heat received through any one given point to vary, merely from the circumstance of reflection. As the same objection might be raised respecting absorption, we will remark, first, that in speaking of asperities in a radiating or absorbing surface, we mean only those minute irregularities produced in destroying polish; for it is obvious that all very sensible protuberances or concavities might act as real reflectors, and accumulate larger quantities of heat in certain directions. Secondly, we must observe that it is not here the question of a general law, but of a particular fact. By the striation of certain polished metallic surfaces an increase is obtained in their emissive and absorbing power: this increase cannot be attributed either to reflection from the points and asperities, or to any other direct and immediate action of these asperities; since we have seen,—1st, that the striæ have no sensible influence on non-metallic surfaces; 2ndly, that the effect is very various, according to the nature and condition of the plate used; 3rdly, that metals which are unalterable by the air, will, if properly prepared, give an inverse effect, the presence of asperities then diminishing instead of increasing the emissive and absorbing powers. This last fact appears to me conclusive. Thus the increase long ago observed in the emissive and absorbing power of striated metallic plates is only a particular case. The absence of effects, and the diminution subsequently observed by us in marble and silver properly prepared, are also particular cases; so that the variations in emissive and absorbing power which accompany the conditions of polish and of roughness, in substances susceptible of being made smooth and shining, have not a character of generality, but on the contrary alter with the nature of the bodies and the state of molecular equilibrium impressed on their superficial layers. These alterations however are only observed with metals, and we know that when subjected to the action of mechanical forces these bodies undergo permanent modifications in the specific gravity, hardness, and elasticity of their superficial layers: now these modifications are the only effects known. In attributing therefore to them the changes observed in the radiation and absorption, we are in reality not advancing any hypothesis; we are merely announcing the phænomenon under a new form, expressing conditions that were unknown before the experiments which we have been detailing in this notice.

Article XVI.

On Vision, and the Action of Light on all Bodies. By Professor Ludwig Moser *of Königsberg*.*

[From Poggendorff's *Annalen*, vol. lvi. p. 177, No. 6, 1842†.]

At the present time, when so many researches are made upon the action of light on the surfaces of certain bodies,—an influence which has been denominated *chemical*, but which can hardly be called so much longer,—the question might be asked, is the influence of light on the retina of the same nature? Is the optic nerve the conductor of the oscillations of light, or does it convey the sensation of its *material action* to the central organ? I believe there are many phænomena of vision which cannot be well brought into accordance with the first supposition, and which become quite comprehensible if we allow of a material influence. To mention only one instance, I would direct attention to the influence which time exerts in the process of vision, inasmuch as external objects are not perceived instantaneously, their colour depending upon the length of time spent in observing them, and finally, the objects seen do not vanish with them, but remain present to the eye for a greater or less space of time. In the following paper I shall attempt to carry out, as far as possible, the analogy in the action of light on the retina and on other substances which are at present being studied, without, however, going one step further than I am justified in doing by my numerous researches. I am well aware that philosophers might not be very favourably inclined towards the subject of which I am about to speak, since a strong prejudice exists against the assumption of light producing material changes in the retina, even though they were so very inconsiderable, as I hope to be able to prove they are. Such being the case, the subject requires the greatest care; but this cannot induce me to keep back a view which, after mature consideration, appears to me to be well-founded.

* Translated by Henry Croft, Esq., Professor of Chemistry in the University of Toronto in Canada.

[† The exact date of this paper cannot be given; it was probably published about the end of June, as the *Annalen* generally appear about the end of the month, for which they are intended.—Ed.]

In the first place the three following propositions will be proved:—1. The blue and violet rays of light are not alone those which act chemically; or if there are supposed to exist in light chemical and luminous rays, the chemical ones are not contained solely in the most refrangible rays of the spectrum. 2. It is not necessary to suppose—and in those phænomena which have been best observed it certainly is not the case—that light produces a separation of chemically-combined bodies. The action of light is, as I hope to show, of such a kind that it may be imitated in a perfectly different manner, so that the idea of a chemical decomposition is fully refuted. 3. Even the most continued action of light appears to affect only the finest surface of bodies, and by no means to penetrate the usually so extremely thin layer of iodide of silver.

It is evident that if these three statements can be proved, the strongest objections to the new view are put aside. Whilst all colours act on the eye, it is generally believed that only certain colours, principally blue and violet, exert their influence on those substances which are capable of being affected by light.

I shall not attempt to nullify this objection by the remark (which however it is as well to remember in other instances), that we at present know the action of light on extremely few substances, in which cases it can be made visible to the eye; that light may act on many other bodies, perhaps on all (see the end of this chapter); and that among them there may probably be some which are affected by all colours alike. This call upon our ignorance for assistance is in so far unnecessary, as iodide of silver really does undergo a certain action from all colours, and as there is, indeed, as I shall show, a state of this body which, if it has once acquired, all colours act on it equally. The second and third statements show us how small the changes produced by light are; by a continued action they might become more considerable, but that would be exactly in favour of the new hypothesis: for the continued fixing of an object, which is always so difficult for the eye, shows by its after-action that the changes produced on the retina cannot be so very inconsiderable; as it requires, according to circumstances, minutes, hours, even days and months to return to its normal condition.

Before I proceed to the experiments I will say a few words on the best manner of making them. It appears at first sight most convenient to make use of sensitive papers and a darkened chamber; but we soon find that this is not the most advantageous method. No well-defined results are obtained, and we always remain in uncertainty, at least as far as regards the more delicate phænomena. The method of Daguerre is the only one which can be advantageously adopted. The operations proposed by him are calculated to produce a perfect artistical representation, and they answer their purpose effectively, except indeed in the covering the silver with iodine, for the performance of which he proposes a very inconvenient apparatus.

When engaged in physical experiments we may sometimes neglect, to a certain extent, some of the minuter prescriptions; but still it is always better to keep constantly in view the production of perfect images. It is not my intention to enter further into this subject at present; I will only describe a convenient apparatus for iodizing, so that others may easily repeat the experiments about to be described.

A box, of any material, is made of a sufficient size to receive the silver plates, and about an inch in height. On the bottom of this box a glass plate, covered with any kind of woollen stuff, is fixed, and another glass plate is arranged so as to cover this. Iodine is sprinkled between these two glasses, and soon diffuses itself through the whole of the woollen stuff, and colours it black; the plates prevent its evaporation, and it is very seldom necessary to repeat the sprinkling. If the silver plate be now laid above the lower glass plate, a perfectly uniform layer of golden-yellow iodide of silver is obtained, and what is of very great importance, in a space of time which does not vary, unless indeed there be very great changes in the temperature. In my apparatus the operation lasts from 60 to 70 seconds.

It has been said in Paris that metallic edges are necessary for obtaining a uniform layer of iodide; but this is just as incomprehensible as it is erroneous: the silver plate may be exposed to the vapours of iodine, either by itself or in any other way the operator may think fit.

For many experiments it is desirable to increase the sensibility of the iodide of silver, and the chloride of iodine seems to be best suited for this purpose. If prepared and used in the manner I am about to describe, the objections which have been made to it on the score of its insecurity, will be found quite invalid. A small quantity of iodine is put into a wide-mouthed glass-stoppered bottle, and this is inserted *open* into a larger jar which contains chloride of lime. A little sulphuric acid is poured upon the chloride, and the larger vessel closed and allowed to stand for two or three days; at the end of that time it will be found that the smaller bottle contains either one or other, or perhaps both the chlorides of iodine. If the plate of silver, after having been exposed to the iodine vapours, be moved over this chloride for 20 or 30 seconds, it will then yield images in a few seconds, and always in the same time if the intensity of light remain the same, so that there is no insecurity at all.

If the prepared plate be exposed a sufficient time in a camera obscura to the action of light, an image is produced in which those parts are light which in nature are dark, and *vice versâ*, because the iodide of silver retains its original colour on those spots where the light did not act. This kind of picture, which is perfectly useless in an artistical point of view, I shall hereafter denominate "negative," as Herschel has already proposed.

Daguerre's discovery is, that before the negative image has been formed, at a time when no kind of action can be seen to have taken place on the iodide of silver, a certain kind of influence has been exerted in such a manner that those parts acted upon by the light have acquired the property of condensing or precipitating the vapours of mercury. The first point that interests us is the fact, that light can produce in iodide of silver modifications which are not rendered visible to our eyes by ordinary means; and in this respect the beautiful discovery of Becquerel, jun. leads us a step further. If the vapours of mercury are to be condensed by the iodide of silver, it is necessary that the action of the light shall have continued for a certain time; if this period has not been allowed, no mercury is precipitated, and it might appear as if the plate had undergone no change whatever. Becquerel, however, exposed a plate thus treated to the sun, under a red glass, and he then obtained the positive image formed of the mercurial vapours, when the action of the light had not been too long continued; when, however, the plate had been allowed to remain under the red glass above a certain period, the negative image was produced. From this it follows, that although the camera obscura had acted only for a short time, its influence on the iodide of silver had taken place, for the red rays were capable of continuing the action.

Proceeding from the assumption that the chemical effects of light are combined with the violet and blue rays, Becquerel believes that the above-mentioned fact proves the existence of two distinct kinds of chemical rays, and that it is therefore necessary to distinguish between,

1. Exciting rays (*rayons excitateurs*).
2. Continuing rays (*rayons continuateurs*).

The blue and violet rays belong to the first class, and the red and yellow to the second.

These experiments I have on the whole been able to confirm. A plate which required eight minutes' exposure in the camera obscura, in order that it should give the proper image with mercurial vapours, was taken out at the end of one or half a minute, and laid under a red glass in the sun for two minutes. On being exposed to the vapours it gave a perfectly correct image. I mention the spaces of time merely for the sake of examples, inasmuch as no general rules can be given on this point; and I will only add, that when I interrupted the action of the camera obscura too soon I obtained no correct image, although my red glasses allowed very few blue or violet rays to pass through them. If the plate lay longer under the red glass, a negative image was produced, and this circumstance can often be advantageously employed to determine whether any action has taken place on the iodide of silver without having recourse to the vapours of mercury.

Very different, however, are the effects of *yellow* glasses. I possess some which exhibit a pure yellow, and others which have a reddish tinge. The latter act like the red glasses, only they allow a large quantity of white light to pass through, which blackens the iodide. If a prepared plate be covered with a yellow glass, after it has been in the camera obscura for about the proper time, and be then placed in the sun, a curious phænomenon is observed. While the plate exhibits at first no image whatever, a negative one is now rapidly produced; this vanishes, and in about 10 or 15 minutes a positive image is formed. Under a yellow glass these positive images always have a blackish covering, but with this exception are perfectly clear and well-defined. I could never obtain these positive images under red glasses, even by the most continued action of light; but they are easily produced by *green* glasses, although not so quickly as by yellow ones.

If we attentively consider all these facts, we shall arrive at a conclusion concerning the action of the several colours of light, very different from that of Becquerel. The violet and blue rays are the only ones which affect the common iodide of silver, although the action is at first so trifling that it cannot be perceived. Exposure to the mercurial vapours proves, however, that a certain change has taken place. We can distinguish two steps in this modification of the iodide. In the first it is acted on in such a manner that afterwards the red and orange rays affect it just the same as the blue and violet. The yellow rays have not yet the power to act, for if it be taken out of the camera obscura too soon, yellow glasses produce no action. In the second stage of modification the yellow and green rays also have power to act. This is about the moment when the modified iodide of silver has acquired the power of condensing the vapours of mercury. At this time then, *all* colours can exert their influence on the iodide, and consequently it would not be correct to speak of rays in the spectrum which act only chemically. What follows will form a still stronger objection to this general opinion.

The action of the violet and blue, and at a later period of all the other rays of the spectrum, is to blacken the yellow iodide of silver. If this state has once been produced, the blackened salt is no longer affected by any of these rays; their influence is at an end. There are many opinions concerning the nature of this black body; some consider it as pure metallic silver, others as a lower iodide of silver, a sub-iodide; while others contend that even when the light has exerted its influence for a length of time it produces only an isomeric change in the iodide of silver; this view appears to me the most probable. It will hereafter be more fully demonstrated that this black body is not pure silver. At present it is sufficient to mention the fact that the yellow rays produce a positive out of a negative image, that is, they decolorize the blackened silver, which is sufficient to prove it is not pure silver, with which no such phænomenon can be observed. It would be highly desirable to have this curious body better examined. Now, as the violet, blue, red and orange rays no longer act on the blackened silver, but the green and yellow rays do, we have here another proof that there is no reason to speak of the peculiarly chemical action of one group of rays compared with the other, and I consider my first postulate (No. 1.) as fully proved. If we consider the preceding statements, we shall find, that, in order to examine the peculiar action of the green and yellow rays, it is not necessary to use coloured glasses, inasmuch as these rays only begin to act when all the others can no longer exert any influence. We may therefore use common white, undecomposed light, and we shall thus be led to the following striking experiments, which are easy enough of confirmation.

An iodized plate was laid in the sun until it was blackened, which took place very quickly. Half of it was now protected from the sun; after a few minutes it was evident that the uncovered part was strikingly lighter than the covered portion. This decolorizing process lasts some time, and finally, the plate which was at first black assumes a greenish-yellow tinge when viewed by reflected light. This action depends upon the sun's green and yellow rays, and it is clear that sunlight acting uninterruptedly on yellow iodide of silver, first makes it dark and then restores its colour.

An iodized plate of silver was exposed to the sun until the blackening no longer increased, and was then put into a camera obscura directed towards some houses; after a lapse of twenty-four hours it was removed, and exhibited, as might be expected, a correct positive image. The light parts of the object had in the image a steel-gray colour, the dark parts were black.

The following experiment is very striking, although easily to be explained from the above-mentioned action of the green and yellow rays. An iodized plate is put into the camera obscura on a day when the sun shines, and allowed to remain in it for half or a whole hour, or even longer, so that a strongly marked negative image is produced. The plate is now to be laid in the sunshine, and a very beautiful phænomenon will be observed. After a few minutes the negative image vanishes and an equally strong positive one is produced, in which the bright parts are blueish-green and the dark reddish-brown. From all these experiments it will be seen, that the question as to which colour of light operates chemically, cannot be answered in the abstract, as has been fully shown by the yellow and black iodide of silver. As yet our knowledge of bodies in this respect is very deficient, and for the present we must leave it undetermined whether the law discovered as applicable to the iodide of silver is general, with slight variations, however, as regards this or that colour. On this point I have only examined the alcoholic tincture of guaiacum, which certainly offers very analogous appearances. Papers stained with this solution have at first a reddish colour, which becomes tea-green in daylight. The rays which act first are the blue, green and violet. They colour the red paper blueish-green, the violet rays even blue. If the paper has become green by exposure to daylight, and it is then laid in the sun under cover of red and yellow glasses, it acquires a bright red, somewhat brownish tinge. Red and yellow therefore act on guaiacum which has been modified by the blue, green and violet rays, just as green and yellow on the blackened iodide of silver. And accordingly one might expect that after the sun had coloured the red paper green, it would make it appear reddish if its action were continued, and such is found to be the case. The fact may be confirmed in a very short time if there be strong sunlight; in the camera obscura, however, I obtained the image only after twenty-four hours, and even then the finer details were not very visible.

I will now endeavour to prove my second statement, viz. that the action of light does not necessarily consist in the separation of two chemically-combined bodies. This part of my supposition follows indeed from the discovery of Daguerre, and still more from that of Becquerel, according to which light produces changes in iodide and bromide of silver (the latter substance was experimented on by Becquerel), which are exhibited, it is true, by the mercurial vapours and red glass, but are otherwise quite invisible, and thus the assumption that iodine is hereby separated from silver is completely refuted. I shall hereafter describe phænomena which teach us to produce effects similar to those of light in these experiments in a quite different manner, *e.g.* upon a pure surface of silver, where there can be no possibility of a chemical action.

This refers solely to the primary modifications which light produces on the surface. Its continuous action is exhibited externally as a blackening of the iodide or other compound of silver, and this modification is no doubt considerable; but it still does not admit of the assumption that light really does produce chemical decomposition. In order not to be misunderstood, I will remark that the other chemical effects of light, as for instance on chlorine and hydrogen, on oxide of gold, some salts, nitric acid, &c. &c., do not interest me, for,—1st, many of these chemical effects probably deserve and indeed require an entirely new investigation in the present state of the science; and 2ndly, the principal point here is to prove the absence of chemical changes in one single well-known case that can be easily examined, in order to allow us to suppose that the effect of light on the substance of the retina is not considered as a more considerable chemical metamorphosis than it really is.

It has been noticed above, that the blackened iodide cannot possibly be metallic silver, inasmuch as we can produce an image on it. I will now pursue this subject further. If we take an iodized plate of silver, and placing it in a camera obscura, allow it to remain there twenty-four hours, we obtain, as I have before stated, a positive image, which is therefore the *second* that has been formed upon the plate, for the first is a negative image. The positive one exhibits in reality only shades of gray, and I have never seen colours produced under these circumstances. We may then naturally ask, whether this second image be the last, or whether a third negative, then a fourth, &c. can be formed, if we allow the light sufficient time to exert its full influence.

For the elucidation of this point two plates were iodized, one of them was exposed to the vapours of chloride of iodine, and then each put into a camera obscura. The weather was very unfavourable, it being winter, and the sun scarcely shining at all; each camera obscura was placed in a darkened chamber, in order to exclude all light from the sides, and was directed towards distant houses. The plates were taken out at the expiration of thirteen days, and both exhibited a correct positive image. That one which had been exposed to the chloride of iodine exhibited a most beautiful appearance; the light parts were of the brightest sky-blue, and the dark spots of a fiery red colour. The other image possessed the same colours, but not near so bright. I have not the slightest doubt that this was still the second image, for by such bad light a fourth could hardly be expected, even if it be ever formed. But this experiment allows us to go one step further. It teaches us that the blackened iodide of silver had changed into the coloured variety, solely by the continued action of light. It was found that this iodide was similar to the original coloured salt in other respects. The original unaffected iodide of silver is easily dissolved by a solution of hyposulphite of soda, as Herschel has observed, but not so the blackened iodide. On introducing the above-mentioned plate into a solution of this salt, the coloured coating was rapidly dissolved, and a negative image remained behind. This is a clear proof that the continued action of light brought back the blackened iodide to its former state, and that this black body cannot well be a substance differing in any very great degree from the common coloured iodide of silver. As these experiments require the entire use of the instruments, I have not been able to carry them out in this manner; moreover, I was prevented from so doing by the suspicion that even after exposing them for months to this treatment I should not be able to determine *which image* the plate really did possess.

In order to arrive at a conclusion concerning this subject in a more convenient manner, I exposed iodized plates to daylight, and whenever possible to strong sunlight. The plates became first black, then lighter, and lastly greenish. After a lapse of fourteen days they appeared to become dark, and after a still longer time light again, and I have observed these alternations take place five or six times. In order to observe the changes more easily I placed strips of dark paper on the plates from time to time, by which certain parts were protected from the influence of the light; moreover, I always observed the plates in the same position with regard to reflected light, because otherwise light often takes the place of dark, as may be seen in the well-executed images of Daguerre. But the result of these experiments became more convincing to me by facts to be mentioned hereafter. I have no doubt that a well-conducted experiment will exhibit all these alternations in the effect of light; it will, however, be necessary to protect the plates from dust and moisture better than was the case in my trials, where they appeared to exert a deteriorating influence. An experiment made by Draper (Philos. Mag., Sept. 1841) is greatly in favour of the idea that light does not drive off the iodine from the plate. He saturated paper with a solution of starch and laid it on an iodized silver plate. When the sun shone through, the plate became dark green, but the paper did not evince a trace of a blue colouring. From all these facts I consider it as fully proved, or at least as highly probable, that light produces no decomposition of the iodide of silver, and refer to the classes of phænomena which will be hereafter described.

I will now return to the proof of my third assumption, that the longest continued action of light only affects the most external surface of the iodide of silver, although the whole layer is so excessively thin. A yellow prepared plate was exposed for two months to daylight and direct sunlight; it was then rubbed with dry cotton, placed in the sun, and the shadow of a neighbouring body thrown upon it. In a short time the rest of the plate had become evidently dark, while the part where the shadow rested was light. The plate was now polished again, and placed in the sun with the same success. I operated on it thus eight times, and the experiment is not yet completed. After every polishing or rubbing it is seen that there is still sensitive iodide of silver equally diffused over the whole plate. The result of this experiment is extremely curious, if we consider how extremely minute the original layer of iodide is. Dumas states, without however explaining his method of measurement, that its thickness is less than one-millionth of a millimetre. Notwithstanding all the manipulations which are necessary to produce an image according to Daguerre's plan, I have executed thirty images in succession on common plated copper; the plate then certainly began to show traces of projecting copper, but only single red spots, while the remainder of the plate was still in a perfect state.

I will now mention certain objections which might be made to the proposed hypothesis concerning the action of light on the retina. They are objections which might certainly be raised, but which, when we observe them more narrowly, will be found to be rather confirmations, and are partly founded on phænomena which it was quite impossible to explain according to the usual view.

In the first place, one might compare the sensations of the eye with those of the ear, and consider *oscillations* as the regular cause in both instances. But without considering that in the one case we have to deal with vibrations of ponderable matter, and in the other of imponderable, there is in point of fact no analogy between the impressions of the eye and those of the ear; for if we look only to the outward causes which excite both the organs of sense, we should expect to find an analogy between the different colours and the deep and high tones, and yet there is nothing less analogous, at least as far as we can see. Colours produce a perfect impression (if we may be allowed the term), which cannot be confounded with each other, while the tones effect nothing of the kind. We certainly should not confound together very high and very deep tones, but this occurs very easily with an ordinary ear in the case of closely approximated tones, and in any case it requires a particularly fine and musical ear to determine at once a tone as commonly designated, while the eye encounters no such difficulty in the determination of the colours. We should be much more inclined to compare the depth or height of a tone with the intensity of one particular colour, while the different colours themselves might be likened to the quality (*timbre*) of the tones. The perceptions of these two organs of sense are therefore very different when similar outward causes are acting, and do not allow of much comparison; if, however, this must be made, it then only appears that the act of vision must be quite different from that of hearing, and that while in the one case thousands of oscillations in a second are counted, in the other hundreds of billions probably cannot be.

A more important objection might be made in the following manner. If, for the sake of simplicity, we leave out of consideration the green and yellow rays, which exert a peculiar influence on the iodide of silver, and only take notice of the violet, blue, red and orange, they all appear to have the same effect on the iodide; they reduce it to such a state that it is capable of condensing mercurial vapours. All matters sensitive with regard to the action of light, with which we are acquainted, exhibit in the same manner a similarity in the actions of *several* colours. From this we might be induced to ask why it is they all act differently on the retina, to which they appear as various modifications? We might perhaps here be allowed to hint that our knowledge of the action of light on iodide of silver is too scanty, that we have only lately learnt the use of mercurial vapours as a test for it, and that there may probably be other means which, when employed, would show whether the action exerted has proceeded from one colour or the other. Moreover, it is not at all necessary that that which is correct for iodide of silver should also be so for all other bodies, as for instance, the retina; and that guaiacum exhibits a sensible difference between the action of blue and violet rays, inasmuch as the former turns it blueish-green, the latter blue. But even here it is not necessary to seek for an excuse in our want of knowledge. Seebeck found that chloride of silver is variously coloured in the different rays of the spectrum, and every observer who has made experiments on the action of light on any substance whatever, will have been astounded at the number of tints presented to his view under certain circumstances.

These tints are proofs of so many modifications which the substance can assume under the influence of light, and then the supposition that all colours act in the same manner, is quite untenable. It would be difficult to make experiments on this subject, as it is difficult to operate with pure colours; but we must accede to the hypothesis that different colours act differently. There certainly is in one case a similar action in all colours, yellow and green included, viz. when they act for a length of time they then endow the iodide of silver with the power of condensing mercurial vapours, and by still continued action they blacken it. We have here then decidedly the same action in all the colours of the spectrum, and it is a question whether or not anything similar takes place in the eye.

If there be any convincing proof of the correctness of my view concerning the process of vision, it certainly is the fact that exactly the same thing takes place with the eye, viz. that by continued action all difference of colour vanishes, and what remains of them is merely a general impression of light. This is partly seen when we attempt to look at a brightly coloured object for a length of time, on which more will be said in the next part of this paper. But a clear evidence of the correctness of this view is an experiment for which we are indebted to Brewster, the result of which may be easily confirmed.

If we observe for a length of time the spectrum of the flame of a candle through a prism, first of all the red and green parts vanish, and also a little of the blue; if we still continue to look at it without moving the eyes, the yellow disappears, and passes into white; so that at last, instead of the prismatic colours, we only see a uniform white, lengthened image of the flame. This experiment may be performed without the slightest difficulty, and, as I have found, most easily, if the upper eyelid is retained in its position by the hand, and prevented from dropping down. If the white image has been obtained in about half a minute, and the eyelid is allowed to drop, and the eye immediately reopened, the spectrum appears for a moment with all its colours, and then changes rapidly into the white image. Here then we have exactly the same effect produced by all the colours of the spectrum, as was found above to be the case with the iodide of silver.

Brewster's experiment is so striking a confirmation of the theory in question, that we may well ask how the usual opinion concerning vision can explain it. Without doubt the complementary colours would be called in to aid the explanation; it would be said, that with the primary impression of any colour, the complementary is formed after some time, which combines with the primary colour and forms white. This, however, can hardly be called an explanation; for if we consider colour according to our ideas as a determinate number of oscillations of the æther in a certain time, we cannot attach any precise meaning to such an hypothesis. The theory of the action of complementary colours on the eye, a subject which will be treated of more fully in the next part of this treatise, seems to me to have been only an invention of necessity, in order to bring under it the subjective colours, while, according to our view of the subject, these phænomena become easily explicable without the assistance of a peculiar vital principle. The general opinion, and more particularly in France, at present entertained, is that there are peculiar chemical rays which are different from the luminous ones. My hypothesis as to the process of vision could not possibly stand, if such a theory were admitted as correct; but as far as I know there is nothing in favour of this hypothesis except the existence of non-luminous, so-called chemical rays. I am not aware of any other phænomenon which tends to support it, and certainly no one will seriously adduce as such the experiments on the passage of chemical rays through certain bodies. The best way of determining whether there is any necessity for making a distinction between chemical and luminous rays, appeared to me to be the examination of the action of polarized light on sensitive bodies. I have never observed any appearance of this kind in the most varied experiments; and if we allow the existence of chemical rays, we must at least admit that, as far as regards polarization and interference, they follow the same laws as the luminous rays. By means of Iceland spar I was enabled to obtain images similar in every respect to those of Daguerre.

For this purpose I placed before the lens of the camera obscura (the lenses are best if with short focal distances) an achromatic prism of calc-spar, and adjusted it so that both images of the object were apparent. The best object that can be chosen is a statue, for if buildings are taken the parts overlap. When achromatic calc-spar is used, only the ordinary image is colourless, the other is furnished with coloured edges, which, however, in my experiments did not injure the clearness of the image. Moreover, I obtained a representation of the annular system of calc-spar, which is depicted in a very beautiful manner. I generally made this experiment by fastening, just before the lens of the camera obscura, the common apparatus, consisting of calc-spar, with two parallel faces, standing perpendicular to the axis between two Nicols rhombs, and directing the whole towards a clear sky. Moreover, I succeeded in representing the figures of rapidly cooled glasses, sometimes in such a manner, by placing a prism of calc-spar between the glass and the lens, as to obtain both complementary figures at once. These experiments were completely successful, and prove very evidently that if there are chemical rays they are polarized in precisely the same manner as the luminous ones. For instance, the black cross in the image, produced by a quickly-cooled cube, shows that the chemical rays are just as little reflected from the cross mirror of the polarizing apparatus as those that are luminous.

In repeating these experiments it may perhaps be desirable to know the time necessary for their performance, so that there may be some guide by which the operator may be directed. The lens of my camera obscura had a focal distance of 3·90 inch. and an aperture of 0·501 inch. By means of a prism of calc-spar, when chloride of iodine was used, and the object was a white bust placed in the sun, the double image was obtained within a minute. The annular system was depicted on iodide of silver without the assistance of chloride of iodine, and on a dull day in the space of two hours and a half,—the cooled glasses in about the same time. We see, therefore, that sometimes hours are necessary for these experiments, if the intensity of the light is very small, and chloride of iodine has not been employed. It is therefore necessary to perform the experiments in a darkened room, and to take care to exclude all lateral light.

As we may conclude from the foregoing experiments that the chemical rays do not exhibit any difference from the luminous rays, as far as regards reflexion, refraction, interference, and polarization, we cannot possibly allow the assumption of their being a peculiar class of rays. The matter would stand in a quite different light if we were to adopt the view which I have given of the action of the rays on the retina. It has been often stated that we must consider a luminous body as one from which are emitted rays of the most various kinds, possessed of the most different rapidities of translation and oscillation. The fact that the light of those stars towards which the earth is moving, possesses the same index of refraction as that of stars from which it is receding, led Arago to this theory a long time since*. We must thus conclude, that of the various rays of a luminous body, one particular system acts on one sensitive body and another system on a different body, and that the retina itself must be numbered among these sensitive matters. There is a certain system of rays which act upon it alone, and which produce the impression of colour. This system is not more extended than it is with respect to other substances, for we have seen above that there is an iodide of silver which is affected by all colours just as well as the retina. If this be the case we could not deny the possibility of the existence of *non-luminous* chemical rays; they would be such as do not act on the retina, but exert an influence on other sensitive bodies. There is nothing *à priori* objectionable in this. Ritter, as is well known, observed the existence of such dark rays in the action of light on chloride of silver, for he noticed a blackening above the violet of the spectrum; and Wollaston, Seebeck, and others, have confirmed this statement. I myself have tried the experiment in a darkened room, without however arriving at any very great degree of certainty. The limits of the violet cannot be determined so easily; and there is no doubt that they would be different for different eyes, or for the same eye in different circumstances. Moreover, unless a heliostat is used we must keep moving the paper, which must diminish the confidence to be placed in the experiment. However, I did not take any very great care when making these experiments, for at that time they did not interest me so much, and therefore I will not on their account throw the least shadow of doubt on the existence of non-luminous and yet active rays, which, as above stated, is perfectly immaterial for my theory either one way or the other. Herschel† considers it possible that some animals, viz. insects, do not receive an impression from any of those colours which are visible to us, but are affected by a species of oscillations which lie beyond the limits of our senses. Wollaston‡ adopts the same hypothesis with respect to their sense of hearing.

I will now direct my attention to the very interesting question concerning the *sensibility* of the retina to the action of light,

* Poisson, *Traité de Mécanique*, 2me édit. tom. i. § 168.
† On Light, § 567.
‡ Phil. Trans. for 1820. London and Edinb. Phil. Journal, vol. vii. p. 158.

compared with that of other known sensitive bodies. This sensibility may be regarded in two points of view:—1st, with regard to its degree, and 2ndly, as to its variability; for the eye unquestionably possesses the power of adapting its sensibility to the intensity of the light, and it must also possess means of restoring this sensibility after it has been taken away. I intend to show that in both cases there is nothing in which the retina differs particularly from other substances subjected to the influence of light.

In the first place, as regards the degree of sensitiveness, it certainly is extremely great in the case of the retina; but we need not consider it immeasurably greater than it is in the case of the silver compounds. It has been already stated that the exposure of the iodide to the vapours of chloride of iodine increases its sensibility very greatly (according to my experiments the time appeared to be reduced to one-tenth of what was otherwise requisite). Gaudin * has lately said that the vapours of bromide of iodine may be still more advantageously employed; and he states that by its means he has obtained images in the space of one-tenth of a second. We must not however forget, that in order to procure an image according to Daguerre, we make a certain demand on the light, viz. to bring the iodide of silver into such a state that it will condense the vapours of mercury. This is a special demand, which, if it is to be fulfilled, requires that the plate should remain a certain time in the camera obscura. The light, however, has acted much earlier, which could be proved if we possessed reagents efficient for the purpose. Red glass is one of these; it shows us that long before the plate is usually taken out of the camera obscura the image existed with all its shades and details; and without doubt we shall hereafter discover other means by which we may prove the existence of the image in its earlier stages of development.

The period at which we make use of the action of the light cannot in reality be compared to the time in which external objects become visible to the retina. It is analogous to that state of the retina in Brewster's experiment, when the colours of the spectrum disappear, and there only remains a uniform impression of white. This is an unnatural state, which can only be attained after continued observation for a half or whole minute. If, therefore, we wish to make a comparison between the

* *Comptes Rendus*, hebd. 18 Octobre, 1841.

iodide of silver and the retina, in respect to the necessary duration of the light's influence, then the time which the plates require to remain in the camera obscura would be analogous to the time required by the retina to pass into the above-mentioned anomalous state; and these periods are commensurable. One of my acquaintances obtained the perfect image of a common candle-flame on pure iodide of silver in the space of two minutes, Draper that of an Argand gas flame in fifteen seconds, and without chloride of iodine. These are by no means much greater spaces of time than the retina requires to assume a similar state. These periods will certainly become greater when the intensity of the light is diminished; but there a comparison with the retina is not possible, because it cannot be brought into that anomalous state at all, if the colours do not possess a considerable degree of intensity.

It is the general opinion that ordinary vision does not take place instantaneously, but that it requires a definite though certainly very minute space of time to bring it into action. This is of itself very probable; and Fechner concludes that the strongest impresssion on the eye only takes place after the lapse of a certain time, from the fact that a black spot on a rapidly revolving white disc leaves behind it a dark circular track, and also a white spot on a black disc leaves a light mark *. The same philosopher explains an analogous phænomenon visible with coloured discs in a precisely similar manner †. It is just the same with the retina as with the sensitive papers, which are affected very rapidly at first, as I and others have found, indeed quicker than the iodide of silver, but which require a long time before they exhibit the after phænomena which I have described above. This is also the case with the retina, inasmuch as it produces the after images analogous, as I shall show, to those later appearances, only after the light has acted for a length of time.

Concerning the second point, viz. the variability of sensitiveness, there is no doubt that the eye is capable of producing it, although, according to the usual theory of vision, when the oscillations were traced to the retina, and the consideration of their action then dropped, this point has never become the subject of discussion. A strong light at first stuns the eye, as it

* Poggendorff's *Annalen*, Bd. l. S. 202.
† Fechner's *Repertorium*, Bd. ii. S. 213.

were; it closes, and it is only after a greater or less space of time that we are enabled to see by this light. On the other hand, the greater degree of sensibility for light of low intensity is acquired only after a certain period, as is well known. I have already mentioned, when describing Brewster's experiment, that when, by continued observation, the colours of the spectrum have been converted into white, they appear again immediately for an instant, if the upper eyelid be allowed to drop. It is possible that this motion produces a certain degree of pressure, and this generally causes a displacement of the ocular lens. We involuntarily press an eye that has been strongly dazzled; if we have an after image on the retina which is on the point of vanishing, a pressure on the eye, produced by the external muscles, generally serves to reproduce it. The pressure of the fingers produces luminous appearances; the stronger effort, as is well known, a fiery circle, the less degrees colours; and, as Prof. J. Müller states, it is possible to change one colour into the other by varying the degree of force. The usual description is not sufficient for these phænomena, at least it does not offer an explanation of them; for it might then be asked why pressure does not produce analogous effects on the organs of smell and taste, not to mention those of hearing, in which something takes place similar to the actions of the retina, but which allows of a different explanation.

In adapting the eye to different intensities of light, the pupil itself certainly takes a part; but still only in a small degree. Its changes take place within much too narrow limits for it to be able to regulate the comparatively large variations of intensity. And, moreover, the pupil alters when the intensity of light remains precisely the same. When Olbers*, for instance, measured the diameter of his own pupils in a mirror, he found it to be

$2{\cdot}01'''$ at a distance of $4''$, and
$2{\cdot}74'''$ at a distance of $28''$.

The opening of the pupil had increased, therefore, in the proportion of 1·86, although the observed object retained the same intensity of light. According to Lambert the changes are much greater †. It has been proved that the expansion and contraction of the eye are in general destined for other purposes than

* G. R. Treviranus, *Anatomie und Physiologie der Sinneswerkzeuge*. Bremen, 1828, Heft I.

† *Photometria*, &c. § 853.

those here mentioned, and we must therefore seek elsewhere for the means by which the sensibility of the retina is modified, *i. e.* increased or diminished, and by which the sensitiveness which has been reduced may be brought back to its normal state.

In the present research I was partly led by the idea that pressure might be the means in question; and I was thereby induced to try whether something similar might not be the case with other bodies sensitive to the action of light. I have succeeded in discovering a series of facts, and some general laws, which it appears to me may be classed among the most extraordinary phænomena in this department, and which promise to extend our views. I will describe these facts in the same order as they presented themselves to me, which plan may perhaps give the best general view of the subject.

It is a well-known fact, that if we write with certain substances on a well-polished glass plate, and then clean it, the characters always make their appearance again by means of the aqueous vapours, if we breathe over it. The experiment has also been made by placing a coin on a glass plate, breathing on it, and then removing the coin; if the plate was afterwards breathed on the figure of the coin appeared, although certainly only in outline.

This is all that I have found of this particular kind. But the experiment has been made much too confined; we may write on the glass plate with any substance whatever, the after-phænomena always remain the same. We may first breathe uniformly over the whole plate, and then write on it either with blotting-paper, a brush, or anything else; the characters will become visible whenever the plate is breathed on, and this phænomenon lasts for some time. Not only is glass applicable to this purpose, but every other polished body exhibits the same appearance: I have tried it with metals, resins, wood, pasteboard, leather, &c. Even fluids may be used; if we take a very clean and still surface of mercury, hold over it a body, and breathe on the other parts, or what is better, breathe on the whole surface first, and then remove the moisture by any gentle means from particular parts, they will again become visible when breathed on, even after several days, if the mercury remain undisturbed. Moreover, absolute *contact* with the extraneous body is not necessary, whether it be before the breathing, or only to remove the precipitated moisture from particular parts. If we hold over a polished body a screen, part of which has been cut out according to pleasure, but without allowing it to touch, then breathe on the whole, and allow the water to evaporate, we shall find that on breathing on it again we shall be enabled to distinguish fully the figure of the excised parts: and still further, it does not require a polished body, inasmuch as *dull* glass exhibits the same appearance.

These phænomena are therefore widely diffused, and it seemed not altogether impossible that, if followed up, they might become of importance to the object in view; for it is perfectly clear that the surfaces of the bodies must have undergone some kind of change. I was of opinion that the differences of temperature might exert some influence, and this idea appeared to be correct. An engraved metallic plate was warmed, and then held for about half a minute on a well-polished piece of silver foil, or a clean mirror plate. When the plates were cold they were breathed on, and exhibited the above-mentioned appearances in a much more perfect manner; for not only were the outlines of the body visible, but also the individual figures, letters, &c. and all with the greatest distinctness.

Frequently silver or other metallic plates were made warm, and cold bodies, variously cut stones, figures of horn, pasteboard, cork, coins, &c. allowed to remain on them for some time. The phænomena were all the same, and I did not observe any difference in the employment of these different bodies, either in these or in the succeeding experiments.

As I was now able to obtain definite images on many bodies, I examined the plates with respect to vapours of another kind. A plate of silver, prepared as above, was exposed to the vapours of mercury, without having been previously breathed on; the mercury was exposed to a temperature of 60° R. The mercurial vapours did exactly the same service as the vapours of water, but much more perfectly: a very beautiful perfect image of the object employed was obtained. I now tried vapours of iodine, which were more particularly interesting, inasmuch as they combine chemically with the silver, whilst the vapours of mercury are only condensed by the cold plate, and adhere to it without combining with the substance of the silver. A similarly prepared plate was iodized yellow, and now indeed I saw in some cases the definite image of the objects by means of different colouring; in other cases the image was not yet visible.

I now introduced these plates into the vapours of mercury, and then the perfect image became visible; that is to say, Daguerre's phænomenon was produced without the intervention of light, for the experiments succeeded just as well by night as by day. Other similar plates, which after being iodized did not exhibit any figures, were laid in open day- or sunlight, when the representations of the objects employed quickly made their appearance, and indeed depicted with the greatest delicacy. I cannot possibly describe here all the experiments that I made on this subject; I should by so doing far exceed the limits of this paper. I will only mention that the vapours of chlorine and chloride of iodine perform the same service; that the phænomena may be produced on blackened iodide of silver, on copper, steel, &c. although I was not capable of giving the latter body any very good polish. I have also used platinum and black mirror glass for experiments of this kind, not only with the vapours of water, but also with those of mercury, which are condensed and adhere to them.

The result of these experiments is of considerable importance. The discovery of Daguerre consists, in a physical point of view, in the knowledge of the fact that the action of light on iodide of silver produces a state in which it is capable of condensing the vapours of mercury, which adhere to those parts affected. This peculiar action of light stood so far quite isolated. Here we have the same action, only much more general: by the contact of a clean or polished surface with a heated body, or *vice versâ*, by the contact of a heated surface with a cold body, particular parts of this surface are brought into such a condition that they are capable of condensing all kinds of vapours, and of causing them to adhere. And it must be remarked, that even when the vapours combine chemically with the substance, as for instance iodine with silver and copper, this state produced by contact makes itself equally visible; partly by a different colour, which proves a different degree of combination; partly so, that when exposed to light or the vapours of mercury, the image becomes clearly developed. In fact, when a plate has been in contact with a body it may be afterwards exposed not only to vapours of iodine, but also to those of chloride and bromide of iodine, and yet the image is produced in light, or in the vapours of mercury.

The action of light was therefore imitated and extended by my experiments; and indeed, as it appeared, by the employment of unequal temperatures. But this latter view could not possibly be long entertained; it is only necessary to observe one of the above-described images, if well executed, in order to be convinced that such representations, in which the finest lines of the original may often be traced, could not possibly be produced by differences of temperature, more particularly on a thin, well-conducting plate of metal. Moreover, the variety of the substances employed forms a great objection to this view of the subject. It was therefore necessary to try whether the same phænomena could not be produced without the application of heat, and in this I succeeded. For this purpose I kept all the bodies used for the experiments in the same closed space, then allowed contact to take place for ten minutes, or even for several hours, and afterwards exposed the plates to different vapours; and in this manner I obtained precisely the same appearances with the same degree of accuracy. Sometimes the application of heat did appear advantageous, more particularly in the case of glass; but then only because the contact took place more fully, moisture and other adhering gases being driven off from the body.

The fact discovered may be expressed as follows:—"If a surface has been touched in any particular parts by any body, it acquires the property of precipitating all vapours, which adhere to, or which combine chemically with it, on these spots differently to what it does on the other untouched parts." By this the representation of the body in contact is produced.

I have been obliged to use one ambiguous expression in the statement of this law, inasmuch as I cannot tell whether the vapours are condensed by the touched parts more or less strongly than by the untouched portions. This point cannot be settled in this case any more than with the plates of Daguerre, and I shall shortly return to the true and remarkable behaviour of vapours in this respect.

The preceding experiments seem to show that contact is capable of imitating the action of light; and this fact is clearly proved by the following experiment. A silver plate was iodized during the night, and even without the light of a candle; a cut slab of agate, an engraved metallic plate and a ring of horn, &c. were then laid upon it, and the plate was afterwards introduced into the vapours of mercury. A good, clear picture of all the figures, of the stones, the letters of the plate, and of the ring was obtained. A plate which had been treated in the same manner was exposed to day- or sunlight, and similar pictures were produced. Other plates of the same kind were placed under coloured glasses, yellow, red, and violet. Under the first two only a trace of the image was evident, but under the violet glass it was clearly defined. This result might have been foreseen; for the modifications which are produced by the contact of plates is analogous in most cases to the first stages of the action of light, in which the red and yellow rays are of no effect, as has been already shown. We see, therefore, in these experiments, that the violet rays *continue* the action commenced by contact, and we might ascribe to them a continuing power, the same as Becquerel has given to the red rays, if I had not already sufficiently proved that the whole difference between exciting and continuing rays is altogether deficient in real foundation.

In the above-detailed experiments an iodized silver plate had been brought, by means of contact, into the same state as it is by the influence of light; as stated above, it is possible to produce these appearances on other surfaces, *i. e.* on a pure plate of silver. Consequently we have advanced much further in the knowledge of the phænomena produced by contact than of those relative to light, whose influence upon simple or difficultly changeable bodies, as for instance glass, has never been proved. It was therefore necessary to examine the action of light on such bodies.

A new plate of silver was cleaned and polished as well as possible. A surface with various excised characters was suspended over it without touching, and the whole was exposed to the sun for some hours, and directed towards it. After the plate, which of course did not exhibit the least change, had been allowed to cool, it was held over mercury heated to about 60° R. A clear image of the screen was produced; those parts where the sunlight (which had been very weak) had acted, had caused the deposition of a quantity of mercury. This interesting experiment was repeated several times with like success; sometimes the plates, after having been held in the mercurial vapours, were exposed to those of iodine, and then placed in the sun, by which the images often improved.

If we compare this remarkable fact of the action of light upon surfaces of silver with the above-mentioned phænomena produced by contact, we can no longer doubt that light acts on *all* bodies, modifying them so that they behave differently in condensing the vapours of mercury. In order to render this more plausible, I made a similar experiment with a plate of copper during very unfavourable weather. I had not succeeded in giving a good polish to the copper, and consequently the image produced by the mercurial vapours was slight, although distinctly visible. On exposing the plate to vapours of iodine the image became very strong, and this method I found very advantageous in experiments with copper.

Finally, I examined the action of light on mirror-glass just in the same way; in this case the action was quite as plain as on the plate of silver, if the glass was afterwards breathed on; and for a long time subsequent I always saw the image produced in this manner. If we admit of generalization, the proposition would stand as follows:—*Light acts on all bodies; and its influence may be tested by all vapours that adhere to the substance or act chemically on it.* There is no necessity for restricting ourselves solely to those vapours which I have examined; without doubt hydrofluoric acid might be so used as to exhibit the action of light on glass just as well, and at the same time to corrode the parts affected. I have not occupied myself with such experiments, as it appeared of more interest to search for the primary laws.

We now see that the discovery of Daguerre is a special case of a very general action; for this instance only teaches us the action of light on iodide of silver, as proved by the condensation of vapours of mercury.

Before proceeding further, I thought it necessary to examine the action of the different coloured rays on simple substances, at least in one instance. Nothing can be said concerning it *à priori*; for I have shown above that we cannot ascribe an exclusive chemical activity to the blue and violet rays. My introductory experiments have shown that only the rays of the latter kind exert any influence on pure silver, for I obtained very clear images by means of glasses of these colours, while only traces could be rendered visible when red glasses were employed, although they transmit more light and heat. However, the weather was exceedingly unfavourable while I was making these trials, so that I was obliged to give them up for the time.

I will now describe the manner in which the different vapours are condensed by the plates. My first experiments with the breath tended to discover whether the untouched parts, the deepened figures of an engraved stone, appeared lighter or darker. If they appeared dark the vapours were precipitated exclusively or for the most part on the touched parts, and the contrary if they appeared white. The phænomenon seemed, however, to be complicated and irregular; for sometimes, by means of the breath, I obtained light and dark figures, and sometimes both together; and finally, from one and the same figure, at first a white image, and on breathing stronger a dark one, and then again a white one. Mercury exhibited similar complicated appearances; it was deposited principally either on the touched or the untouched parts; oftentimes it could be wiped off, at others it was dry, at least it could not be removed by rubbing; and, finally, the vapours of iodine were irregular in the extreme. The parts of the silver that had been touched, appeared, after iodizing, sometimes darker and at other times lighter than the untouched parts. When brought into the light, either of them first became blackened. Once, when brought into the light, the letters of a seal were at first black, then the surrounding parts became darker and the letters lighter; then again the phænomenon was reversed, and the letters were darker, and remained so for several days. This inversion may be easily explained from what has already been said concerning the alternating action of light, and would be comprehensible if the deep figures of an engraved stone, which lay upon the same plate as the seal, had exhibited the same phænomena; but they did not. These figures, although in every respect analogous to those of the seal, were lighter than the surrounding parts when first exposed to light, then they became darker, and remained so for several days. This may suffice to prove the complexity of these appearances; the attempt to unravel the mysteries cost me considerable pains, by which other facts were brought to light, which increase still more the strangeness of the condensation of vapours. The vapours of mercury seemed to me to be best adapted for the examination of this subject; they act with great delicacy, and may be employed at any degree of tension. It was not exactly necessary to examine images produced by contact; the common ones of Daguerre do not differ from them in any particular, and consequently they must lead to the same result. Iodized plates of silver, frequently those that had been exposed to chloride of iodine, were allowed to remain the proper time in a camera obscura, and then held over mercury, which was gradually warmed. In this way the following facts were discovered.

It will have been seen from the preceding general laws, that it is not necessary to place the plate at any particular inclination to the vapours of mercury, *e. g.* that of 45°, as is always stated; the plates may be placed according to pleasure if the vapours can reach them. I have also discovered a fact, of which it is necessary to be aware in these experiments, viz. that there is no action of mercurial vapours of a high tension which cannot be produced by those of a low one, if the times be proportioned. In forming an image according to Daguerre, it is not necessary to heat the mercury; if the plate, when taken out of the camera obscura, be placed above mercury and allowed to stand for some hours, the picture will be produced with the same exactitude. The complicated actions of mercurial vapours that I shall have to describe succeed equally well when a high or low degree of tension is employed, only in the latter case they require a much greater length of time. It is important to know this fact, for in the previous experiments, when the mercury has been heated once, sometimes no image is formed; if the plate be then allowed to stand over the cold mercury for 12 or 24 hours, and the operation has been well conducted in other respects, a correct picture will make its appearance. The true reason of this will appear from what follows.

Let an iodized silver plate, which has remained the proper time in the camera obscura, be brought into the mercurial apparatus, and the mercury gradually heated; the ordinary image will be seen to form when the temperature is about 70° R.*; let the heat be then raised to 100° R. If the picture be now removed from the apparatus, it is *fixed,* while the common ones may be easily wiped off. This picture may be strongly rubbed; it loses a little of its intensity at first, but not afterwards. The images, however, cannot generally resist rubbing with moist materials, or with polishing substances. I have obtained these fixed representations in a very beautiful state, and with great minuteness of detail, even by heating the mercury to 60° as

* The temperatures will vary in the different experiments, according to the manner in which the plate has been treated, according to the mass of mercury and the source of heat, the height of the plate above the mercury, its temperature, &c.

usual, and then allowing the plate to remain for a long time over it. If it be wished to engrave such pictures, the fixed ones would be found well-adapted for the purpose. It is not necessary to remove the iodine by means of hyposulphite of soda, because it may be taken away just as well by rubbing. It might appear as if the mercury in the fixed images not only adhered, but entered into combination with the silver, which, however, is by no means the case, as will presently be seen.

If the mercury be now heated still stronger, the plate acquires a yellow appearance, and above 120° R. the image becomes *negative,* i. e. the bright parts of the object appear dark, and *vice versâ.* If further heated, this negative image becomes fixed; and if it be rubbed we are soon convinced that the mercury has left those parts to which it previously adhered, for these spots appear quite bright; on the contrary, it now fastens on those places where it formerly did not. If these negative images have become fixed, it is difficult to get them off the plate; even fluids and sharp powders do not always effect it; and it often happens, that after other processes, as for instance exposure to vapours of chlorine, the traces of the old picture reappear in the plainest manner.

These peculiar phænomena, exhibited by the vapours of mercury, explain the difficulty with regard to the condensation of vapours, for they take place with all of them. Place, for instance, a screen with characters cut in it over a polished plate, and breathe over it; take the screen away and allow the breath to evaporate, and breathe on it again; those parts which before were affected by the breath will now appear black. The experiment may be repeated again and again, and always with the same result. Now breathe longer and more strongly; the characters will then generally appear lighter than the surrounding parts; and the image is usually destroyed at the same time, for only traces of it are seen afterwards.

That the vapours of iodine have an analogous effect might be supposed from what precedes, and it will be hereafter proved when I come to consider these vapours more in detail.

If this behaviour of the vapours by continued action should appear strange, I have to remark that it differs in no respect from that of light itself, and I therefore now turn to perhaps the most interesting proposition in this department,—that *the same modification is produced in plates when vapours are condensed* *as when light acts on them.* This identity between two causes apparently so different might have been supposed from the former experiments on breathing on surfaces. I will now adduce striking proofs of it.

It has often been stated that there is a natural relation between light and shade in Daguerre's pictures. Nothing can be more incorrect than this assumption, and that may be learnt without seeing many such images. The following is the true state of the case. If an iodized silver plate be allowed to remain too short a time in the camera obscura, it afterwards exhibits no image when exposed to the vapours of mercury; a light film of mercury is deposited over the whole plate, which is not only the case with this, but also with a plate of the pure metal and the blackened iodide of silver. If the plate remain a longer time in the camera obscura a picture certainly is produced, but in which only the brightest parts are depicted, and, which is here of importance, the light parts are of a white colour, *i. e.* they condense the most mercury. If the plate remain still longer in the camera obscura, a picture with all its details is formed, but the bright parts have lost a portion of their whiteness, and appear gray, *i. e.* they do not condense so much of the mercurial vapour. If it be left still longer, on taking it out no picture at all is to be seen; if now inserted into the mercurial vapours, a negative image is produced, or, in other words, these bright parts do not condense any mercury; consequently, as a general proposition, we cannot speak of a correct relation between light and shade.

If, therefore, light acts on iodide of silver, it imparts to it the power of condensing mercurial vapours in an increasing ratio; but if it acts beyond a certain time, it then diminishes this power, and at length takes it away altogether, and this happens before the yellow iodide has changed its colour. The vapours of mercury have been seen to do the same, and if in the last-described phænomenon they produced a negative image, it is only what light would have done if it had been allowed to act still longer. This identity between the action of light and mercurial vapours is so very remarkable that I will adduce other proofs of it.

An iodized plate of silver is allowed to remain the proper time in a camera obscura, and then exposed to the sun under a yellow glass. As I have already stated, a negative image is first produced, which vanishes after a time and is replaced by a positive one. If the plate be brought into the vapours of mercury at the moment when the negative image has disappeared, the positive one will be produced, as is the case when the light is allowed to continue its action. The images cannot be distinguished from each other, and therefore in this case light and mercurial vapours are identical in their effects.

The most beautiful proof of this proposition is the following:—As far as I am aware the blackened iodide of silver has never been prepared except by the influence of light. It cannot be obtained by the application of heat, for if an iodized silver plate be heated it assumes a milky white appearance on cooling, and becomes light-gray when exposed to light; but I will show that the *blackened iodide may be prepared by means of the vapours of mercury.*

It has been already mentioned that these vapours convert a correct positive image into a negative one by means of continued action, and the plate assumes a yellow appearance the moment before, like ordinary iodide of silver. The facts of the case are these:—the vapours are first deposited on the bright parts of the object (the sky or a landscape); on continued action they remove from these parts, and the yellow iodide again makes its appearance. If the vapours are allowed to act still longer the yellow iodide becomes black, although all other light has been excluded and the operation performed in the dark. The negative image has been produced.

I was long since made aware of this curious circumstance,—the blackening of iodide of silver by mercurial vapours, inasmuch as I found it extremely inconvenient in a series of experiments tending to a different object. It appeared to me possible to contrive a simple method of shortening the time necessary for the action of the camera obscura, by exposing the iodized plate for a moment to the sun, either before or after it has been introduced into that instrument; but as often as I tried this means, and however rapidly I executed it, a blackish colouring was always produced when the plate was brought into the mercurial vapours, although it had exhibited its usual yellow colour before being exposed to them. I must also mention, that the black iodide produced by mercury adheres to the plate just as little, and is not more easily soluble in hyposulphite of soda than that formed by light.

Although the identity of the action of light and mercurial vapours, and that of aqueous vapours, has been proved, still the general application of the principle to all substances and vapours is so important that some other instances should be given. There is no doubt that this proposition will be confirmed in all cases where the vapours produce a visible effect, and where they can be made to act so slowly that the proper moment may be easily discovered; for it is very evident that if the action is too rapid and violent, the peculiar states of the surface which cause the production of the images will not be sufficiently clearly developed; and for the same reason it is necessary to employ well-polished surfaces, a point which in my experiments considerably reduced the number of substances examined.

The phænomena described above as taking place with iodide of silver may be observed on pure silver, platinum, copper, steel and black glass, by means of mercurial vapours. A well-polished plate must be covered with a partially excised screen, and exposed to the vapours of mercury, which, in the cases of steel and black glass, may be heated to 90° R. and upwards. Mercury will afterwards be found on the plates, and of course on the parts where the screen was cut out. If the former experiments have been understood, it will be easily seen that the parts (*a*) of the plates which have condensed the mercury are in a similar state to those portions of an iodized silver plate which have been nearly blackened by light. Just as these latter have lost the power of condensing mercurial vapours, so have also the parts (*a*) of the plates now experimented on. If it be a platinum, steel or glass plate, it may be easily proved by removing the mercury, either by gentle and careful rubbing, or by a very slightly raised temperature. If the plate be now introduced into the vapours without a screen, the former figures will make their appearance, but this time darker than the rest of the plate, depending upon the fact that those parts which formerly condensed the mercury now cause the deposition of little or none. This is, properly speaking, the same phænomenon as we have previously observed in the case of aqueous vapours, and which consists in the fact, that if we breathe on a plate, we find, on repeating the operation, that the breath avoids those parts on which it had been deposited in the first instance; but it was necessary to mention the experiments in this manner, on account of further experiments, which cannot be so well made with vapours of water.

As the plate, when it has condensed the mercurial vapours, is, as has been shown, in the same state as iodide of silver that has been nearly blackened by light, it is evident that vapours of low tension must produce those states, which, in the case of iodide of silver, precede the blackening, and which cause a stronger condensation of the mercury; and not only vapours of low tension, but also those of a high degree, supposing they act only for a short time; for it is always the same with vapours as with light, the same effect is produced by high as by low intensities, provided the times are proportionate. I therefore repeated the former experiment, by exposing a well-polished plate with a prepared screen to the vapours of mercury, but only for a short time. Nothing was visible when the plate was taken out, but it was in the same state as an iodized silver plate that has remained in the camera obscura the proper time. After it had been again exposed to the mercurial vapours, but free and without the screen, an image of the excised figure was produced, and, as was to be expected, the previously uncovered parts were lighter, and condensed more mercury. The picture acquired strength by repeatedly warming the mercury, as is often seen in Daguerre's pictures, and in general in those images produced by aqueous vapour. Continued breathing on these latter generally causes them to appear clearer and better defined, because the vapours of water, like all others, continue the action on the surface which has been begun, and are capable, in the course of time, of inverting it.

In the preceding experiment the mercurial vapours were employed twice; the first time in so weak a state, or for so short a period, that they produced no perceptible precipitate. Other vapours, as for instance those of iodine, may be employed with equal success, instead of those of mercury, in the first instance. Let vapours of this kind act on particular parts of a plate of silver for so short a time that no colouring of the plate is produced, about the fourth of that which is necessary to produce the first yellow tinge. If it be now introduced into the mercurial vapours, the mercury is deposited on those parts which had been previously affected by the iodine.

It will be evident, from the above detailed experiments, that the vapours of iodine act on silver just like light; and it is in fact a matter of indifference whether light be first allowed to act on the plate of silver and then iodized, or whether it be iodized first and then exposed to the influence of light; but iodine acts just as light upon the already formed iodide of silver. If a plate of silver be exposed to iodine, it first becomes yellow, then red, rose-coloured, blue. There is no doubt that these colours may be derived from the varying thickness of the layer of iodide of silver; but light itself also produces a similar series of colours if it be employed properly, and not allowed to blacken the surface too rapidly. In this latter mode of experimenting the thickness of the layer of iodide of silver is not altered; and if we wished to explain the different tinges by this alone, we should meet with the difficulty which Draper has observed*, viz. that the later colours are less applicable to the production of images, inasmuch as they are less fitted for the condensation of the vapours of mercury. This last circumstance cannot depend upon the thickness of the iodide of silver, because there are still thicker layers, which, according to Draper, are well-suited for the production of images. On the contrary, this behaviour of the later colours towards mercurial vapours will be better understood from the fact of light being capable of producing the same effect, and by continued action rendering the iodide of silver incapable of precipitating those vapours, as has been already proved. Light and the vapours of mercury blacken iodide of silver; vapours of iodine also effect it; for Draper correctly states, that on continuing the action of iodine the plate of silver assumes a metallic tinge after the primary blue colours, which proceeds from a layer of black iodide, and which latter is no more adapted to the condensation of mercury and the production of images. I have convinced myself of the blackening of the iodide of silver by iodine, by means of the following experiment. After a plate had been exposed to these vapours a sufficiently long time, I introduced it into a hot solution of hyposulphite of soda; a blackish powder remained behind, which could easily be wiped off, and was the black iodide of silver.

After iodide of silver has been blackened by light it becomes greenish-yellow, and afterwards, by long-continued exposure, first red, and then blue, which was found by exposing it in the camera obscura for thirteen days, as mentioned above; consequently the action of light on blackened iodide of silver is to colour it yellow, red and blue...... If we overlook for the present the very small differences of colour, we find that the action of light is precisely similar to that of iodine; it is also

* London and Edinb. Phil. Mag. for September 1841.

similar in regard to the capability of the iodide of condensing mercury and of being dissolved by hyposulphite of soda.

With regard to the differences of colour, the causes may be, 1st, as may be seen *à priori*, that in iodizing the successive colours cannot always be the same, for the surface of the silver is not perfectly homogeneous, not equally iodized in all parts, not everywhere of the same temperature, and the vapours of iodine are not uniformly distributed. My experiments, which were more extensive than those of Draper, exhibited the second cycle of colours, nearly the same as the first, beginning with a pure yellow. The third series began with a greenish-yellow, which was formed in the middle of the plate, while the former colours were visible at the edges. This was evidently because the vapours of iodine had acted strongest towards the middle, and allowed the other colours to be seen side by side, while at the same time they became mixed and lost their purity. If such an unequally iodized plate be exposed to light, the earlier colours of each series are acted on sooner than the later ones, and the plate soon acquires a uniform coloured appearance. It cannot be doubted that this succession of sets of colours might be very frequently repeated in iodizing a plate of silver, and that it might be observed if it were not for the different colours being present on the plate at the same time, a difficulty which becomes greater as the experiment is continued. In the former part of this paper I have rendered it probable that there are similar alternations caused by light, and both kinds resemble each other in the fact that the later cycles or sets require a longer time for their production than the earlier ones.

If we reconsider the process of Daguerre, it is evident that the great degree of sensitiveness which it produces depends partly upon the iodine in the form of vapour acting on the silver. I think it doubtful whether iodide of silver would possess the same advantages if prepared by any other method. Probably the vapours of chloride and bromide of iodine assist in the same manner, and mercury may exert a similar influence. By being brought into contact with the plate in a vaporized state, they diminish the time it must remain in the camera obscura. In the case of chloride and bromide of iodine this is very evident, from the fact that they quickly colour a plate of silver yellow, red, rose . . . . although it scarcely acquired any colour in vapours of iodine, and that these plates may be again exposed to the vapours of iodine without diminishing their sensibility.

By these experiments I think I have proved that *contact, condensation of vapours, and light produce the same effect on all bodies.* The differences which appear may be referred to the varying intensity of the producing cause, and the greater or less depth to which the action extends. The black iodide of silver, for instance, is obtained with more difficulty than is generally believed with intense light, as that of the sun, and the different colours cannot be procured at all; for such an intense light rapidly changes the outermost layer into black iodide, and then reconverts it into the coloured variety; while the parts which are more deeply situated are subsequently blackened, and consequently very different states of metamorphosis may be present, even in the very thin layer which is affected by light. In the case of the vapours of mercury we may also see very different effects, according to their elasticity, either negative or positive images, pictures which remain fixed, or may be easily and completely wiped off. The most general axiom that I can propose with reference to the influence of the above-mentioned causes is, *that by their means the affinity of all bodies for vapours is modified,* so that they are precipitated and adhere to them in a greater or less degree. As has been sufficiently proved, the modification of the substances is double, and can produce either a stronger or a weaker condensation.

If I have extended the above axiom to all bodies, this generalization will not be considered inadmissible, for the foregoing experiments form a whole in such a manner that a conclusion from one may be applied to another. For instance, if *contact* cause that peculiar modification in fluid mercury, there is no doubt that aqueous vapours as well as *light* would be able to produce the same if the experiment were properly made. If we consider the subject from this point of view, we see that the experiments have been extended to sufficiently heterogeneous bodies, and there is no visible reason why the generalization should not be admitted. The necessity for the surfaces of all the bodies being polished is no objection, because this state must be produced in order to render the images more visible. Moreover, I have made experiments with several bodies that were not polished at all.

We cannot but suppose that the modifications of the bodies are accompanied by other physical and chemical changes beyond those already observed; hyposulphite of soda has already afforded an instance. It dissolves the variously modified iodide of silver with different degrees of ease, and the blackened iodide not at all; consequently the behaviour of some of these modifications is chemically different.

There are many other suppositions which occur to me; I will only mention one, viz. that it is possible the modifications stand in close connexion with the hitherto so mysterious phænomena of phosphorescence. A step towards its explanation has been made by the discovery of Riess*, that polished plates even of conducting substances, which were situated along the path of the electrical explosion, or upon which the electricity has passed in the form of brushes of light, afterwards exhibit spots which do not condense aqueous vapours, and which are evidently in that state which we have produced in such different methods. Moreover, it is known that when the electric spark passes over a suitable substance, it renders it phosphorescent, and that in this case coloured glasses have the same influence as in the so-called chemical actions of light. It even seems as if phosphorescence had been produced by contact alone, although the value of this agent has not been noticed†. But these points must be reserved for future experiments.

I now return to the original object of this memoir, viz. the Action of Light on the Retina. I am inclined to believe that the state of this postulate has considerably changed, and that it will no longer appear as a daring hypothesis. If light produce the same modification in all bodies, it will do so in the retina; nothing is more natural. The questions, how the retina alters its sensibility, and how it regains its normal state, have been answered approximately, although not definitely. The modifications produced by light are of themselves subject to different variations, and can be produced by the same intensity of light if different times be employed. The preceding experiments have shown that there are means of making an iodized plate more or less sensitive without the assistance of the vapours of chloride or bromide of iodine, for instance, by means of contact

* Poggendorff's *Annalen*, Bd. xliii. s. 85. *Repertorium der Physik*, Bd. vi. s. 180.

† Poggendorff's *Annalen*, Bd. xlvi. p. 613.

the modification is destroyed as easily as it is produced. Experiments have shown that it disappears of itself without the interference of external causes; Draper found that iodized plates which only required exposure to the vapours of mercury to produce the images, if allowed to remain untouched several days, did not afterwards exhibit a trace of the picture when treated with mercury, although the iodide had not changed, and was still perfectly sensitive with regard to light. We must not overlook the fact to which I have already directed attention, viz. that the modifications which we produce on the plates of silver by means of light in order subsequently to form a picture, are considerably less than those necessary for vision to be enabled to act. It is probable that a very short period of repose would be sufficient to destroy this small degree of modification. If, however, in the case of vision, we allow the action to proceed further, by regarding for a length of time a strongly illuminated object, then the retina naturally does not so soon regain its normal state, and even lasting ill consequences may ensue. The images discovered by Riess by means of electric action, and which were visible after a lapse of more than four years, were owing to the higher degrees of modification. Disregarding these, we find that the first stages are easily destroyed, by a slight friction, by the breath, &c. Very inconsiderable causes act on bodies, for I have found that if a polished plate of silver be allowed to remain untouched a few minutes, the process of iodizing requires a much longer time than if the plate had been immediately exposed to the vapours of iodine. This cannot be explained so easily; for light requires a much longer time to exert its influence on these plates than on others, although they are iodized just the same.

We see then by what insignificant causes the modified state of the surface is changed or destroyed, and we therefore understand why the system of after-images, although always present to the eye, is still not of any very great importance. The pressure of the outer muscles of the eye, whose action extends to the retina, is here of great advantage, as is also the continual motion of the eye itself, which produces successively the most varied states of the separate parts of the nervous membrane. Moreover, there is a point which must not be forgotten at the end of this treatise, but to which we have not as yet alluded, in order not to put a stop to all further research, viz. that the retina is an organic formation, and that the substance of the nerves can be so easily restored.

ADDENDUM*.

In my treatise on Vision, &c. I have shown that contact produces the same effect as light, and that in this manner we may obtain a very accurate image of the body in contact. The accuracy with which engraved figures, for instance, are depicted in successful experiments, rendered it very improbable that a perfect material contact is necessary, and I have since convinced myself that such contact did not really take place in most experiments. Neither the bodies themselves, nor the silver plates on which they were depicted, were so even as to allow of the possibility of complete contact; consequently an action seemed here to have been exerted at a distance. A plate of agate with several engraved figures was covered with thin strips of mica, and upon these the silver plate was laid, so that the space between the two surfaces amounted to one-fifth of a line, and admitted of seeing through; when, after the lapse of several hours, the plate was introduced into the mercurial vapours, a perfect image of the engraved figures was produced. I have examined other bodies placed at a greater but not measured distance, and always found them depicted, and have thereby discovered the curious fact, that *when two bodies are sufficiently approximated they reciprocally depict each other*. It is necessary to remark, that all extraneous light was carefully excluded in these experiments; that I made these experiments during the night, and without candlelight; that the bodies were placed in a dark box in a dark chamber; and, which is the principal point, that all other light falling upon these approximated bodies would render the formation of images quite inexplicable. One kind of experiment is to keep the bodies apart from each other, the other is to bring them into close contact, as for instance a plate of silver lying upon one of agate, or anything else; how can any extraneous light act in this case? Notwithstanding, I do not believe that the existence of a new *power* belonging to bodies has been hereby proved; I am much rather of opinion that these experiments lead us to another result, viz. *that every body must be considered as self-luminous*. This property, as far as my experiments go, is not first produced by extraneous light, as is the case with phosphorescence; it makes no difference whether the bodies have been kept in the dark for a long time or exposed to the sun be-

* From a letter of the Author, dated 2nd of June.

fore the experiments are made. After the plate of agate had been many days removed from light, I placed it half covered in the sun's rays. On being afterwards depicted on a plate of silver, no perceptible difference could be observed between the covered and the uncovered half.

If we admit this self-luminosity of bodies, and assume, moreover, that which I think I have been able to prove elsewhere, viz. that those rays which proceed from the surface in a slanting direction exert a much smaller degree of influence than those which take a perpendicular course, then there is no difficulty in explaining the clearness of the images even when formed at a considerable distance. We also easily perceive why heating the depicting body may sometimes be advantageous; because, as heat employed in a proper degree produces incandescence, so it is natural to infer that the inferior degree will produce a self-luminosity, and therefore increase the influence in question. The power of self-luminosity seems to be present in bodies in very different degrees, and the polish seems to exert a perceptible influence on it, although comparative experiments in this respect will be very difficult.

As yet I have depicted the following substances on pure silver: Pure silver, iodized silver, brass, iron, steel (particularly the stamp of a coin), violet and red glass, black polished horn, white paper with characters inscribed, gypsum, mica, agate and cork, and as yet I have not met with any body with which the experiment did not succeed. As the list of bodies is varied enough, we may assume that the property of self-luminosity is inherent in all matter. Under certain circumstances the force must be regarded as very considerable, for I obtained the image of several bodies on a plate of silver in the course of ten minutes, which proves the existence of a very powerful action. As the bodies depicted were partly dark ones, we arrive at the conclusion, that, even where for the retina only darkness is present, there is still a considerable evolution of light, which proves its presence by acting on suitable substances. It is true that in so short a time as that above named images can only be obtained by the continuous action of mercurial vapours of a low tension; because, as I have shown, these vapours, as indeed all, are capable of continuing the action commenced by light. In the preceding experiments, however, no vapours are necessary, and if an iodized plate be brought near any body in the dark, and sufficient time allowed for the action to take place, then the plate exhibits the picture, for on those parts most exposed to the influence of the body the iodide of silver is *blackened*, although all that could possibly be called light by the retina has been carefully excluded. I have often made this experiment myself.

We must not, in the present state of the subject, examine the influence of pressure or contact on the surfaces of bodies, because the effects which are always obtained must be referred to the light evolved from all bodies. However, I do not entertain any doubt as to the influence exerted by pressure, for I think I have sufficiently proved that nothing more takes place on the retina during vision than is the case with all other bodies when these surfaces are affected by rays of light, and then, *vice versâ*, it is allowable to draw conclusions concerning the phænomena observed on other bodies from those seen on the retina. As in the case of this latter body pressure so decidedly produces the same effects as light, it is probable that the same takes place with other bodies, although the real proof of it has not yet been given. But it is just the same with electricity; that power produces in the eye effects similar to those of light, and one might from this conclude that the same would hold good for other substances, if the fact had not already been directly proved by Riess's beautiful experiments.

Article XVII.

Some Remarks on Invisible Light. By Professor Ludwig Moser *of Königsberg.*

[From Poggendorff's *Annalen*, vol. lvi. p. 569, No. 8*.]

As the subject of invisible light seems to have excited considerable interest, I will communicate the following experiments which I have made upon it:—

1. Besides those bodies mentioned in my paper on Vision, I have also obtained pictures from gold, copper, German silver, zinc, white transparent glass, bismuth, antimony, tin, lead, metal for mirrors, wood, mother-of-pearl, black pasteboard, black leather, black velvet, and lamp-black. I covered an iron body of convenient form with a thick layer of this last body, and placed it at some distance from a plate of silver; on the plate being

[* This paper is dated Königsberg, July 1842.—Ed.]

exposed to the vapours of iodine, a very distinct image was formed; it would consequently be a discovery if we could find any body which does not possess self-luminosity, or in which it is present in so small a degree as to escape our observation.

I have made all the experiments on the self-luminosity of bodies in so-called darkness, in order to remove the objection that any influence might have been exerted by extraneous light. But this is not necessary, because we have seen that there are characteristic tests for invisible light (*vide* the treatise on Latent Light which follows), and by means of which it becomes impossible to confound it with visible light. The two kinds differ, however, from each other, only as violet does from red, in a physical sense, and the method which teaches us the difference between these two last-mentioned colours extends also to the visible and invisible rays. With this exception, there is *no effect that cannot be produced by one kind of light just as well as by the other*. These characteristic tests are however not necessary, and we need not be so careful in excluding daylight in our experiments with the peculiar rays of light, inasmuch as it cannot produce any additional effect if they be properly conducted. I have already shown that if we form images by means of common light (including the dark rays of Ritter) on clean surfaces of silver, copper, glass, &c., the sun requires to act for one or two hours; and I can add, that if we allow the picture of a camera obscura, directed towards houses illumined by the sun on a favourable day, to act for twelve hours on any of the above-named plates, and afterwards expose it to the vapours, scarcely a trace of an image is produced; consequently, if a body can depict itself upon metals by means of its peculiar light in the course of ten or even two minutes, it is evident that common light, even of great intensity, cannot increase the effect.

2. As yet I have caused the invisible rays to act on gold, silver, German silver, copper, brass, iron, steel, zinc; moreover, on yellow and on blackened iodide of silver, on copper which has become tarnished of a purple colour, on glass, porcelain, mica, ordinary japanned plate, and even on mercury. In order to employ the latter I covered a pure plated sheet of copper with a thick layer of it, so that the surface consisted of fluid mercury. The bodies which I caused to act on it were of iron and horn, and also the steel stamp of a medal. The mercury did not exhibit a trace of an image after this treatment, but it was produced so distinctly on exposing it to the vapours of iodine, that

the inscription of the stamp could be read with the greatest ease. The above-mentioned substances differ only, as regards the experiment, in the degree of polish which can be given to them, or which they naturally possess. Mica produces the images with great exactness, as does also well-polished copper. I have in my possession a plate, the surface of which consists of brass, iron, copper and zinc, and upon which I allowed an engraved plate of agate to act. When exposed to the vapours, all the metals exhibited their respective parts of the image; but it was less distinct upon the iron, which was not well polished. A good plate of steel gave excellent images. In all cases where it was possible I first used the vapours of mercury of the usual tension, and then those of iodine, a process which has proved to be advantageous even with japanned plate. [*Vide* the following treatise.] We cannot therefore doubt that light acts uniformly on all bodies, and that, moreover, all bodies will depict themselves on others, and it only depends upon extraneous circumstances whether or not the images become visible.

3. Among these circumstances may be reckoned the divergence of rays, which takes place with invisible as well as with visible light. It prevents us from placing the bodies at too great a distance from each other, if we wish to obtain distinct images. At first I removed the silver plate only one-fifth of a line from the body which was to be depicted, afterwards I increased the distance to a whole line. Even when this was the case, by the use of suitable objects I obtained distinct images, parts of which were however confused, as might be expected.

If we consider the circumstances under which the pictures of the substances are produced, we see that engraved plates are much more fitted for the purpose than raised figures, which cannot be advantageously employed. I succeeded very well with engraved plates of agate or wood, brass or iron stamps, and printers' types. In using the latter a suitable distance is found to be advantageous, for it changes the sharp lines of the object into a more uniform shadow on the image. Black writing on white paper depicts itself so as to be visible, but I have never seen it very well executed. The same is the case with mosaic work, the single divisions of which are represented with surprising accuracy, while, on the other hand, I never could render the figures, which differ only in their colour, very visible to the eye. I will remark, as a general rule, that it does not necessarily follow that those parts of a body which to our eye appear strongly marked in daylight should be very clearly visible in a picture produced by its own peculiar rays.

4. Finally, I will mention the method by which we may convince ourselves of the action of visible light on many bodies, a method which we should be able to employ in all cases, were it not that we are compelled to use light of great intensity, whereby heat often exerts a disturbing influence. I direct a small camera obscura with a lens of 15^{mm} aperture towards the sun, and insert into it a plate of mirror-glass to receive the image. After the sun has passed through, I remove the plate and breathe on it; a distinct and accurate picture of the sun's path is produced. I have made the same experiment with plates of silver, gold, copper, German silver, iron, steel, brass, zinc, and also with the compound plate mentioned in section 2, and always with the same success. Vapours of mercury, iodine, &c. may be employed instead of those of water.

In order to meet the objection which might be raised, viz. that heat contributed to the success of these experiments, I allowed the image of the sun to pass through a yellow, bright red, and a tolerably bright violet glass, and then to fall on a plate of silver. When it was introduced into the vapours the path of the sun was visible, though not very perfectly, on the spot where the yellow image had acted. The violet image had produced a very visible effect, and the red none at all. As I had expected this, I had placed the red glass in the most favourable position, viz. in the axis of the lens. Another time I allowed the sun's image to pass through a red and a blue glass, each of which occupied half of the field of view. The image formed under the blue glass was very evident, but could not be traced under the red one. It will be seen that these results agree completely with what has previously been said respecting the action of the differently coloured rays; but they do not agree with the transmission of heat. In order to determine this approximately I constructed a small battery of German silver and iron, as proposed by Poggendorff, which was very sensitive, although it required some time for the needle to adjust itself, a circumstance which induced me to regard merely the direction of the deviation. When the operation was conducted in the sun it was found that the red glass transmitted most heat, and yet it had not rendered the image of the sun visible.

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ARTICLE XVIII.

On the Power which Light possesses of becoming Latent. By Professor LUDWIG MOSER *of Königsberg.*

[From Poggendorff's *Annalen*, Band lvii. 1842. No. 9. p. 1.]

IN the following I intend to prove that there is a Latent Light as well as a Latent Heat; that they both are produced under similar circumstances, and when under the same influences pass into a free state; and that when a body alters its state of aggregation we cannot any longer regard this as produced by heat alone; light also takes an active part in the process. I certainly must require the same favour to be shown to latent light as has long since been granted to latent heat, viz. to allow the question, as to what idea we can have of such a state, to remain unanswered. Since Deluc and Black discovered latent heat in the middle of the last century, nothing has been done to render this extraordinary power comprehensible, and yet the fact has not been denied on this account. I hope, therefore, that the incomprehensible nature of a combined state of light will not be adduced as an argument for its non-existence; the proofs in both cases are equally convincing. The peculiar nature of light, which does not allow of a determination of its intensity, also prevents a quantitative measurement in the case of light which has become latent. On the other hand, however, I believe I shall be able to extend the knowledge of this latter to such a degree as has not yet been done with latent heat, and which, even since the great discoveries of Melloni, will not very speedily be achieved.

In my treatise on Vision, &c., I have shown that the precipitation of vapours exerts an influence on the condensing bodies similar to that of light; but at present a new definition of this agent is necessary, for, since I have proved the existence of invisible rays of light, the retina has ceased to furnish us with the most striking proofs. The most general definition which I should be inclined to propose for the present is, *that by light the surfaces of all bodies are modified, so that they condense vapours otherwise than usual.* A plate of iodized silver, which has undergone the action of light in a camera obscura, and has afterwards been exposed to mercurial vapours, furnishes a very simple example of this definition.

As I am here obliged to give a determinate definition of light, I may perhaps be allowed to cast a comparative glance at the nature of heat, by which means its value may be better understood. The human frame is affected by both forces, by light as well as by heat, and in general detects their presence. But precisely in the same manner that it was found necessary to abstract from the impression of heat on the senses, so it is now in regard to light. Although the eye is much more sensitive to the latter power than the general feelings are to heat, although it is even capable of distinguishing the different degrees of refrangibility, still there is a class of rays of light which entirely escapes its attention, although these rays possess a greater index of refraction than all the others. The human eye, therefore, is not sufficient for proving the presence of rays of light in a particular case; and even among the group of visible rays it points out certain relations of intensity which are not confirmed by the majority of other bodies, and which depend either on the construction of the retina or upon the nervous membrane not being directly exposed to the rays of light, but placed behind some refracting bodies, which may easily cause an unequal absorption of rays possessing various periods of oscillation.

When we abstracted from the subjective impressions of heat, we were at least fortunate enough to find an objective action of this power, which it exerts equally on all bodies; the measurement of its intensity has been the direct result. We are not yet so far advanced with regard to light, and there is as yet no possibility of measuring the intensity of rays of different refrangibilities.

Before the expanding power of heat was known, the following definition of the force might have been allowed, although, practically, it would not have been very useful. Heat is that which when acting on particular parts of a surface modifies them so that they condense vapours of various kinds otherwise than usual. This definition would have been correct, but not very applicable, on account of its belonging to the nature of heat to expand itself on all sides, both without and within the same substance. If light, on the contrary, affects portions of any surface, nothing of this extension is visible, a proof of which we often observe in the extraordinary accuracy of a Daguerre's picture; consequently, in the case of light, a similar definition is of more practical use, and the condensation of vapours is a good means of proving the existence of some action of light, even although it cannot be employed in its measurement. If, then, this test for light, as well as another hereafter to be mentioned, is considerably inferior to that for heat (the thermometer), still the former offers, as we shall see, advantages of a peculiar kind, which are quite wanting in the instrument for the measurement of caloric.

In now turning to the real subject of the treatise, I will refer to my previous experiments, which have in so many ways established the fact, that the condensation of vapours produces the same effect as the direct action of light. Even some peculiar effects of light, as for instance the blackening of the iodide of silver, may be produced by the condensation of mercurial vapours, of which I have mentioned several examples. I only refer to this phænomenon in order to add that I have obtained the same effect with the same vapours with nitrate of silver, by means of papers brushed over with a solution of this salt; and, moreover, to add, that this very striking observation is by no means new, but was made several years since, although no importance was ever attached to it, nor could well be, inasmuch as the phænomenon acquires its true interest only when connected with other facts. Bayard observed that when papers covered with bromide or chloride of silver are taken out of the camera obscura before a trace of a picture is visible, and are then treated with vapours of mercury, those parts which were exposed to the light become black.

If the precipitation of vapours produces effects similar to those of light, we are justified in assuming a latent light belonging to the vaporized state, just as from an evolution of heat under similar circumstances we adopt a latent state of that power. For the present all that remains to be shown is, how the reversed process, evaporation, produces effects of light; and this I have observed on evaporating water, alcohol and æther. I employed plates of silver, gold, copper, German silver, and glass; we may, however, evaporate fluids of any superficial extent, if they only produce small permanent changes, and we are able to write down as it were the effects which have taken place. Experiments of this kind, to succeed well, are by no means easy of execution; even distilled water readily leaves traces on well-polished bodies, particularly when employed in large quantities; but I succeeded in obtaining incontrovertible results in the following manner:—I dipped a small thermometer-bulb into distilled water and wrote upon a plate with the adhering drop, but so that the bulb itself did not touch. It is possible to perform the experiment so that the water is deposited on the plate in a concrete form only in some places, while it has passed over the greater part merely as a breath of vapour, which, however, is quite sufficient for these experiments. Or the trial may be made thus:—Strips of bibulous paper are fixed on to any convenient body and moistened with a very little distilled water; the polished surface is then touched for a moment with these strips, which is also quite sufficient. If, however, the evaporating liquid should leave some traces behind, as is generally the case with alcohol and æther, but in the case of water arises chiefly from the pulverulent matters employed in the polishing, then these spots may be carefully rubbed off. It is also as well to perform the operation under a damp cloth. After a silver plate has been exposed to the action of the evaporating water, alcohol, or æther, it is to be exposed to the vapours of water, mercury, iodine, or to those of hydrochloric acid, chloride, or bromide of iodine, &c.; in all cases a perfectly distinct picture is produced: in the aqueous vapours it is generally the strongest, in mercury it appears only after a long action, and in vapours of iodine it is often very faint. But even in this case it is only necessary to expose the plate to daylight in order to obtain a strongly marked image. If any other metals besides silver have been employed for evaporating the fluids, it is only necessary to expose them to any of the above-mentioned vapours in order to obtain good pictures. I will moreover mention, that if we allow hot water to evaporate from a Daguerre's picture as it is taken out of the camera obscura, the image is completely destroyed. This is caused by a process of light, as will be seen from the following paragraphs.

It is therefore evident that evaporation produces phænomena similar to those of light, just as well as the condensation of vapours; and I leave it for experimental philosophers to extend the result to the passage of a solid state of aggregation into a fluid one, and *vice versâ*, and to take into consideration these transitions, both as refers to the processes of light and those of heat. As the matter is so new and of so much importance, I may be permitted to draw attention to the fact, that (remaining within the limits of what has been empirically proved) if we have a vapour of a certain elasticity and temperature, and any plate also of a determinate temperature, the condensation of the vapour is not found at once, but it depends upon those actions of light to which the plate has been exposed. This state of the matter is not sufficiently advanced to prove what influence light has in the change of the state of aggregation. Although a silver plate, *e.g.* has the same temperature in every part, still the aqueous vapours will be condensed, either in a greater or less degree, by certain portions of it, according to the action of light which these parts have undergone*.

The same holds good with regard to the condensation of the vapours of mercury, iodine, chlorine, &c.; it depends as much upon light as upon heat, and it is evident, from the difference of those vapours which I have examined, that such a result is of general application. For instance, although aqueous vapours are precipitated very readily on plates of silver, they evaporate again just as quickly, and exhibit very small or no adhesion. The vapours of mercury adhere permanently, for the images of Daguerre do not exhibit any perceptible change even after a length of time. But what is very peculiar in this respect is, that they do not seem to combine with the mass of the silver, or with that of gold, or even zinc, while mercury in a fluid form exhibits so great an affinity for these substances. Vapours of iodine combine chemically with the silver. Notwithstanding, then, that these vapours behave so differently in certain respects, they still agree together in one point, viz. that their condensation is influenced by light just as much as by heat. Indeed we should have been able to draw this conclusion from the Daguerre's pictures alone, if we had had a correct insight into the process which produces them.

I will now proceed to the interesting question concerning the *colour* of latent light. The determination of this point is as important as it is difficult, and I have only succeeded after many attempts with some kinds of vapours, but in these cases in a manner that promises sufficient security. This examination is important, inasmuch as one might easily be induced to

* With regard to the greater or less degree of condensation, I refer to my first treatise, in which I have examined the vapours of mercury in this respect with more minuteness. The vapours of water do not differ in this from those of mercury, and when of sufficiently high tension invert the images just as well as the others.

advance facts as objections to that correct statement, that the condensation of vapours has an action similar to that of light; while, if we consider them from the proper point of view, viz. that the various kinds of vapour contain different coloured latent light, we find in them the strongest confirmations of the proposition. When I first discovered the effect of light in the precipitation of vapours, I commenced a series of experiments to find whether anything analogous to colour occurred in these experiments. I then believed that the different degree of elasticity might perhaps be compared to the different colours of the spectrum; but this opinion was overthrown, when it was found that there was no effect produced by mercurial vapours of high tension which could not be brought about by the long-continued action of vapours of a lower degree of tension; consequently in the case of the vapours of mercury there was nothing in their degree of elasticity that could be compared to the colours of the spectrum. There would have been no reason for ascribing to this species of vapour a peculiar action different from others; but it was more natural to suppose that in all cases a continued action would have the same effect as a high degree of tension. Only those vapours like those of water could make an apparent exception, inasmuch as they do not remain on the plates, but evaporate again, and thereby give occasion to the production of a double process. The question remained, therefore, unanswered, until I obtained an insight into the action of the condensation of vapours, as produced by the light which was in a combined state.

It was now not to be doubted that each species of vapour retained a peculiar kind of light in combination, either rays of a simple colour, or of several, but combined in a definite proportion. After this point had been settled, I soon became convinced that what we as yet know of the action of the different colours of the spectrum is quite insufficient for determining with safety the latent colour of even one vapour. Our knowledge is not insufficient because too few substances have been examined with regard to their behaviour towards light; on the contrary, I believe that there is still sufficient to be studied in one substance, viz. iodide of silver; and I know only one question, to be hereafter mentioned, which I do not think can possibly be answered by iodide of silver alone, but requires the test of other kinds of matter. I shall therefore principally make use of this substance, and I consider the solution of the following problem as the groundwork for every further step in this new sphere, viz. as far as regards the determination of latent colour. This problem is—

"*Light has exerted its influence upon iodide of silver: whether it be visible outwardly or not, it is required to find by which colour this action was produced.*"

I have not succeeded in exhausting this proposition; but I think that I have advanced some steps towards its solution, which already lead to remarkable results.

According to the definition given in my treatise on Vision, the influence of the differently refrangible rays on iodide of silver was, that the blue and violet rays commenced and continued the action until the iodide was blackened. This is a fact which, in the case of chloride of silver, has been known since the time of Scheele. E. Becquerel made the additional discovery that the red rays cannot indeed commence the action, but are capable of continuing it when once begun, and can carry it on even until blackening ensues. Finally, I had stated that the yellow and green rays restore the blackened iodide of silver to its primitive coloured state; consequently, the groups of colours, which for the sake of brevity may be denominated blue, red and yellow, exhibit qualitative differences in their action upon iodide of silver, and this action cannot be brought into connexion with the refrangibility (or more correctly the length of oscillation), which is a very remarkable circumstance; for the action is begun by the most, continued by the least refrangible rays, and carried on still further by those possessed of a medium index of refraction. On further consideration, this appeared to be very incredible, and I shall now show that that idea was quite erroneous.

The reality is, *that the rays of every degree of refrangibility all act similarly on the iodide of silver*, and there is no positive result which cannot be obtained by rays of all kinds. *The only true difference is, that in order that the same action should be produced by the various rays, it is necessary to employ different times*, and in such a manner, that if the action, measured in any manner, be represented as a function of the time, this function will have different forms for the different colours.

As a proof of this proposition, I will first mention, that the iodide of silver may be blackened by coloured glasses of all kinds. In this respect there can be no doubt with regard to the violet, blue, green, yellow and orange glasses. I have had opportunities of examining a great number of them, and when the operation was performed in the sun the action was never long in making its appearance. The only uncertainty might be with regard to the red glasses. I have some of these, in experimenting with which I thought I observed that they retained the iodide of silver in its original colouring, and were not capable of commencing the action. On further examination, however, I found that this depended on the manner in which I had previously made the experiments, inasmuch as I had laid a uniformly red coloured glass on the iodized silver plate. In such circumstances it is difficult to observe the slow action of these glasses. But if any drawing be made on these glasses, by rendering certain parts non-transparent, it will then be seen that they are capable of commencing the action just as much as the others, although indeed not so rapidly. My friend M. Dulk succeeded just as ill in some experiments that he made. He believed that chloride of silver does not undergo any blackening under a dark red bell jar, even after the lapse of several days; but when I introduced into this jar an iodized plate, situated behind a screen, a perfect picture of the excised part was produced in a few hours.

All colours then agree with the blue in having the power of commencing the action, and of continuing it until blackening ensues, although the periods required are very different. Here I may be permitted to make a short digression, to obviate a deception which might easily mislead. We are accustomed to say that blue and violet glasses, which allow but few rays of light to pass through them, act powerfully on iodide of silver, while yellow and red glasses, which allow the transition of a large quantity of light, act only feebly; but I do not think that in the present state of the science we may be allowed such an expression; by using it we should be placing more reliance on the judgement of the eye than it in reality deserves. We are well aware that our retina is affected most strongly by yellow and orange; so that if by its means we seek to judge of the prismatic spectrum, these colours are always considered as the brightest. If the retina were a substance like iodide of silver, or if it were freely exposed to the rays of light, the decision would certainly be different; under the same circumstances, the violet, or Ritter's dark rays, would appear to it the brightest, *i.e.* those rays which in its actual state produce little or no effect on it, would then exert the strongest action. It is just the same with the retina and the different compounds of silver as with the other bodies which have as yet been exposed to the action of light; it has always been found that the rays of a particular period of oscillation are the most energetic in their action. This appears to me to prove both that these bodies and the retina are by no means adapted to the determination of the relative intensities of the different coloured lights, and that therefore no very great degree of faith can be reposed upon the decision of the latter, viz. that a violet glass transmits few, and a yellow many rays. Apart from this, we find that rays of different degrees of refrangibility do not agree in regard to the blackening of the iodide of silver; certainly in respect to the time necessary for the operation, but in nothing else. And now we see that this effect is connected with the duration of oscillation; so that we may say, the greater the duration for one group of rays the longer space of time will they need to commence action on the iodide of silver, and to continue it until blackening ensues.

I now return to the peculiar power which red rays are said to have of continuing an action which has been already begun. In my treatise on Vision I have raised some doubts with regard to the peculiar action of this class of rays, and I have mentioned the principal argument by which such an idea would be subverted; but among so many others I did not direct more particular attention to the importance of that fact. It is true, that if the operation, as for instance the production of a Daguerre's picture, be performed in the common rays of sun- or daylight, then the red rays have a powerful continuing action, for it was in this case commenced by the blue, violet, and Ritter's dark rays. But now I have discovered and described another class of rays of light, those emitted by every body without exception, because it is self-luminous,—rays whose presence is evidenced by the fact of two sufficiently approximated bodies impressing their images on each other, although everything that the retina could denominate light has been excluded. I call them the invisible rays of light, to distinguish them from Ritter's dark rays at the violet end of the spectrum; I might also call them the most refrangible rays, for it appears that their refrangibility is greater than that of the other rays of the spectrum. As I shall show, they are not present in day- and sunlight, and must not therefore be confounded with the above-mentioned dark rays. When the invisible light has commenced its action upon iodide of silver, the blue or violet rays possess the same continuing power as the red rays when the primary effect has been produced by visible light. Place any body on an iodized silver plate, either in absolute contact, or else at a very small distance, so that an image may be produced by the emitted rays. This body must be retained in its position so short a time that there is no outwardly visible image produced, and no visible alteration of the iodide of silver. If the plate be now placed in sun- or daylight under a blue or even a violet glass, the picture is rapidly formed with all its details, while the most continuous action of the red and yellow rays does not produce in this case any more than a very slight trace of the image. This I have stated in my former paper, and it shows that the blue rays stand in the same relation to invisible light that the red do with regard to visible light. I will only mention one of the numerous experiments which I made on this point, and which all gave the same result, because I must return to the subject when speaking of the latent colour of vapours. I iodized a plate of silver, and laid upon it an engraved brass plate, a similar one of silver, and an engraved stone, for the short space of two minutes, and in the dark. Of course there was nothing at all to be seen on the plate; but when exposed to the sun under blue glasses, the images appeared in a few minutes, and those of the metallic bodies were very perfect; consequently the invisible rays of light had produced perfect pictures in the course of two minutes. If red or yellow glasses had been used, probably no image at all would have been produced by so short an action; for rays of these colours render images of the invisible rays apparent only after a long interval, and then very imperfectly.

Using more general expressions, we should say, if rays of a refrangibility N have commenced an action, then rays of a refrangibility $N - n$ are capable of continuing it where n has a determinate value. If it is chosen very small, that is, if the rays which are to continue the action lie too near those which begin it, then no image will be produced. If, on the contrary, n is too large, then the continuation of the action will become small, and almost nothing as far as regards the experiment; consequently that which has been, not very suitably, named "continuing force" becomes identical with the index of refraction. I here repeat, that when I speak of refraction, index of refraction, &c.,

I only wish to accommodate myself to the general manner of speech; properly I ought to substitute duration of oscillation, which, however, would be of but little use, as we do not know the function of that duration on which the action of light on bodies depends.

If we also add the proposition, that the length of action of a particular class of rays is an equivalent for their inferior refrangibility, so that, for instance, a more continued action of yellow light produces the same as the shorter action of the violet rays, then we shall easily understand all the phænomena of the so-called "continuation" (Fortführung). If an iodized silver plate be exposed for a short time to the ordinary rays in a camera obscura, then the red rays alone are capable of producing a visible image; the green and yellow rays certainly will not cause its appearance. If the visible rays had acted longer, then we might suppose that, if rays of a greater refrangibility than the violet or the dark rays lying at the end of the spectrum had commenced the action, the green and yellow would continue the formation of the image which was begun, and would render it visible. I may pass over practical proofs of this, as they are contained in my former paper. If, moreover, the invisible rays have begun an image, and have only acted a short time, then the blue, and even the green rays will possess a continuing power, but the violet ones will not. If they have been acting for a long time, then the violet rays will have the same effect, &c. &c.

In order to render the preceding, which is practically so important, more visible at a glance, I will adjoin the determined proportions of refraction to the several rays, viz. to the

Invisible Rays.	Blue Rays.	Yellow Rays.	Red Rays.
$N\,N(1-n)$.	$N(1-2n)$.	$N(1-3n)$.	$N(1-4n)$.

If the blue rays acted for a *short time*, then the action is carried on by the red ones, *i. e.* rays which differ in the refracting proportion $2nN$. If the invisible rays acted for a short time, then the blue ones continue the action, the difference being $2nN$ as before. If the blue rays have acted for a *long time*, we may suppose it as if rays of a still greater retrangibility had acted, for instance, the invisible rays with an index of refraction $N(1-n)$, and in that case the yellow rays can continue the action. No more value must be attached to this exemplification than it was meant to possess, namely as a means of showing the connexion of the duration of oscillation with the continuation, and the similarity in the action of all rays in this respect.

I will now turn to the peculiar power which the yellow and green rays are said to possess, of converting the blackened iodide of silver into its primitive coloured variety*, of which many examples were offered in my first treatise. The most convenient way of making these experiments is to allow a negative picture to be produced in the camera obscura, *i. e.* one in which the bright parts of the object are dark, and then to expose it to the light of day or the sun, covered by a green or yellow glass. A positive image is produced, for the black iodide is decolorized, and the unaffected parts are turned black. Gaudin made his experiments in this manner†.

I will now show that this singular property does not appertain to the yellow or green rays alone, but just as well to all the others, and that in this respect there is no difference between the colours except in point of time. A plate, in that state in which it is generally exposed to the mercurial vapours, is exposed to day- or sun-light under a red glass; a negative picture is produced. If in its first stages it be placed under a yellow glass, it becomes positive; if under green or blue, it becomes not positive. If, however, the negative image is further advanced, *i. e.* if it has remained longer under the red glass, then the green rays will render it positive, and finally also the blue and violet. This result is so interesting, that I will mention one or two of the experiments performed. An iodized silver plate was allowed to remain in the camera obscura for three minutes, and then placed under a reddish-yellow glass. First of all a negative image was produced, which was afterwards transformed into a positive one. Just as the latter was about forming, the plate was put under a violet glass, and it was then very quickly developed. Moreover, on a tolerably clear day, an iodized silver plate was left in the camera obscura eleven hours; a beautiful negative image was visible, the light parts were greenish, and

* It is necessary to remark, that by the expression "coloured iodide," I only mean to represent the phænomenon, but by no means to express any opinion as to the chemical nature of the body so produced. It may be proved that this state of the iodide of silver at this particular period, if considered physically, may also be attained by simple bodies, as silver, iron, &c., by means of continued exposure to the action of light; only this subject requires a more extended examination.

† *Comptes Rendus*, Juin 1841.

the dark of a reddish colour. On placing the glass under a dark blue glass, a strong positive image was formed in the course of half an hour, and this was not changed by exposure for several days.

We now understand the manner in which the various rays act upon iodide of silver; they all blacken it and then re-convert it into the coloured variety. These processes require very different spaces of time, according to the refrangibility of the rays. If we consider the degree of colouring of the iodide of silver, as expressed by the ordinates of a curve, and the times by the abscissæ, then, in the case of the blue, violet, and Ritter's rays, the curve will rapidly ascend and attain its maximum, which is what we call blackening. From this point the curve continues to approach the axis of the abscissæ. In the case of the yellow and red rays the curve takes an opposite course; at first slowly and gradually approaching the maximum, and afterwards descending rapidly towards the axis. With the invisible rays we find by experiment that the curve which represents their action rises at first very rapidly, but does not so soon reach its maximum as in the case of the violet rays; for although, as I have already shown, they produce a blackening of the iodide of silver, it is only caused after a length of time. It is true there is no means of measuring the intensity of the invisible rays and of comparing them with violet ones; my experiments, however, have shown that the former are capable of commencing their action on iodide of silver very quickly, for instance, in two minutes. If the blue or violet rays had commenced the action so rapidly, they would sooner have produced the blackening. I do not possess experiments to determine the course of the curve of the invisible rays after it has reached its maximum.

I must here correct an error to which my former treatise might lead. In describing the action of the yellow and green rays, I mention that the same phænomena could be obtained by means of daylight and the sun. This is correct; but considering the theory then established concerning the influence of the three groups of colours, I was obliged to assume that this action depended upon the portion of green and yellow rays contained in ordinary light, which is not correct. It has just been shown that the blue and violet rays are capable of decolorizing the blackened iodide of silver as well as the green and yellow; and I can now add, that if undecomposed light exerts this action it is owing principally to the portion of blue and violet rays contained in it. In those cases where the blue rays do not invert the negative image because it is in a too early stage of development, the light of the sun or day is also not capable of effecting it; consequently in this case the undecomposed light acts like the blue and violet rays, and unfortunately this fact is general. After manifold experiments I have not been able to discover anything in which the action of undecomposed light on iodide of silver differs from that of the blue and violet, or even of the dark rays of Ritter. This is the circumstance which I mentioned above, and which will probably render it necessary to examine other substances besides iodide of silver. How extremely disagreeable this circumstance is, will be seen hereafter in the determination of the latent colour of vapours of water.

If we assume the proposition concerning the identity of the action of all colours on iodide of silver as proved, it does not appear to advance, but rather to hinder the solution of the principal question, viz. to determine the refrangibility of light from the action it has produced; for if the condensation of mercurial vapours produces an action similar to that of light, it would not be possible to determine the latent colour of these vapours, because all colours produce the same effect, and only differ in their degrees of intensity, for which however we possess no means of measurement. Such is then the case, and we should thereby be compelled to forgo the most important part of the examination of latent light, if I had not succeeded in finding a peculiarity in the consecutive action of *two* kinds of differently refrangible rays, by which the determination of the latent colour of vapours is already possible within tolerably narrow limits, and which leads us on towards the solution of the more general problem on which we are engaged. I shall denominate this peculiarity the levelling (*Nivelliren*); the facts referring to it afford a purely empirical, but at the same time invaluable, means of distinguishing different colours in their action on iodide of silver. The theoretical consideration of this peculiarity belongs, as it seems to me, to another class of interesting facts, which I hope to bring together at another opportunity.

Bring an iodized silver plate into the camera obscura, and leave it there until the image has arrived at the state which may be generally called "that of Daguerre." No trace of the image is seen on the plate, as is well known, although it is present and may be brought out by means of mercurial vapours. If this plate be now exposed to day- or sun-light, strange to say, no image is formed, but the plate is only uniformly blackened. In such a case I should say that the undecomposed light has "levelled" (*nivellirt*) the image, for there was no apparent difference between the affected and unaffected parts. What is still more extraordinary is, that if the plate be allowed to remain still longer in the camera obscura, so that a negative image is formed, it is also washed out by undecomposed light, and only remains fixed when the negative image has reached one of the higher stages of development, and in this case it is destroyed neither by undecomposed light nor by any colour whatever, but it remains for some time unchanged, and then passes into a positive image, as has been already described. This destruction of the images by undecomposed light does not depend upon the simultaneous action of the differently coloured rays of which it is composed, because blue and violet light, and partly the green, are capable of performing the same thing. The first two will always destroy the image if it is in the Daguerrian state, or even a negative one, if not very strongly formed, and indeed just as well as day- or sun-light.

If we consider the above-mentioned fact, keeping in mind the axiom which we have proposed concerning the action of the several colours, we shall arrive at an idea of the phænomenon of *levelling*, which agrees sufficiently well with the reality. When the ordinary rays in a camera obscura have only acted a short time on the iodide of silver, then rays of any degree of refrangibility are capable of levelling the image, which is always supposed to exist, although means have not been employed to render it apparent. If the rays have acted for a longer time, then all of them except the red are able to level the image, and this is because the rays of this colour differ sufficiently in refrangibility from those which had commenced the action. If the plate remains still longer in the camera obscura, we may suppose that the effect has been produced by still more refrangible rays than the blue, violet, or dark parts of the spectrum, and now a uniform illumination of the image with yellow light is not capable of destroying or levelling it, &c. Finally, if the plate has remained several hours or even days in the camera obscura, then the blue and violet rays are not capable of levelling the image, as has been shown above. We will now extend the discussion to the invisible rays, which have been proved to possess the greatest degree of refrangibility. In this case also they justify the character which has been given to them, and consequently the process of levelling furnishes us with a means of determining the relative refrangibility in cases where the methods employed with the ordinary rays are of no avail whatever. As the most refrangible rays are capable of levelling the image while it is in the Daguerrian state, the invisible rays will also be able to effect it. In order to prove this I allowed several iodized plates to remain so long in the camera obscura that they would have produced the images with great accuracy if afterwards treated with mercurial vapours. I then placed these plates on pure silver, gold, copper, mirror-metal, iodized silver, and porcelain, either in such a manner that a contact (although only partial) took place, or else so that they could not touch each other on account of the interposition of strips of mica. When the plates had lain a considerable time in the dark the images were for the most part completely levelled; in some cases a trace of them was made visible by the mercurial vapours, but even then only on certain parts of the plate. The invisible rays had in this case levelled the images produced by the visible rays; they are capable of doing the same with images produced by their own means, if they are still in their earlier stages of development. Bodies of silver, gold, iron, and horn were laid upon an iodized plate of silver and kept there for an hour, so that their images were formed. However, after a plate of silver and iron had lain upon them for several hours, the images were completely levelled and could not be rendered visible by any means; but if the image produced by the invisible rays is in a more advanced state, then it cannot be destroyed by others of the same kind, of which fact I have convinced myself by a number of experiments.

The visible rays, on the other hand, are in no case capable of levelling an image produced by invisible light, and by this it is evident that the latter consists of rays more refrangible than those of the ordinary prismatic spectrum. It would be useless to adduce proofs of this statement, because I am indebted for the knowledge of the identity in the action of light on all bodies, and the extension of Daguerre's discovery to all vapours, to the circumstance that visible rays are not capable of destroying images produced by those which are invisible.

As may be seen, the levelling by rays of different refrangibilities provides us with a very desirable means of distinguishing the group of the invisible rays from that of the ordinary ones, and it is no longer necessary to make our experiments on the rays of the first kind in so-called dark chambers and at night, in order to remove any possible objection. The two kinds of rays are so easily distinguishable, that I might proceed to the solution of the question as to whether or not there are any rays in day- and sun-light which possess the refrangibility of the invisible ones. That there are none such present in the camera obscura, is evident from the fact that every image which is in the Daguerrian state, or even a little more developed, is destroyed by means of the blue and violet rays. But this might possibly arise from the lens having the power of absorbing these invisible rays, which I will again remark must not be confounded with Ritter's dark rays. It was necessary then to act with pure sunlight, and for that purpose I placed a figured screen over and at a sufficient distance from an iodized plate of silver, and exposed the whole for one or two seconds to the sun. On being now placed in the sun under a blue glass, no image was produced. In order to render this negative result more positive, I exposed the plate with its screen to the sun for three seconds, and during the eclipse of the 8th of July for fourteen seconds, so that a very clear picture of the excised part was rendered visible. The plate was then laid in the sun under a blue glass, and in a very short time the image was completely levelled and destroyed. Images of this kind were much more plainly formed under red, yellow, and green glasses. It has hereby been proved, that among the various rays of the sun the invisible ones are not present. Nor are they to be found in daylight, a fact of which I have convinced myself by several experiments.

If these rays, which are emitted by every body considered as self-luminous, are wanting in the sun, the cause of it may be, that they are absorbed during their passage through the atmosphere. But in respect to this other opinions are possible, depending upon the motions of elastic bodies, but which must not be mentioned here, more particularly as the experiments which I have made, with a view of establishing one theory or the other, have as yet yielded no positive result.

It is now possible to determine the latent colour of some vapours, and I will first direct my attention to that of mercury. Its latent colour is not blue or violet, for these colours level the images that are in the Daguerrian state, while the vapours of mercury render them visible. It may be said that it is very fortunate for the science of Daguerreotype, that this vapour does not possess blue light, for in this way it would not be possible with the visible rays of light. From what has been said above, it is also quite evident that the latent colour of this vapour cannot be white. Moreover it cannot be red; for if an iodized plate be allowed to remain only a short time in the camera obscura, no image is produced on it by means of mercurial vapours, although red glass soon renders it apparent. I will only mention one of the numerous experiments that I made on this point; on a day when the time necessary for the production of a good Daguerrian image was ten minutes, I placed the plate of silver in the apparatus for only half a minute to one minute, and on covering it with a red glass I afterwards obtained a perfect negative image. I then exposed it to the action of light in the camera obscura for one, two, and three minutes, and afterwards treated it with mercurial vapours, but then no picture was produced. A similar plate was allowed to remain for three days over mercury, which was several times heated to 60° R. and upwards, and yet only a uniform layer of condensed mercurial vapour was produced, and no trace of an image.

At different times, and when I had become acquainted with the general behaviour of vapours, I was much astonished at this result of many experiments; now that we consider the latent colour of these vapours, it will be evident that nothing else could take place. To return once more to Daguerreotype, we cannot help lamenting that the latent colour of the vapour of mercury is not red; in that case its images would be formed in a much shorter space of time, and would not probably possess so gray a tint.

The colour of the latent light of the vapours of mercury is yellow; all phænomena with which I am acquainted agree with this supposition. In that state of the image on iodide of silver in which it can be destroyed by yellow rays, the vapours of mercury cause its appearance; but in the earlier stages the image produced by ordinary light is "levelled" both by the yellow rays and the vapours of mercury. Yellow light very easily produces blackening, and the same is the case with the mercurial vapours; and indeed it appears as if the usual gray or brownish tint of Daguerrian pictures, particularly when exhibiting much detail, depends upon a partial blackening caused by the mercury.

Just as yellow light most easily converts the negative image, as obtained in the camera obscura, into a positive one, so also the vapours of mercury easily invert the image, if allowed to act for a longer period, or employed of a greater tension, as I have already shown in my former treatise. Yellow light produces the metamorphosis by blackening the unaffected iodide, and by decolorizing that which has been already blackened. Vapours of mercury exert a similar action on the parts not affected, which represent the shaded portions of the object; it here commences its action, becomes on these parts more strongly condensed, and consequently alters the appearance of the picture. The latter remains unaltered for several days if suspended over mercury occasionally warmed, a fact of which I have lately convinced myself. According to this we must modify the opinion which I expressed in my former treatise, viz. that on continuous action the mercury disappeared from those parts where it had at first been condensed.

When yellow light inverts a negative image, a moment must arrive when no image whatever can be distinguished on the plate, and afterwards a positive one is formed. If we now remove the plate from the action of yellow light and expose it to the vapours of mercury, a positive image is produced, which cannot be distinguished from that formed by the yellow glasses. The quantity of mercurial vapours which is precipitated on the plate in one kind of this experiment, is too inconsiderable to produce any perceptible difference in the two images.

Moreover, the behaviour of the vapour of mercury towards the luminous effects produced by the invisible rays, is as characteristic as it is conclusive of its latent colour. It never destroys an image produced by these rays, but it often produces it by very continuous action, as I have stated in my former treatise; the vapours of mercury produce the image with rapidity only when the invisible rays have acted for a long time. The true reason of this was not at that time very clear, now it is quite evident, for the same is the case with the yellow rays. It does not destroy an image produced on iodide of silver by invisible rays; but a long time is required even in the sun before an image in its earlier stages makes its appearance; and even then we usually see only traces of the picture, as already stated, probably because yellow and red glasses transmit a large quantity of heat, by which the iodide is altered and the experiment disturbed.

The colour of the latent light of the vapour of iodine is either *blue* or *violet*; for this vapour levels every image in the Daguerrian stage without fail, and even a partially developed negative one; consequently this vapour does not contain either red, green, or yellow light in a latent state, but behaves just like blue or violet glasses. When however the negative image is strongly formed, as for instance by allowing the iodized plate to remain ten hours in the camera obscura, then the vapour of iodine is not capable of levelling it even by the most continuous action. The image is always visible, notwithstanding the manifold colours which are produced.

While on the one hand the vapours of iodine easily and completely destroy images produced by ordinary rays of light, they bring out those formed by the invisible rays just as well as blue or violet glasses, and in this respect may be very advantageously employed. I placed an engraved brass plate, an iron body, a small plate of silver, and a ring of black horn upon an iodized sheet of silver, and allowed them to remain there for several hours. The plate afterwards exhibited nothing whatever, but on being exposed to vapours of iodine until the iodide acquired a blue colour, the images became plainly visible, and when placed under a blue glass were developed with all their finer details. My former treatise contains several other proofs of this kind.

If we consider the nature of these vapours, we must conclude that the iodizing the silver plates does not under all circumstances increase their sensibility. It is true, that if we operate with ordinary rays, as in a camera obscura, it is then very necessary to iodize the silver, whéreby it is exposed to the action of the blue or violet light which is being set free. But this operation cannot be of any service when we employ invisible rays; it must in this case be as much a matter of indifference whether the plate be previously iodized, as it is in ordinary experiments whether the iodized plate be exposed to yellow or red light before being introduced into the camera obscura. It may at first appear somewhat singular to allow the light to act first on the pure silver and then the iodine, in order to obtain a visible image; but the idea proceeds from the nature of the invisible rays and of the vapour of iodine, and it is confirmed by experiment. Bodies of brass, silver, horn, glass, &c. were laid upon a plate of pure silver only for two minutes. On introducing it into the vapours of iodine the images became visible although only weak; they became much stronger and better defined under blue glass or in daylight. I have had occasion to make many similar experiments, and have convinced myself, that as far as regards the invisible rays iodized silver possesses no advantages over the pure metal. The only circumstance which can render the iodizing desirable is, that after the plates have been polished they may be covered with a very uniform coating of iodide.

As this is the case I was enabled to remove a difficulty which I at first met with, and also to give a further proof, if such be necessary, of how insufficient this kind of experiment is for the *measurement* of the intensities of light. When I examined the action of ordinary light on pure silver, copper and glass, I was compelled to employ sun-light for one or two hours, in order then to obtain images by means of the vapours of mercury, water or iodine. If I had taken iodide of silver, a quarter of a second would have been sufficient to allow the vapours of mercury to exert their influence in the production of the pictures; consequently pure silver was much inferior to the iodide in point of sensibility. But this was not the case in other experiments; in those, the success of which in my former memoir I attributed to contact, I had procured images in the course of ten, and afterwards in that of two minutes. In this case pure silver, and other bodies besides, proved to be very sensitive, and by no means inferior to the iodide. This contradiction is now explained, and in such a manner as to allow us to suppose that it is very different with regard to the sensibility of various bodies to what we should have as yet conceived. We may consider that the most refrangible rays, or those whose time of oscillation is the least, are the most intense in regard to the effects we are now speaking of; that is to say, they are most fitted for commencing the action. If we extend this assumption to the invisible rays, which possess very great refrangibility, it is natural that they should act most powerfully on pure metals, glass, &c., while visible light has such very little effect upon these bodies. In the case of an influence exerted upon pure silver, these latter rays stand in the same relation to the visible ones as the red or yellow to the blue, in respect to their action on iodide of silver; consequently ordinary light must be employed in a very intense state, and for a great length of time, if its action is to be rendered apparent by means of vapours. If however the silver has been iodized, then the blue or violet latent light of this vapour has acted on the substance, and we may see how it might have become more sensitive towards ordinary light. As I have not made any experiments on other iodized metals, I cannot at present pursue this subject further, but must return to the latent colours of other vapours.

With regard to those of chlorine, bromine, chloride and bromide of iodine, they cannot be very if at all different from that of the vapours of iodine. These vapours possess the same general properties as iodine; and if the refrangibility of their latent light is different, the variations can only be such as cannot for the present be further examined.

The vapours of water behave precisely like those of iodine in all experiments with which I am acquainted; where one is capable of destroying an image the other does so likewise; where one can produce an image it is also rendered visible by the other. Every image which is still in the Daguerrian state is destroyed by the vapours of water, and if it be breathed on after being removed from the camera obscura, or exposed to the vapours of warm water, it is so completely destroyed that vapours of mercury do not render it again visible. If however the invisible rays have produced an image on any substance whatever, the vapours of water do not destroy it; on the contrary, they make it appear. If it is very faint at first, repeated breathing on it often renders it more defined.

Suppose an iodized silver plate which has received an image, which however is not yet outwardly visible. If the image was produced in the camera obscura, or, speaking generally, by ordinary light, when exposed to the vapours of iodine, water, or to the action of blue glass, it not only does not appear, but is "levelled" and destroyed. If, on the contrary, it was formed by the invisible rays, it is rendered visible by the vapours of iodine or water, and by the blue glass.

If, from this similarity in the action of the vapours of iodine and water and blue and violet glass, any one should be inclined to conclude that the vapours of water contain blue or violet light in a combined state, which is set free during the act of condensation, I must remark that the following circumstances are to be considered:—

1. That as yet I have discovered no difference between blue and undecomposed (white) light.

2. That the research has not yet been carried far enough to enable us to distinguish between rays of nearly the same refrangibility, as for instance between blue, violet, and dark (not invisible) light.

As the vapour of water is of such great importance, it is not my wish that the examination of its latent colour should appear more complete than is as yet possible; and I must content myself for the present with having proved that this latent colour is certainly not green, yellow, orange, or red, but that it appears to belong to the ordinary prismatic colours, inasmuch as I have never seen it destroy an image produced by the invisible rays.

I will conclude this Memoir with an example, showing how easily somewhat complicated phænomena may be explained by the knowledge of the latent colour of different kinds of vapour; the example has also considerable practical importance as regards the performance of experiments of this class. If we allow invisible light to act for a comparatively short time on pure copper, when it is exposed to the vapours of mercury of ordinary tension, an image is obtained after the lapse of some hours. If the plate be removed at an earlier period, there is frequently not a trace of the picture to be seen. If however it be now introduced into the vapours of iodine, a perfect image becomes apparent in a few seconds. I had accidentally discovered this method of operation, and described it in my former treatise, without however understanding the reason why the previous application of the mercurial vapours should be so advantageous, although of themselves they frequently produce no image whatever. We now see that it depends on the yellow latent colour of the vapour. It is very evident that yellow light stands in the same relation to invisible light, particularly when it has acted for some time, that the red does to the violet or dark rays; and if we first expose a plate of copper which has received an impression from the invisible rays to the vapours of mercury, and then to those of iodine, it is just the same as if we expose an iodized plate of silver to the uniform action, first of red, and then of green or yellow light. After the rationale of the process had been thus found, it was natural to suppose that it might be generally advantageous,—an idea which has been confirmed by a series of experiments that I made by allowing invisible rays to act upon gold, silver, German silver, brass, iron, steel, zinc, and even japanned plate. The most complete inversion of Daguerre's process that is possible, and which depends upon the employment of light of a greater rapidity of oscillation, is to act upon pure silver, first with light, then with mercury, and lastly with the vapours of iodine.

Königsberg, July 1842.

Note on the preceding Treatises of M. Moser.

The results of the researches of M. Moser were first made known in this country at the meeting of the British Association at Manchester in June last, by M. Bessel. The statement of them by Sir D. Brewster excited much interest*; it has been thought desirable therefore to give a translation of the three Memoirs of M. Moser, being all that he has published on those properties of light of which they treat. Since attention was first called to these curious phænomena by Dr. Draper of New York in 1840†, and subsequently by M. Moser, the following communications on the subject have been published in the London, Edinburgh and Dublin Philosophical Magazine:—

"On the Chemical Action of the Solar Spectrum on preparations of Silver." By Sir J. F. W. Herschel, Bart.—Phil. Trans. Feb. 1840, and Phil. Mag. S. 3. vol. xvi. p. 331.

"On the Action of the Rays of the Solar Spectrum on Vegetable Colours, and on some new Photographic Processes." By Sir J. F. W. Herschel, Bart. Communicated to the Royal Society June 15, 1842.—Phil. Trans. 1842, and Phil. Mag. S. 3. vol. xxii.

"On certain Spectral Appearances, and on the Discovery of Latent Light." By J. W. Draper, M.D., Professor of Chemistry in the University of New York.—Phil. Mag. p. 348, Nov. 1842.

"On a new Imponderable Substance, and on a Class of Chemical Rays analogous to the Rays of Dark Heat." By Prof. Draper.—Phil. Mag. Dec. 1842.

"On Thermography, or the Art of Copying Engravings from Paper on Metallic Plates; and on the recent Discovery of Moser relative to the formation of Images in the Dark." By Mr. Robert Hunt.—Phil. Mag. Dec. 1842.

"On the Action of the Rays of the Solar Spectrum on the Daguerreotype Plate." By Sir J. F. W. Herschel, Bart.—Phil. Mag. Feb. 1843.—See remarks in this Paper on the use which Moser has made of Coloured Glasses.

Also, a communication by Prof. Draper "On the Rapid Detithonizing Power of certain Gases and Vapours, and on an instantaneous means of producing Spectral Appearances," is announced for publication in the Philosophical Magazine for March.

The following extract of a Letter from M. Fizeau to M. Arago is from the *Comptes Rendus*, Nov. 7, 1842:—

On the Causes which concur in the production of the Images of Moser.

"Since my return I have been actively occupied with the singular phænomena observed by M. Moser, and I hope shortly to have the honour of laying before the Academy a communication on this subject. At present I shall therefore merely apprise you of the general results to which I have come. The experiments which I have as yet made have for the most part confirmed the facts announced; but I must say that all have led me to take quite a different view of this subject from that which M. Moser does.

"Far from thinking that we must admit the existence of a new species of

* See Phil. Mag. Nov. 1842, p. 409.

† See the following communications by Professor Draper in the London, Edinburgh and Dublin Philosophical Magazine:—

1840. Feb. p. 81.—"An Account of some Experiments made in the South of Virginia on the Light of the Sun." [In this letter Dr. Draper refers to previous communications in the Franklin Institute.]

1840. Sept. p. 217.—"On the Process of Daguerreotype, and its application to taking Portraits from the Life."

1841. Sept. pp. 198, 199; 196, 204, 205, 206.—"On some Analogies between the Phænomena of the Chemical Rays and those of Radiant Heat."

radiations escaping from all bodies, even in complete darkness, and subjected in their emission to laws entirely peculiar, I am convinced that no kind of radiations whatever are to be had recourse to for the explanation of these phænomena, but that we should rather connect them with the known facts which I shall now mention.

"1. Most of the bodies upon which we operate have their surface clothed with a slight layer of organic matter, analogous to the fatty bodies, and volatile, or at least susceptible of being carried off by aqueous vapour.

"2. When vapour is condensed on a polished surface, if the different parts of this surface are unequally soiled by extraneous bodies, even in an exceedingly minute quantity, the condensation is effected in a manner visibly different on the different parts of this surface.

"When therefore we expose a polished and pure surface to the contact of, or at a small distance from, any body whatever with an unequal surface, it will happen that a part of the volatile organic matter which covers this latter surface will be condensed by the polished surface in the presence of which it is; and as I have supposed the body to present inequalities or projections and hollows, that is to say, its different points to be unequally distant from the polished surface, the result of this will be an unequal transfer of the organic matter on to the different points of this surface; at the points corresponding to the projections of the body the polished surface will have received more, and at the points corresponding to the hollows it will have received less: hence, then, there will result a kind of image, but generally invisible. If a vapour be then condensed on this polished surface, we see that it is then under the conditions which I just now mentioned, and that the condensation will take place in a manner visibly different upon the different points, that is to say, the invisible image will become visible.

"Here, then, in brief, is the notion which my experiments have led me to form on the subject of the new phænomena observed by M. Moser. In this point of view the study of them doubtless presents less interest than in that of the physicist of Königsberg; yet the singular part which this organic matter, which is found on the surface of almost all bodies, appears to perform here, allows us to hope for some light as to its nature and its properties, as yet so little known."

To this we may add a Note by the writer of an Article on the subject in the Edinburgh Review for January 1843, p. 343:—

"We have found that many of the phænomena ascribed to *latent light*, or to *heat*, are owing to the absorption of matter in the state of vapour or minute particles, passing from the object to the surface of the glass or metal upon which the image of that object is impressed; and by this means we have obtained very fine pictures upon glass, which are *positive* when seen by reflection and *negative* when seen by transmitted light. These pictures are rendered visible by the vapour of water, &c."—See also the Report of Prof. Grove's Lecture, delivered at the London Institution, January 18, in the 'Literary Gazette' for January 21, 1843.

NOTE.—That light is not an emission of luminous particles, but "a substance distinct from all other, existing in darkness, expanded through all things at all times [in a *latent* or *invisible* state], and *rendered visible* by being properly *excited*," is a notion suggested by Dr. John Taylor in his 'Scheme of Scripture Divinity,' a posthumous work, published by his son, Mr. Richard Taylor of Norwich (my grandfather), in 1762, and republished by Bishop Watson in the first volume of his 'Collection of Theological Tracts, Cambridge 1785.'—R. T.

Article XIX.

Abstract of some of the principal Propositions of Gauss's *Dioptric Researches*.*

1. THE demonstrations here offered of some of the principal propositions of the Dioptric Researches of Gauss, though less elegant and perhaps less rigorous than those given by the author, are rather shorter, and more in accordance with the methods pursued by the optical writers of this country.

The author begins by observing that the consideration of the path of a ray making a very small angle with the axis of the lenses through which it is refracted affords results of great elegance, which, though they may appear to have been exhausted by the labours of Cotes, Euler, Lagrange, and Möbius, still leave much to be desired. An essential defect in the propositions enunciated by those mathematicians is, that the thickness of the lens is neglected, an omission that impresses upon the investigations a character of inaccuracy that greatly diminishes their value. The idea of the axis and of the focus of a lens is perfectly clear. Not so that of the focal length, which most writers define as the distance of the focus from the middle point of the lens, having previously assumed, either tacitly or expressly, that the thickness of the lens is indefinitely small, therefore in practice admit an inaccuracy of the order of the thickness of the lens. When an attempt is made to define the focal length more accurately, it is stated to be either the distance of the focus from the nearest surface of the lens, or its distance from the centre of the lens, or else its distance from a point half-way between the two surfaces of the lens. Different from all of these is that value of the focal length which must be assumed in order that the linear magnitude of the image of an indefinitely distant object may correspond to its angular magnitude. This last is in fact the only correct value of the focal length.

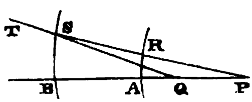
The author then proceeds to say that he does not consider it superfluous labour to devote a few pages to this perfectly elementary investigation, principally in order to show that, in the elegant

* Communicated by W. H. Miller, Esq., Professor of Mineralogy in the University of Cambridge.

theorems already mentioned, the thickness of the lens may be taken into account without any loss of simplicity; the only limitation retained being that the inclination of the rays to the axis is very small, or that the spherical aberration is neglected.

[It is worthy of observation, that in most of the elementary treatises on optics in use at present in this country, the point in the lens from which the focal length is measured is distinctly stated. Also those points in a single lens which are called "haupt-puncte" by M. Gauss, have been long known to English optical writers under the name of "focal centres." (Wood's 'Optics,' fifth edition, Cambridge, 1828, Art. 165. Coddington 'On Reflexion and Refraction,' Art. 69.) An investigation of the principal relations between the distances of the conjugate foci of a thick lens measured from the focal centres will be found in Wood's 'Optics,' Arts. 194. 469. 470.—Ed.]

2. To determine the path of a ray after refraction through any number of media separated by spherical surfaces having their centres in the same straight line.



Let R S T be the path of a ray through two media bounded by spherical surfaces meeting the line through their centres in A, B, R S and therefore S T being in the plane A B S. Let R S, S T meet the axis A B in P, Q; then μ, μ' being the indices of refraction of the media in which R S, S T lie, r the radius of the surface B S, supposing the inclination of the ray to the axis to be very small, ultimately

$$\frac{\mu'}{\mathrm{QB}} - \frac{\mu}{\mathrm{PB}} = \frac{\mu' - \mu}{r},$$

whence, observing that

$$\frac{\mathrm{SB}}{\mathrm{PB}} = \frac{\mathrm{RA}}{\mathrm{PA}}, \quad \mu' \frac{\mathrm{SB}}{\mathrm{QB}} = \mu \frac{\mathrm{RA}}{\mathrm{A}} + \frac{\mu' - \mu}{r}\,\mathrm{SB},$$

also

$$\mathrm{SB} = \mathrm{RA} + \frac{\mathrm{RA}}{\mathrm{PA}}\,\mathrm{AB}.$$

When BS is a reflecting surface we have only to put $\mu' = -\mu$. By this substitution the whole of the following investigation may be extended to the case in which one or more reflexions occur.

3. Let A B be the nth of a series of $s + 1$ media separated by

spherical surfaces having their centres in the same straight line, μ_{2n} its index of refraction, μ_{2n+2} the index of the following medium B T:—

$$AB = \frac{t_{2n}}{\mu_{2n}}, \quad AR = y_{2n-1}, \quad BS = y_{2n+1},$$

$$\tan SPB = \frac{\beta_{2n}}{\mu_{2n}}, \quad \tan SQB = \frac{\beta_{2n+2}}{\mu_{2n+2}}, \quad r_{2n+1} \text{ the radius}$$

of B S, $(\mu_{2n+2} - \mu_{2n}) \dfrac{1}{r_{2n+1}} = \varrho_{2n+1}$.

Then

$$\beta_{2n+2} = \beta_{2n} + \varrho_{2n+1}\, y_{2n+1},$$
$$y_{2n+1} = y_{2n-1} + t_{2n}\, \beta_{2n}.$$

Therefore, substituting 1, 2, 3, ... s for n successively,

$$\beta_2 = \beta_0 + \varrho_1\, y_1$$
$$y_3 = y_1 + t_2\, \beta_2$$
$$\beta_4 = \beta_2 + \varrho_3\, y_3$$
$$.. = ...$$
$$.. = ...$$
$$y_{2s+1} = y_{2s-1} + t_{2s}\, \beta_{2s}$$
$$\beta_{2s+2} = \beta_{2s} + \varrho_{2s+1}\, y_{2s+1}.$$

Whence it is easily seen that

$$\beta_2 = \varrho_1\, y_1 + \beta_0,$$
$$y_3 = (1 + \varrho_1\, t_2)\, y_1 + t_2\, \beta_0,$$
$$\beta_4 = (\varrho_1\, t_2\, \varrho_3 + \varrho_1 + \varrho_3)\, y_1 + (1 + t_2\, \varrho_3)\, \beta_0,$$
$$.. = ...$$
$$y_{2s+1} = g\, y_1 + h\, \beta_0,$$
$$\beta_{2s+2} = k\, y_1 + l\, \beta_0,$$

where g, h are the numerator and denominator of the last but one; k, l the numerator and denominator of the last of the fractions converging to the continued fraction, of which the quotients are

$$\varrho_1, t_2, \varrho_3, \ldots\ldots\ldots, t_{2s}, \varrho_{2s+1}.$$

Hence, by a well-known property of converging fractions,

$$g\, l - h\, k = 1,$$

$$\therefore y_1 = l\, y_{2s+1} - h\, \beta_{2s+2}, \quad \beta_0 = g\, \beta_{2s+2} - k\, y_{2s+1}.$$

4. Let A, B be the points in which the axis intersects the first and last surfaces; P, Q the points in which the axis is inter-

sected by the directions of a ray in the first and last media, or the conjugate foci of a small direct pencil refracted through all the surfaces; O a fixed point beyond the last medium; a, b, p, q the distances of A, B, P, Q from O; then

$$\frac{\beta_0}{\mu_0} = \frac{y_1}{p-a}, \quad \frac{\beta_{2s+2}}{\mu_{2s+2}} = \frac{y_{2s+1}}{q-b},$$

$$\therefore y_{2s+1} = g y_1 + h \frac{\mu_0 y_1}{p-a},$$

$$\frac{\mu_{2s+2}\, y_{2s+1}}{q-b} = k y_1 + l \frac{\mu_0 y_1}{p-a}.$$

Whence, writing μ, ν for μ_0, μ_{2s+2} respectively,

$$k \frac{p-a}{\mu} \frac{q-b}{\nu} + l \frac{q-b}{\nu} - g \frac{p-a}{\mu} = h.$$

Let $p - a = u + \mu\theta$, $q - b = v + \nu\phi$, and we get

$$k \frac{uv}{\mu\nu} + l' \frac{v}{\nu} - g' \frac{u}{\mu} = h',$$

where $g' = g - k\phi$, $l' = l + k\theta$, $h' = h + g\theta - l\phi - k\theta\phi$.

Let $h' = 0$ and $g' = l'$, $\therefore g - k\phi = l + k\theta$, $0 = h + g\theta - l\phi - k\theta\phi$, whence $l + k\theta = 1$, $g - k\phi = 1$,

$$\therefore \frac{\nu}{v} - \frac{\mu}{u} = k,$$

where $u = p - \left(a + \mu \frac{1-l}{k}\right)$, $v = q - \left(b + \nu \frac{g-1}{k}\right)$.

In the above equation u, v are the distances of the conjugate foci from two fixed points M, N, such that m, n being the distances of M, N from O, $m = a + \mu \frac{1-l}{k}$, $n = b + \nu \frac{g-1}{k}$.

M, N are called the first and second focal centres of the system of surfaces.

5. Let the directions of the ray in the first and last media meet the axis in P, Q, and perpendiculars to the axis through M, N in S, T. Then

$$\text{S M} = \frac{\beta_0}{\mu} \text{P M} = \frac{\beta_0}{\mu}\left(p - a - \mu \frac{1-l}{k}\right) = y_1 - \beta_0 \frac{1-l}{k},$$

$$\text{T N} = \frac{\beta_{2s+2}}{\nu} \text{Q N} = \frac{\beta_{2s+2}}{\nu}\left(q - b - \nu \frac{g-1}{k}\right) = y_{2s+1} - \beta_{2s+2} \frac{g-1}{k}$$

$$= g y_1 + h\beta_0 - (k y_1 - l\beta_0)\frac{g-1}{k} = y_1 - \beta_0 \frac{1-l}{k}.$$

$$\therefore \text{TN} = \text{SM}.$$

This result, combined with the equation expressing the relation between u, v, shows that, when μ, ν are unequal, the path of the ray in the last medium is exactly the same as if the distance between the focal centres M, N had been annihilated, and the first and last media separated by a spherical surface having for its radius $\frac{\nu - \mu}{k}$, passing through the point in which M, N are supposed to coincide. When μ, ν are equal, the path of the ray in the last medium is the same as if a thin lens, having $\frac{\mu}{k}$ for its focal length, had been placed, *in vacuo*, at the point in which M, N are supposed to coincide.

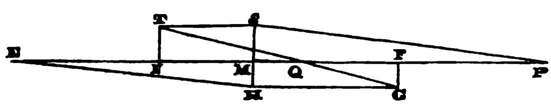
6. Let E be the place of P when Q is indefinitely distant, F the place of Q when P is indefinitely distant; e, f the distances of E, F from O, $u = p - m$, $v = q - n$, $\therefore \frac{\nu}{q-n} - \frac{\mu}{p-m} = k$,

when $q = \infty$, $p = e$, when $p = \infty$, $q = f$, $\therefore m - e = \frac{\mu}{k}$, $f - n = \frac{\nu}{k}$,

$$\therefore e = a - \mu\frac{l}{k},\ f = b + \nu\frac{g}{k},\ \frac{f-n}{q-n} + \frac{e-m}{p-m} = 1.$$

Whence $(p-e)\ (f-q) = (m-e)\ (f-n)$.

Or PE . FQ = ME . FN, PE, FQ being measured in different directions from E, F.



Let PS, the path of a ray in the first medium, and a parallel to it through E, meet a perpendicular to the axis through M in S, H. Draw ST, HG parallel to the axis meeting perpendiculars to the axis through N, F, in T, G. Then GT will be the path of the ray in the last medium; for

$$\frac{\text{QF}}{\text{FG}} = \frac{\text{FN}}{\text{SH}},\ \frac{\text{PE}}{\text{SH}} = \frac{\text{EM}}{\text{MH}},\ \therefore \text{PE}.\text{FQ} = \text{ME}.\text{FN}.$$

7. When $u = 0$, $v = 0$, therefore M, N are conjugate foci, or when the path of a ray in the first medium passes through M, its

direction in the last passes through N. Let the inclinations of such a ray to the axis in the first and last media be ε, η respectively.

$$\beta_{2s+2} = k y_1 + l\beta_0, \quad \therefore \nu\eta = k y_1 + l\mu\varepsilon,$$

but

$$k y_1 = \mu\varepsilon(1 - l), \quad \therefore \nu\eta = \mu\varepsilon.$$

Hence if a small object at the distance u from M subtend an angle ε at M, its image will be at the distance v from N, and will subtend an angle η at N.

$$\therefore \frac{\text{linear mag. image}}{\text{linear mag. object}} = \frac{\mu v}{\nu u} = \frac{m - e}{p - e} = \frac{f - q}{f - n}.$$

The image will be erect or inverted according as the numerator and denominator in the above expressions have the same or different signs.

8. When $k = 0$, the points M, N, E, F are indefinitely distant. In this case let the first and last surfaces meet the axis in A, B. Let A O $= a$, B O $= b$; then, since $k = 0$, the expressions for y_{2s+1}, β_{2s+2} become

$$y_{2s+1} = g y_1 + h\beta_0, \quad \beta_{2s+2} = l\beta_0, \quad g l = 1,$$

$$\frac{\beta_0}{\mu} = \frac{y_1}{p - a}, \quad \frac{\beta_{2s+2}}{\nu} = \frac{y_{2s+1}}{q - b},$$

whence

$$\frac{q - b}{\nu} = g h + g^2 \frac{p - a}{\mu},$$

$$\beta_{2s+2} = l\beta_0 = \frac{\beta_0}{g}.$$

When $p - a = \infty$, $q - b = \infty$, therefore when the rays of a pencil in the first medium are parallel, the rays in the last medium are also parallel. The ratio of the angles the incident and emergent rays make with the axis is in all cases invariable.

A telescope adapted to an object indefinitely distant and to a long-sighted eye, is an example of the preceding case. If the first and last media are the same, and if β, γ denote the angles which the incident and emergent rays make with the axis,

$$\frac{\gamma}{\beta} = l = \frac{1}{g}.$$

On this property depends the method of determining the power of a telescope proposed by Gauss in the second volume of the *Astronomische Nachrichten*, as well as the dynamometers of Ramsden and Plössl.

9. In a single lens r, s being the radii of the first and second surfaces, t its thickness, and μ its index of refraction, the indices of the first and last media will each be unity,

$$\varrho_0 = \frac{\mu - 1}{r}, \quad t_1 = \frac{t}{\mu}, \quad \varrho_1 = \frac{1-\mu}{s},$$

whence

$$g = \frac{\mu-1}{\mu}\,\frac{t}{r} + 1, \quad h = \frac{t}{\mu}, \quad l = 1 - \frac{\mu-1}{\mu}\,\frac{t}{s}.$$

$$k = (\mu-1)\left(\frac{1}{r} - \frac{1}{s} - \frac{\mu-1}{\mu}\,\frac{t}{rs}\right),$$

$$m - a = \frac{1-l}{k} = \frac{rt}{\mu(s-r-t)+t},$$

$$n - b = \frac{g-1}{k} = \frac{st}{\mu(s-r-t)+t},$$

$$\frac{1}{v} - \frac{1}{u} = (\mu-1)\left(\frac{1}{r} - \frac{1}{s} - \frac{\mu-1}{\mu}\,\frac{t}{rs}\right).$$

$$(p-e)\,(f-q) = \frac{1}{(\mu-1)^2\left(\frac{1}{r} - \frac{1}{s} - \frac{\mu-1}{\mu}\,\frac{t}{rs}\right)^2}.$$

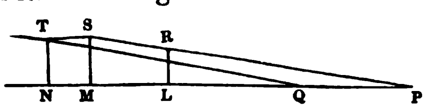
$$m-n = a-b - \frac{(s-r)\frac{t}{\mu}}{s-r-(\mu-1)\frac{t}{\mu}} = a-b - \frac{1}{1-\frac{\mu-1}{\mu}\,\frac{t}{s-r}}\,\frac{t}{\mu},$$

$$= \frac{\mu-1}{\mu}\,t + \frac{\mu-1}{\mu(s-r)-(\mu-1)t}\,\frac{t^2}{\mu}, \text{ since } a-b=t,$$

$$= \frac{\mu-1}{\mu}\,t + \frac{\mu-1}{\mu(s-r-t)+t}\,\frac{t^2}{\mu},$$

or distance between focal centres $= \frac{\mu-1}{\mu}t$ nearly.

10. To determine the path of a ray after refraction through any number of lenses having a common axis.



Let L be the second focal centre and M, N the first and second focal centres respectively of two lenses having a common axis, R S the direction of a ray after refraction through the first lens in the plane S M L, Q T its direction after refraction through the second lens, and $\therefore$ also in the plane S L M; f the focal length of the second lens. Let perpendiculars to the axis through L, M, N meet P S in R, S, Q T in T. Then

$$\frac{1}{\mathrm{QN}} - \frac{1}{\mathrm{PM}} = \frac{1}{f},$$

whence, observing that

$$\mathrm{TN} = \mathrm{SM}, \text{ and } \frac{\mathrm{SM}}{\mathrm{PM}} = \frac{\mathrm{RL}}{\mathrm{PL}}, \quad \frac{\mathrm{TN}}{\mathrm{QN}} - \frac{\mathrm{RL}}{\mathrm{PL}} = \frac{\mathrm{SM}}{f};$$

also

$$\mathrm{SM} = \mathrm{RL} + \frac{\mathrm{RL}}{\mathrm{PL}}\,\mathrm{ML}.$$

11. Let M, N be the focal centres of the nth of a series of lenses having a common axis, f_{2n+1} its focal length.

$$\mathrm{SM} = y_{2n+1}, \quad \frac{\mathrm{TN}}{\mathrm{QN}} = \beta_{2n+2}, \quad \frac{\mathrm{SM}}{\mathrm{PM}} = \beta_{2n},$$

$$\mathrm{LM} = t_{2n}, \quad \frac{1}{f_{2n+1}} = \varrho_{2n+1}.$$

Then

$$\beta_{2n+2} = \beta_{2n} + \varrho_{2n+1}\, y_{2n+1}$$

$$y_{2n+1} = y_{2n-1} + t_{2n}\, \beta_{2n},$$

whence, as in the former case, y, h, k, l retaining the same signification,

$$y_{2s+1} = g\,y_1 + h\,\beta_0,$$
$$\beta_{2s+2} = k\,y_1 + l\,\beta_0.$$

By a process exactly similar to that which has been employed in the case of a system of surfaces, if A be the first focal centre of the first lens, B the second focal centre of the last lens; M, N the first and second focal centres of the system of lenses; P, Q the foci of incident and emergent rays; E the principal focus of rays coming in the contrary direction; F the principal focus of rays coming in the same direction; O a fixed point in the axis beyond the last lens; a, b, m, n, p, q, e, f the distances of A, B, M, N, P, Q, E, F from O; u, v the distances of P, Q from M, N respectively; then

$$m = a + \frac{1-l}{k}, \quad n = b + \frac{g-1}{k},$$

$$\frac{1}{v} - \frac{1}{u} = k,$$

$$e = a - \frac{l}{k}, \quad f = b + \frac{g}{k}, \quad f - n = -(e - n) = \frac{1}{k},$$

$$(f - q)(p - e) = \frac{1}{k^2}.$$

12. The angles subtended at M, N by the object and its image respectively are equal.

$$\frac{\text{linear mag. image}}{\text{linear mag. object}} = \frac{v}{u} = \frac{m - e}{p - e} = \frac{f - q}{f - n}.$$

The image will be erect or inverted according as the numerators and denominators have the same or different signs.

13. In a combination of two lenses ϕ, ϕ_1 being the focal lengths; m, n the distances of the first and second focal centres of the first lens from the fixed point O; m^1, n^1 the distances of those of the second lens; $\bar{m}$, $\bar{n}$ the distances of the first and second focal centres of the combination,

$$\varrho_1 = \frac{1}{\phi}, \quad t_2 = n - m^1, \quad \varrho_3 = \frac{1}{\phi^1}, \quad a - b = m - n^1, \quad m = \bar{m}, \quad n = \bar{n},$$

$$\therefore g = \frac{n - m^1 + \phi}{\phi}, \quad h = n - m^1, \quad k = \frac{n - m^1 + \phi + \phi^1}{\phi\,\phi^1}, \quad l = \frac{n - m^1 + \phi^1}{\phi^1},$$

$$e = m - \frac{(n - m^1)\,\phi + \phi\,\phi^1}{n - m^1 + \phi + \phi^1}, \quad f = n^1 + \frac{(n - m^1)\,\phi^1 + \phi\,\phi^1}{n - m^1 + \phi + \phi^1},$$

$$\bar{m} = m - \frac{(n - m^1)\,\phi}{n - m^1 + \phi + \phi^1}, \quad \bar{n} = n^1 + \frac{(n - m^1)\,\phi^1}{n - m^1 + \phi + \phi^1},$$

$$\therefore \bar{m} - \bar{n} = m - n + m^1 - n^1 + \frac{(n - m^1)^2}{n - m^1 + \phi + \phi^1}.$$

Hence, when $m - m^1$ is small compared with either ϕ or ϕ^1, as is the case in an achromatic object-glass, the distance between the centres of the combination is very nearly equal to the sum of the distances between the focal centres of the separate lenses.

It is evident that all the formulæ of the present article may be applied without any change to the case in which instead of simple lenses partial systems of lenses are combined into one system.

Article XX.

An Account of the Magnetic Observatory and Instruments at Munich: extracted from a Memoir entitled 'Ueber das Magnetische Observatorium der Königl. Sternwarte bei München, von Dr. J. Lamont,' *Director of the Observatory.*

[Dr. Lamont's memoir commences with a review of the early history of our knowledge of terrestrial magnetism; after speaking of the discovery of the declination, and of its secular, annual, and diurnal changes, it notices that besides these more regular movements, there are others, which may be subdivided into,—movements of greater magnitude, resembling tempests of the atmosphere, felt simultaneously at distant places, and which may perhaps be connected with other phænomena of occasional occurrence and remarkable character,—and into movements of lesser magnitude, more transitory in their nature and occurring more frequently, but which are equally unamenable to any known laws. It then proceeds as follows]:—

The latest recognised and not yet completely known peculiarity relates to the different manifestation of the magnetic force in different parts of the earth's surface. The results obtained by single observers, and by numerous expeditions of discovery by sea and land, have pointed out a system in this respect, certainly resting upon laws, though not apparently connected with the form, or peculiarities of the surface, of the globe. They have also indicated in respect of time, a progressive movement of the whole system towards the east*. By virtue of this latter movement the relations observed in these countries two centuries ago have now almost reached the limits between Europe and Asia, whilst other parts of the system have gradually moved over to us from the westward. The cause of this singular phænomenon is the more difficult even to conjecture with probability, because in the whole compass of our knowledge concerning the forces acting on the surface of the earth, we know of no analogous fact.

The results thus generally indicated and referred to pointed to

* Dr. Lamont here refers to the movement in the *northern* hemisphere: the corresponding movement in the *southern* hemisphere is towards the west.—E. S.

an existing force, subject to many conditions, and fitted powerfully to excite the spirit of research. Nevertheless the questions which must have presented themselves when the phænomena were first perceived, as to the proper *site* of the magnetic force, or its connexion with the globe of the earth, as to the *laws* which govern both its *normal condition* and its *variations*, have as yet found no satisfactory solution; and if the brilliant discoveries of modern times in the domain of magnetism, and of its relationship or near connexion with other forces acting on the surface of the earth,—heat, electricity, and galvanism,—have appeared to point to *possible* connexions and *possible* modes of originization, no one has yet succeeded in deducing any determinate part of the phænomena of terrestrial magnetism from any one force.

Although the earlier magnetic researches, notwithstanding the continually increasing accuracy of observation, had not been successful in discerning relations subject to laws, the investigation continued both to gain greater extension, and to approximate gradually to astronomical methods, until, in 1839, by an impulse memorable in the annals of science, a series of observatories, similar to those for astronomical science, were established, extending to remote parts both of the northern and southern hemispheres, forming the foundation of a grand undertaking, which claimed the widest possible cooperation in order to ensure its success. It was under these circumstances that His Majesty, our gracious Sovereign, granted the foundation of the magnetic observatory which I am now about to describe.

Having treated hitherto in a general manner of the *object*, and of the *means* by which its attainment is contemplated, I now proceed to the mode of execution.

The new magnetic arrangements, so different in their origin from earlier proceedings, offer also in their mutual relations a feature as novel in science as it is gratifying. Whereas it has often happened in scientific researches, that from *different paths* having been chosen to obtain the same object, the combination, with a view to a general result, has been impeded, or at least rendered more difficult; we find in this case the *same* instruments, the *same* methods, and the *same* periods of observation at all the stations. The different establishments are parts of one great system of research, embracing as their sphere of action the terrestrial globe, the seat of the force which is the subject of investigation.

In order to cooperate in this system it was urgent to lose no time in the construction and establishment of our observatory, the more so as three years had been named in the first instance as the duration of the research. A period so limited as to admit of the investigation of those relations only which most immediately presented themselves, may appear at first sight but little corresponding with the grandeur of the scheme in other respects; but we must remember that the progress of the accurate investigation of any natural force is certain to lead to new methods and new relations. The history of astronomy teaches us that whilst every exact *result* belonging to the foundations of science is sure to find a future place commensurate with its worth, it is not so with the *arrangements*: these are subject to the constant transformation which the progress of science no less than the changing occurrences of time bring with them.

The first magnetic observations in Munich were begun in the middle of 1836, in a room of the Astronomical Observatory. They were made daily at 8^h A.M. and 1^h P.M. mean Göttingen time. The influence of masses of iron rendered it impossible to determine absolute values, which would have required a locality free from iron, as well as an accurate determination of the value of the scale divisions, &c. Means were wanting for this purpose, and in July 1837 the observations were discontinued.

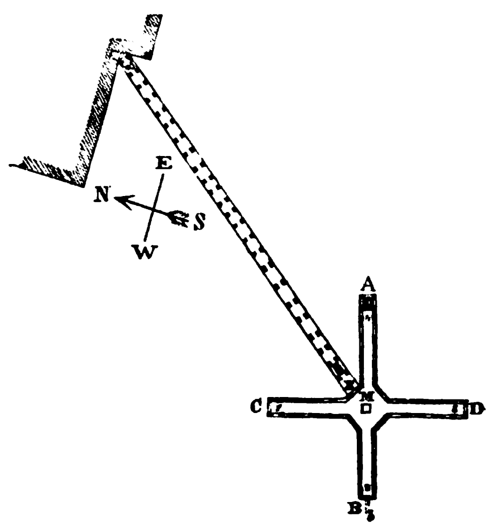
In June 1839 I received the Circular of the Royal Society, announcing that the British Government and East India Directors, on the recommendation of British men of science, had undertaken to found magnetic establishments beyond the limits of Europe, and inviting cooperation for the attainment of a common object. Two months later, M. Kupffer, whom the Russian Government had charged with the establishment and superintendence of the magnetic observatories in the Russian empire, arrived in Munich for the purpose of advocating the desired cooperation. He had visited most of the principal cities of Germany without finding anywhere preparations for the more extended system of observation proposed, or without being able to gain a reasonable hope that cooperation such as England and Russia desired would be attainable. Besides general considerations, this latter circumstance could not but have great weight in inducing the establishment of a magnetic observatory in Munich. After many conversations with M. Kupffer and with M. von Schelling, whom he had interested in the subject, I determined to present to His Majesty a memorial, showing the advantages to science of our country furnishing a link in the chain of research which was to extend over all parts of the globe, and the simple arrangements which would be sufficient for the purpose. It was graciously accepted, and in January 1840 I received the order to commence the building.

His Royal Highness the Crown Prince Maximilian of Bavaria, who has during the present year instituted at his own expense inquiries relating to several branches of physical science, showing his princely liberality, and evincing in the choice of the subjects of investigation a knowledge of the progress of science which can only have been attained by deep study, testified his interest in the magnetic researches, by a spontaneous announcement of his intention of defraying from his own private funds the cost of an additional assistant during the continuance of these observations, and has moreover assigned a further sum for the purpose of rendering the arrangements more complete in other respects.

The preparations for the building of the Magnetic Observatory were made in the month of February, and as soon as the woodwork was nearly ready the excavation was begun. The clay, which extends over the whole district, was met with at the depth of 9 feet, and beneath the clay is gravel. Water does not begin to appear until a depth of 60 feet is reached.

The accompanying ground-plan will best explain the arrangement of the building.

A B C D is the magnetic observatory, which has the form of a cross; A B is in the direction of the magnetic meridian, and C D perpendicular to it. The depth beneath the surface is 13 feet. The building has a double wall of wood, the outermost of which supports the earth; in the intervening space (about 8 inches) is



the wooden framework of the building. A constant current of air between the two walls can be maintained, and regulated so as to prevent damp; and in fact none of the oppressive sensation of damp often experienced in under-ground buildings is sensible. At 1 2 3 4 are wooden pillars, destined to support the magnetic instruments; these pillars were saturated with linseed oil, and are as yet in perfectly good preservation. M is the insulated pillar which supports the theodolite. At the eastern extremity A, a perpendicular shaft has been sunk to 30 feet below the surface of the ground. A ladder leads to the bottom, and long thermometers are placed in the east side of the shaft in such manner that the bulbs are imbedded 2 feet deep in the earth, the projecting part of the tube being turned up so that the height of mercury is read off on the vertical portion of the tube. These thermometers are placed at every 4 feet from the top to the bottom of the shaft, with a view to the investigation of the propagation of the sun's heat through the soil. The subterranean passage E F communicates by steps with the library of the astronomical observatory. At the western extremity there is a quadrangular opening *b* at the same height as the theodolite at M, which is prolonged under ground in a straight line until it issues on the south-western declivity of the rising ground on which the observatory is built; through this opening the theodolite views a distant mark on the church of St. Anne, in a suburb of Munich, the azimuth of which has been accurately determined. The passage E F is covered with earth, but the observatory itself is not; the roofs of the side buildings rise about 2 feet above the ground; that of the middle is still higher, and has glazed openings above M, by which daylight is admitted into the observatory.

The exact instruments introduced by M. Gauss, so far surpassing the accuracy before attained, appeared to open completely anew the question of the manner in which the investigations of terrestrial magnetism ought to be conducted. There are two classes of phænomena for the study of which observations of measurement are employed, viz. such as being called forth by *one* or by a *small number* of causes only, may be followed and explained through *all* their variations;—and such as have besides their *principal cause*, an infinite number of subordinate *accidental* causes, so that it is only in their *mean values* that their subjection to laws can be traced out, whilst an explanation of the *whole course* of the phænomenon becomes impossible. Astronomy belongs to the first of these classes; in it everything is pursued into the minutest detail, and the correctness of theory is demonstrated by such full development; it requires therefore the utmost exactness in the means of observation, so much so that it is only as the accuracy of these increases that we can make progress in the science. To the second of these classes belong all meteorological phænomena—temperature, moisture and pressure of the atmosphere, direction and strength of winds, and other similar subjects. In these the problem which presents itself differs essentially from what is the case in astronomy. We have not to explain the *observed* march of phænomena, but to separate from that march all that is accidental, to draw forth that which ranges itself under laws, and to connect it with its efficient cause. Here we do not require the extreme of accuracy in final results, still less in single observations; but rather there is a limit of accuracy dependent on the amount of the effect of accidental influences, beyond which nothing is gained for the advancement of the object in view. Under which of these two classes should the investigation of terrestrial magnetism be ranged?

This question might at first have well appeared doubtful; but the facts which we have now before us, by our most recent researches, have brought us nearer to its decision. I will now indicate some of these facts, and show their application.

After observing the march of the magnetic instruments for some time, it appears clearly impossible to reduce such irregular and fluctuating movements under one *law*, in the sense in which astronomical phænomena are so reduced.

[Dr. Lamont then gives a table containing the results of a few days' observations, showing their differences from an assumed mean value, and proceeds to comment on them as follows] :—

We see in these observations a manifest movement dependent on the hour of the day; and by sufficiently long continued duration a movement having the year for its period may be similarly traced, having deviations of the same order as those which take place in reference to the diurnal movement. It cannot be doubted that these regular periodical movements have their cause in some influence of the sun; but whether the deviations are also to be ascribed to a similar cause (*i. e.* to irregular solar influence), is a question on which there may be different opinions.

If the atmospheric conditions which modify the influence of the sun's rays on the earth's surface, such as direction of wind, clouds, &c., have also a decided influence on the magnetic deviations of ordinary occurrence,—and if the extraordinary disturbances are in many respects in exact connexion with the times of the day and of the year, (as appears to result from the observations of this observatory,) it seems to me that we have no remaining reason for seeking other *sources* of magnetic variations than the sun*. Assuming this view to be correct, there would be no doubt under which of the two above-named classes of phænomena the manifestations of terrestrial magnetism should be ranged.

M. Gauss has recently subjected to exact observation a highly remarkable feature of terrestrial magnetism, *i. e.* the simultaneity of magnetic variations at different points of the earth's surface. Observations have not determined how we should represent to ourselves this simultaneity. It seems most agreeable to natural analogy to view it as resembling the propagation of atmospheric changes which prevail over districts of greater or less extent, advance with a rapidity proportioned to their magnitude (or intensity), and suffer various modifications during their progress; but with this difference, that magnetic variations pass over great spaces in hardly an appreciable interval of time, and suffer smaller modifications in their passage than any other meteorological phænomenon with which we are acquainted. Observations, at the northern extremity of Europe, and at remoter stations, have however now shown that the variations *are* modified, and that to such a degree that at last the similarity entirely disappears. If the supposition already mentioned, that *all* magnetic changes are connected with periods of the day or year, be correct, it is manifest that no strict simul-

* On examining the great number of disturbance observations recorded in our registers, it is seen that when a disturbance occurs during the day the declination needle makes an oscillation towards the west; if the disturbance is during the night, the movement of the needle is towards the north. Our observations, in accordance with determinations elsewhere, show moreover that the more or less frequent occurrence of disturbances and their magnitude, are in connexion with the times of the day and year. If great and irregular movements of the needle take place during auroras, we have no reason to assume the aurora to be the acting force: it may with more probability be regarded merely as an accompanying phænomenon.

taneity could exist between points of the earth's surface widely remote, and in such case the expectations which have been formed of the results of corresponding observations could hardly be fulfilled.

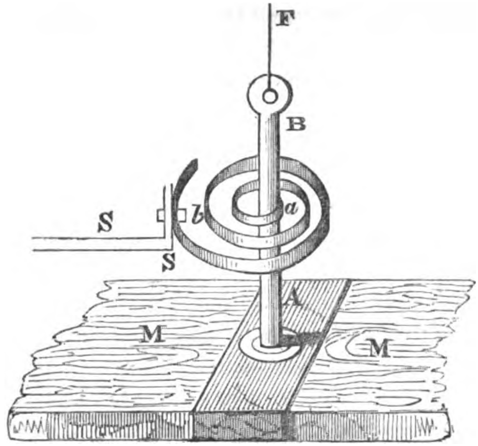
Supposing however that corresponding observations would at all events find a useful application to theory, it may still be questioned whether it would not be more advantageous to choose the times of the more considerable disturbances, instead of term-days as at present in use. Whilst under ordinary circumstances we obtain the total effect of a great number of causes, without being enabled to decide what is due to any *single* cause, disturbance observations give us the immediate result of a single determinate cause, which result we may follow from station to station, and may thus investigate relatively to its origin and extension.

It being now recognised that the magnetic variations are neither perfectly simultaneous nor perfectly similar in their course at different places, the question which presents itself is to discover *where* are the obstacles to the simultaneity and similar course, and whether the qualities of soil or air have any part therein. For this purpose *very exact* simultaneous observations at points near to each other, often repeated and under varying atmospherical circumstances, may be advantageous, and with this view observations were made simultaneously at Munich and on the Hohenpeissenberg on the term-day of September 1841. I hope soon to accomplish many similar experiments.

Without entering further into the results of the observations contained in our registers, or the more immediate consequences which may be derived from them, I proceed to notice, in the briefest possible manner, experiments made with reference to the magnetic instruments, and to give the conclusions to which they have led.

When the magnetic observatory was first established I followed the principles laid down by M. Gauss, and chose magnetic bars of 25 lbs. weight. The declination bar was suspended in the ordinary manner; but as the mode of suspension of the bifilar did not appear to me to offer sufficient security, I employed as a torsion force, instead of two threads*, a steel spiral spring (an English chronometer spring) in the following manner:

* Mr. Christie employed for the examination of the changes of intensity a needle which was kept nearly perpendicular to the magnetic meridian by means of two strong magnets. The use of two parallel threads for the measurement of torsion was first introduced, as far as I know, by Mr. Snow Harris, and is de-



The magnet M M with a strong wire A B affixed was suspended by the thread F; one end of the spiral spring was attached to the strong wire A B at *a*, the other end was made fast at *b* to S S, which projected from an insulated supporting pillar. The tension of the spring was such as to keep the bar 90° from the magnetic meridian. It will easily be conceived that by means of an attached mirror any alteration in the position of S S might be recognized, so that either the torsion force of the spring would be constant, or a suitable correction could be applied. I will only notice further that I determined the effect of temperature repeatedly and with great exactness; and as the temperature of the bar underwent very little change, there have been few instruments employed, the results of which are deserving of equal confidence.

In using the instruments several circumstances were speedily noticed, which appeared to require improved arrangements.

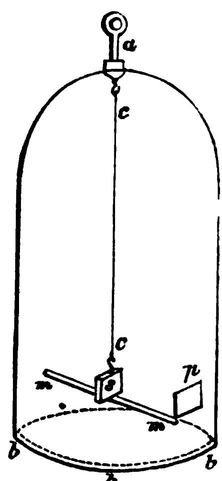
a. The determination of torsion in the declination instrument is a very tedious operation, and at the same time one which the constantly occurring changes require to be performed very frequently.

b. There is no check on the amount of the loss of magnetic force in the bar of the intensity magnetometer.

scribed in the 'Reports of the British Association.' [Also in Phil. Trans. 1836. art. xx.—Ed.] Ingenious as is this arrangement, it can hardly afford in practice all the advantages which it promises in theory. Silk threads are hygrometric, and metal threads, according to my experiments, have the disadvantage, when they extend or contract (from temperature or other causes), of at the same time becoming twisted.

c. The measurement of absolute intensity requires much labour and great precision, and the results are at last uncertain to nearly the one-hundredth part of the amount.

I pass by several arrangements by which I tried to remedy these defects, as in the course of a different inquiry I soon arrived at results which made it unnecessary to proceed with them. Being aware that different observatories used bars of very different magnitudes, and that whilst very different opinions prevailed as to the necessity of using large bars, no experiments had been made to determine whether any and what differences were occasioned by difference of size, this point appeared to me to require further elucidation. I therefore procured from Göttingen four bars of 25 lbs., two of 10 lbs., and two of 4 lbs., being otherwise provided with smaller bars. I first suspended a 25 lb. bar in the magnetic observatory, and a 4 lb. one in the astronomical observatory, and had them observed simultaneously. The result was that the two instruments showed a march nearly *parallel* for a short time; but on continuing the observations considerable *differences* always presented themselves. This remarkable result appeared to show still more strongly the necessity of continuing the investigation. I next constructed a small declinatorium (figured below), with a magnet weighing only 1 gramme, intending to compare its results with those of the 25 lb. bar, in the expectation that the great difference of size between the two instruments would render the difference of their march much more striking.



m m is a small magnet bearing in the middle the mirror *s*, and itself suspended by a silk fibre *c c*, placed under a bell-glass perforated at the top and suspended by a brass wire *a*. A piece of the bell-glass was cut out at *p*, and a plane glass, through which the mirror could be viewed, was inserted instead. The bell-glass was closed below by a round plate *b b b*, and made air-tight. The bottom plate was first made of copper, on account of its quieting effect on the magnet, a purpose which appeared fully answered, as the instrument was quite free from oscillation. Subsequent experiments induced me to take away the copper plate and to substitute one of glass. After this change the instrument continued *as perfectly free from oscillation as before, so much so that in observation each reading gave the position, and it became unnecessary to eliminate the oscillations by repeated readings.*

One condition was still required for exact reading with this instrument. The small size of the mirror did not permit the use of the ordinary scales (on white lacquered paper) with fine divisions. I prepared scales of glass, which instead of *reflecting* the light received *from the front* by the illuminating mirror, *transmit* the light of a mirror placed behind. Besides simplicity of construction this mode of illumination has the advantage of affording much more light, and rendering it possible to apply to small telescopes higher magnifying powers than they would otherwise bear. A telescope of 7 lines aperture suffices for reading a scale of which the divisions are equivalent to only 10″. I used scales in which the single divisions had an angular value of 30″.

I had thus a new declination instrument fitted for the most delicate researches. I soon noticed the frequent occurrence of strikingly rapid changes, consisting in a repeated progression and retrogression in very short intervals, which however were at least four times as great as the time of vibration of the needle itself. I had therefore a second instrument made of the same size, with the view of satisfying myself whether these movements were really produced by the magnetic force or by extraneous influences (such as oscillations of the air, for example). The result was, that the two instruments agreed *exactly* in these small movements, proving thereby not only that small needles are fit for the investigation of the magnetic force, but also that they possess a double advantage over larger bars, first in being free from vibration, and secondly in showing variations which cannot be shown by a bar having a considerable moment of inertia.

I did not however consider that I had yet done enough to justify the use of small magnets, particularly as the examination of the relations of the force in question to surrounding substances and localities has been by no means thoroughly exhausted. There is but one satisfactory proof in regard to magnetic instruments, namely, that different instruments should give, under all circumstances, accordant and therefore strictly comparable results.

This proof, the *demonstratio ad oculum,* cannot be replaced by any theoretic considerations, and when once obtained, it renders all further investigations on the subject superfluous.

After several preliminary trials, on the 13th of May corresponding observations were begun with two similar instruments, one of which was placed in the magnetic observatory and the other in my dwelling-room. In comparisons lasting only a few minutes the result was perfectly satisfactory; but when the observations were continued, instead of the expected agreement notable discordances were found.

[Observations are here detailed in illustration of these discordances.]

In order to investigate further the course of these remarkable differences, I placed in the magnetic observatory first two and then three instruments, which were observed simultaneously and gave the following series.

[The table of observations is omitted. Dr. Lamont proceeds to make the following comments on them]:—

My attention was early drawn to the circumstance that the differences presented a diurnal period, evidently connected with temperature. After different experiments on the possible effects of temperature, I came to facts which established the following principle:—Every alteration of temperature impresses on an inclosed mass of air a circulating movement, which once produced, continues for some time, though always diminishing, and keeps a freely-suspended magnet constantly deflected to one side of its true direction.

This effect may be most easily shown by sprinkling a few drops of spirits of wine on the bell-glass of such an instrument as I have described. At the part where the spirits of wine have fallen and are evaporated, cold is produced inside the glass, the particles of air in contact fall down, those above follow them, and in the course of a few minutes a current of air is established, capable of deflecting the needle at first as much as 5′, then becoming gradually more feeble, but having still a perceptible influence at the end of three-quarters of an hour.

After I had satisfied myself by repeated and varied experiments that the effect was produced by the current of air and by it alone, the application to the observed discordances in the different instruments was easy. By the daily rise and fall of temperature (which though much less in the magnetic observatory than in the open air, still forms a sensible daily period), currents must have been produced in the air inclosed with the magnets in the bell-glasses, differing according to the times of the day and the position of the bell-glasses relatively to external changes.

In order to remove this cause I fastened strips of glass under the bell-glasses so as to protect the magnets from the influence of currents of air, and observed anew.

[Here follows a table of observations.]

Even this insufficient protection diminished the influence of the currents of air; the experiments next detailed show the success of a more careful protection in restoring the accordance of the different instruments, and the degree of exactness to which the small movements of two magnets so protected coincide.

JUNE 26.—*Comparison of Apparatus* I. *and* II.

Time.			App. II.	App. II−I.	Time.			App. II.	App. II−I.	Time.			App. II.	App. II−I.
h	m	s			h	m	s			h	m	s		
7	36	0	31·0	−0·1	9	4	15	32·75	+0·05	10	4	30	23·2	0·0
		15	31·0	−0·2			30	32·75	−0·05			45	23·2	0·0
		30	30·95	−0·15			45	32·4	0·0		5	0	23·25	−0·05
		45	30·95	−0·15		5	0	32·5	−0·05			15	23·1	+0·05
	37	0	30·8	−0·1			15	32·8	0·0			30	23·25	+0·05
		15	30·7	−0·05			30	32·9	0·0			45	23·35	+0·05
		30	30·5	0·0			45	32·7	0·0		6	0	23·35	+0·1
		45	30·45	+0·05		6	0	32·4	−0·1			15	23·35	−0·5
	38	0	30·4	−0·05			15	32·3	−0·1			30	23·35	−0·5
		15	30·35	0·0			30	32·0	+0·05			45	23·1	−0·5
		30	30·35	0·0			45	31·9	0·0		7	0	23·45	+0·1
		45	30·4	0·0		7	0	31·7	0·0			15	23·4	0·0
	39	0	30·4	0·0			15	31·4	−0·15			30	23·4	+0·05
		15	30·5	0·0			30	31·2	−0·1			45	23·5	+0·1
		30	30·6	−0·1			45	31·3	−0·1		8	0	23·4	+0·05
		45	30·6	−0·05		8	0	31·0	0·0			15	23·4	−0·1
	40	0	30·7	−0·1			15	30·5	+0·1			30	23·4	−0·1
		15	30·5	0·0			30	30·4	0·0			45	23·35	−0·05
		30	30·45	0·0			45	30·5	0·0		9	0	23·3	0·0
		45	30·45	0·0		9	0	30·5	+0·05	11	3	0	16·4	+0·25
	41	0	30·45	0·0			15	30·4	−0·1			15	16·45	+0·2
		15	30·45	−0·05			30	30·55	−0·05			30	16·35	+0·15
		30	30·45	−0·05			45	30·45	−0·05			45	16·4	+0·2
		45	30·65	−0·05		10	0	30·45	+0·05		4	0	16·35	+0·2
	42	0	30·8	−0·1	10	3	0	23·3	−0·1			15	16·35	+0·25
		15	31·0	−0·2			15	23·0	+0·1			30	16·3	+0·2
		30	31·4	−0·1			30	23·1	0·0	12	56	0	11·65	+0·35
		45	31·6	−0·05			45	23·05	+0·05			15	11·7	+0·3
	43	0	31·8	−0·1		4	0	23·0	0·0			30	11·7	+0·3
9	4	0	32·7	0·0			15	23·25	−0·05			45	11·65	+0·35

Time.	App. II.	App. II–I.	Time.	App. II.	App. II–I.	Time.	App. II.	App. II–I.
h m s			h m s			h m s		
12 57 0	11·65	+0·3	5 14 30	18·45	+0·45	9 11 30	21·0	+0·45
15	11·65	+0·3	45	18·4	+0·5	45	20·95	+0·45
30	11·6	+0·3	15 0	18·4	+0·5	12 0	21·0	+0·4
45	11·6	+0·3	15	18·4	+0·45	15	21·0	+0·4
58 0	11·5	+0·4	30	18·45	+0·45	30	21·0	+0·4
2 57 0	10·7	+0·35	7 27 0	20·7	0·0	9 16 0	20·7	+0·4
15	10·6	+0·4		20·7	0·05	15	20·7	+0·4
30	10·6	+0·4		20·8	−0·1	30	20·6	+0·5
45	10·5	+0·45	0	20·8	−0·1	45	29·65	+0·45
58 0	10·5	+0·45	15	20·85	−0·05	17 0	20·7	+0·4
15	10·5	+0·45	30	20·9	0·05	15	20·65	+0·45
30	10·6	+0·4	45	20·9	0·0	30	20·6	+0·45
45	10·7	+0·35	28 0	20·85	+0·05	45	20·55	+0·5
59 0	10·65	+0·4	15	20·9	0·0	18 0	20·55	+0·45
4 4 0	14·5	+0·3	9 3 0	21·3	+0·4	9 23 0	20·15	+0·65
15	14·4	+0·3	15	21·3	+0·5	15	20·25	+0·55
30	14·4	+0·3	30			30	20·2	+0·6
45	14·4	+0·3	45	21·3	+0·4	45	20·15	+0·6
5 0	14·45	+0·3	4 0	21·4	+0·4	24 0	20·1	+0·7
15	14·45	+0·3	15	21·3	+0·4	15	20·1	+0·7
30	14·5	+0·2	30	21·3	+0·45	30	20·1	+0·6
45	14·4	+0·3	45	21·35	+0·55	45	20·2	+0·65
6 0	14·4	+0·25	9 5 0	21·2	+0·4	25 0	20·5	+0·7
5 14 0	18·45	+0·45	9 11 0	21·0	+0·4			
15	18·4	+0·5	15	21·0	+0·45			

The investigation concerning the use of small magnets now appeared complete. We obtain from it two new principles respecting the construction of magnetic instruments:—

1. The influence of *currents of air* produced by changes of temperature must be guarded against by carefully protecting the magnets*.

2. The use of *small* magnets is not only advantageous, but even *necessary* in exact magnetic determinations, partly because they can be more easily protected from the influence of currents of air; partly because larger bars, by reason of their considerable moment of inertia, cannot show magnetic variations which take place in very quick succession.

It might have been desirable to exhibit the influence of currents of air on large bars, as has been done above for small magnets; it may be easily conceived that under similar circumstances a current of air will be produced in the box of the large magnetometers as well as under the bell-glasses of the small

* This object would be completely attained by exhausting the air under the bell-glass. There would be no particular difficulty in doing this, but I have not myself tried the experiment, as I hardly expect from it any important practical advantage.

magnets, and that its influence will be so much the greater, as large bars have not so considerable a magnetic moment in proportion to their size as the smaller bars. I have, however, made but few experiments on this part of the subject, because I deemed that when the fitness of small magnets shall once be recognized, the employment of large bars will be less frequent, on account of their great inconvenience, even if it be possible to protect them perfectly from the influence of currents of air.

The experiments hitherto narrated all relate to the declination instrument. After I had satisfied myself of the applicability of small magnets for exact observations, and of the advantages which their employment presents, I constructed a *system* of magnetic instruments, furnishing the means of making all measurements belonging to the horizontal part of the earth's magnetism. These instruments are,

1. A differential apparatus for the declination.
2. A differential apparatus for horizontal intensity.
3. An apparatus for the absolute declination.
4. An apparatus for absolute horizontal intensity.

The differential instruments are designed to give the daily and yearly variations: they may be set up in an ordinary dwelling-house as well as in a magnetic observatory, avoiding only moveable masses of iron; and when once established they do not require further examination or check, as the construction is such that small alterations have no influence on the direction of the magnet. The only checks required are for the scale and the telescope.

The absolute instruments, when used from time to time for absolute determinations, may be put up either in the open air or in an observatory, or in a magnetic pavilion made of wood.

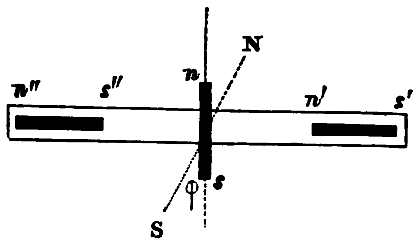
Without giving in this place a description of the instruments themselves, I will notice briefly their essential conditions, and will show, by extracts from our observation journals, the degree of accuracy attainable with them under ordinary circumstances.

1. The *differential apparatus for declination* is the small needle figured in page 508, furnished with a mirror, and inclosed, not in a bell-glass, but in a case partly of glass partly of metal, with an arrangement by which the needle is protected from the influence of currents of air. The telescope has only eight lines aperture; the scale is of glass: the agreement between different instruments of this kind is shown in pages 511, 512.

2. The *differential apparatus for horizontal intensity* differs very much from the modes of construction hitherto employed.

Experiments have convinced me that a magnet can never be placed in a position *perpendicular* to the meridian, either by fixed magnets, or by the force of torsion, (whether by a steel spring or by the bifilar suspension,) without the necessity of a careful check being kept by frequent examination; and that even with great care some uncertainty will always remain.

For this reason I have preferred a constant deflection made in the following manner:—



n s is a magnet freely suspended by a silk fibre about 3 inches in length, bearing in the middle a mirror, and inclosed in a suitable case; $n' s'$ and $n'' s''$ are two deflecting magnets attached to a brass bar; the magnets are of equal strength and equidistant from the suspended magnet, and in the same horizontal plane. The deflectors are perpendicular to the suspended magnet *n s*, and cause its direction to keep the angle ϕ with the magnetic meridian N S. If μ be the moment wherewith the deflectors strive to turn the suspended magnet, we have

$$\mu = \mathrm{X} \sin \phi,$$

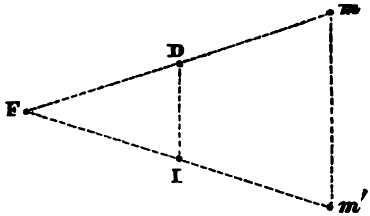
where X denotes the horizontal intensity of the earth's magnetic force. Let the change of the horizontal intensity be $\Delta \mathrm{X}$, the change of the declination (i. e. of the line N S) be $\Delta \delta$, the change of the suspended magnet *n s*, Δi (so that the change of the angle $\phi = \Delta i - \Delta \delta$), the change of μ for 1° increase of temperature $= -\frac{d\mu}{dt}$ and the temperature $= t$, we have

$$\frac{\Delta \mathrm{X}}{\mathrm{X}} = -(\Delta i - \Delta \delta) \cot. \phi \left(\frac{d\mu}{dt}\right) t.$$

If we make $\phi = -45°$, then

$$\frac{\Delta \mathrm{X}}{\mathrm{X}} = -(\Delta i - \Delta \delta) - \left(\frac{d\mu}{dt}\right) t.$$

In order to obtain the change of intensity simultaneous (or nearly simultaneous) readings of the two differential instruments are required; for this purpose their relative position may be most conveniently arranged as follows :—



The telescope and scale are at F. The telescope has a vertical movement, so that it may be directed to the two differential instruments which are placed perpendicularly one above another; that for the declination at D, that for the intensity at I. *m* and *m'* are the marks of reference for the telescope. The influence of the instruments D and I upon each other, though not considerable, must be taken into the calculation. The two instruments can thus be observed with only two or three seconds' interval. In ordinary cases such readings may be regarded as simultaneous; where greater precision is required, D, I, and D, may be read successively at equal intervals, and the mean of the two readings of D will be simultaneous with the reading of I.

I propose to take a future opportunity of describing a construction and arrangement better adapted for dwelling-houses in particular.

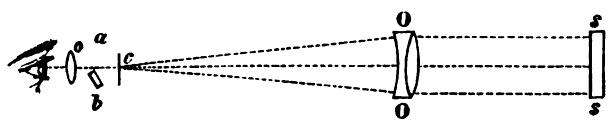
A careful consideration of the above construction will show that, in consequence of the symmetrical position of the deflecting magnets, and their perpendicular direction relatively to the suspended magnet, no change which can occur in the adjustment of the instrument (supposing it not to be very considerable) will produce error in the reading: a check therefore is unnecessary.

The formula shows that the readings of this instrument require a correction on account of temperature, partly because the magnetism of the deflectors is affected by temperature, and partly because their distance from the suspended magnet is altered by the expansion of the brass bar.

It appears to have been latterly supposed that the influence of temperature on magnetized steel bars could not be brought under calculation. However, in constructing my new magnetic apparatus, I have made a series of experiments which agree with previous experiments of M. Kupffer in showing, that in all temperatures which are met with in the open air, and even in some considerably higher, the change in the magnetism of the bar is *perfectly proportioned* to the change of temperature, and is independent of the strength of the bar's magnetism. We also found, that when a magnetic bar has lost a certain portion of its force it attains a steady condition, fitting it for observations on terrestrial magnetism. Magnets differ widely in their sensibility to heat, and whereas a magnet will lose under ordinary circumstances 0·0012, and in more favourable cases 0·0006 of its magnetism for an increase of temperature of 1° Reaumur, I have succeeded in producing magnets in which the loss is only 0·00014. However, I do not regard these experiments as concluded, the more so as I find the mention, in the magnetic works of Col. Sabine, of a small bar used in experiments of vibration which underwent no sensible alteration either in increasing or decreasing temperatures. Without expecting to be able to construct magnets which shall be perfectly free from any effect of temperature, I may at least be justified in considering that the extreme attainable limit has not yet been reached.

3. The *apparatus for absolute declination* consists in essentials of a small magnet furnished with a mirror, which may be inverted in the usual manner for eliminating the error of collimation: I will particularize one circumstance only.

In determining the declination it is necessary to make the optical axis of the telescope of the theodolite perpendicular to the plane of the mirror of the magnet. For this purpose I give to the theodolite telescope the following means of adjustment:—



o is the eye-piece; its tube is pierced above at *a*; a mirror *b* at an inclination of 45° to the optical axis throws the light received from above towards the object-glass, and illuminates the wire at *c*. From *c* the rays, forming a cone, proceed to the object-glass OO, are rendered parallel by the object-glass, and arrive at the mirror of the magnet *s s*. Being reflected back upon the object-glass (still parallel), they produce at the focus of the telescope an image of the wire. The telescope is turned until the wire *c* coincides with its image, when the optical axis of the telescope will be perpendicular to the mirror.

The following observations, made in the magnetic observatory, show the degree of accuracy of which this instrument is susceptible. The first column in the table gives the approximate time; the second column contains the readings of the differential apparatus, taken at the moment of adjusting the circle; and the third column gives the readings of the circle. The zero point of the circle did not coincide with the meridian, but was to the westward of that line, and formed with it an angle noted in the fourth column. The last column shows the absolute declination, corresponding to the scale division 40 of the differential apparatus, assuming the collimation in the absolute apparatus I. to be 18′ 27″·6, and in apparatus II. to be 2′ 53″·4. The scale divisions of the differential apparatus have a value of 30″.

	Time.	Diff. Inst.	Circle.	Zero point of Circle.	Decl. for Scale division 40.	Apparatus.
	h		° ′	° ′	° ′	
1841, Sept. 7.	8	34·9	1 43·08	14 47·92	16 52·13	I.
		35·05	1 43·38		14	...
	9	37·3	2 21·23		2	...
	10	42·6	2 23·93		5	...
		43·3	1 47·28		1	...
		45·7	1 48·57		6	...
	11	47·4	2 26·33		5	...
	12	50·9	2 28·21		13	...
		51·6	2 28·63		17	...
		53·8	1 52·82		18	...
	1	52·1	1 51·95		17	...
		51·4	2 28·65		24	...
	2	49·35	2 27·65		26	...
		48·55	1 50·27		23	...
	3	47·4	1 50·52	14 47·04	16 52·19	...
		47·0	1 50·20		12	...
	5	43·0	2 9·80		27	II.
		41·7	2 3·37		27	...
		41·2	2 3·08		25	...
		40·4	2 8·33		17	...
		39·0	1 46·05		3	I.
		38·7	1 45·82		16 51·58	...
		37·7	2 22·37		16 52· 6	...
		38·0	2 22·48		4	...
Sept. 8.	5	35·1	2 21·32		21	...
		33·65	2 20·33		5	...
	6	33·70	1 43·70		21	...
		33·7	1 43·73		23	...
	8	34·4	2 4·30	14 48·00	16 52·13	II.
		34·2	1 58·27		4	...

[Mr. Lamont then details similar observations made "with a 25 lb. bar suspended as usual, and furnished after the manner of the English instruments with an object-glass, in the focus of which is a scale cut on glass, divided to minutes." These observations are given for the purpose of showing the superior accuracy of those with the smaller apparatus, which moreover were obtained with much less labour.]

4. The *apparatus for absolute intensity* is the one which

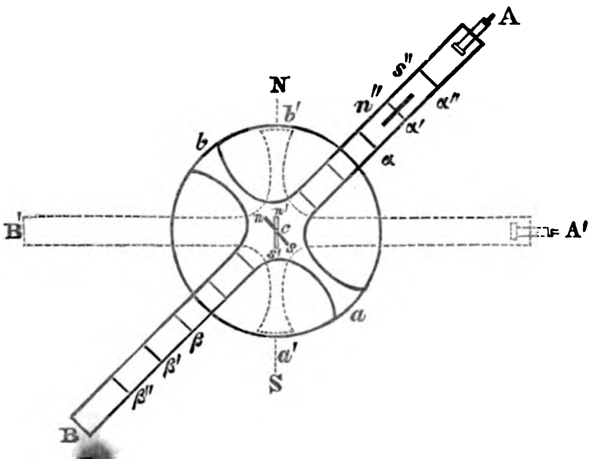
differs most from those in previous use. We owe to Poisson the highly ingenious idea of employing two equations, one of which shall contain the product, and the other the quotient, of the magnetism of the earth and of that of the bar, and by this means of entirely eliminating the magnetism of the bar in the calculation, and reducing the magnetism of the earth to absolute measure. Poisson proposed to obtain the requisite data for the two equations by means of experiments of vibration. Christie showed, on the other hand, that whilst the product might be obtained with great ease by vibrations, the quotient was more conveniently furnished by means of deflections.

M. Gauss, in his treatise entitled *Intensitas vis magneticæ ad mensuram absolutam revocata*, has shown how to obtain a more accurate determination of these two equations, avoiding all constant errors. We find in this memoir measurements which show the degree of accuracy of which Gauss's method is susceptible; I subjoin these results, and also the determinations made by M. Kreil at Prague in 1840, with instruments precisely similar.

Göttingen.		Prague.	
1832. May 21	1·7820	1840. Aug. 21	1·81414
... 24	1·7694	... 22	1·98218
June 4	1·7713	... 24	1·93396
... 24, 28	1·7625	... 27	1·84211
July 23, 24	1·7826	... 28	1·90970
... 25, 26	1·7845	... 30	1·93483
Sept. 9	1·7764	Sept. 3	1·92898
... 18	I·7821		
... 27	1·7965		
Oct. 15	1·7860		

A mere inspection of these numbers leads to the inference that the means hitherto employed for the measurement of the intensity still leave much to be desired. The three first places of figures ought to agree (the effect of intervening changes of the earth's intensity, which has been neglected in the calculation, may show itself in the fourth place); but differences are found always in the third and sometimes in the second place. The principal reasons probably are,—1st, that only small angles of deflection were used; and, 2nd, that in the observation of vibrations the cases were not always sufficiently closed against currents of air. The experiments already narrated in this memoir have manifested that two such magnets never have a perfectly parallel march, rendering it impossible to measure small angles of deflection with *precision.* If, still adhering to M. Gauss's method, large angles are taken, other circumstances equally disadvantageous are originated.

I have succeeded in finding a simple method by which all the previous difficulties appear to me to be avoided. I will endeavour, in a brief description, to make the material points relating to it intelligible to those who have occupied themselves with magnetic determinations. I reserve for a future opportunity an exact development both of the theory and of the practical method, with reference to some circumstances which are here passed over for the sake of brevity*.



On the circle a' a, b' b, which is fixed, is a moveable brass piece A B, having two verniers, a and b; a frame containing the freely suspended magnet $n\ s$ is placed on and attached to the middle of the brass piece; the magnet bears a mirror parallel to its magnetic axis, so that when the magnet is in the position $n\ s$ in the magnetic meridian, the optical axis of the telescope A' is perpendicular to the plane of the mirror, and hence the image of the wire in the eye-piece coincides with the wire itself. If a small magnet n'' s'' is placed on the brass support, it exercises a certain moment in turning the suspended magnet, and the brass support must be moved into the position A B, when the optical

* This memoir has been since published, entitled *Bestimmung der Horizontal Intensitaet des Erdmagnetismus nach absolutem Maase.* The dimensions of the deflection-bar, recommended by Dr. Lamont in this memoir, are 36^{mm} long, 6^{mm} broad, and 1^{mm} thick; or in British inches 1·42 long, 0·24 broad, and 0·04 thick, nearly. The moveable brass piece on which the deflection-bar is laid should have a length of half a metre, or 19·7 British inches.

axis of the telescope is again perpendicular to the plane of the mirror; then $a' \, c \, a$ is the angle of deflection, which we will call ϕ. If we call the distance $c\,\alpha' = e$, the magnetic moment of the deflecting magnet = M, and the magnetism of the earth = X, theory gives the following equation,

$$\frac{\mathrm{M}}{\mathrm{X}} = \sin \phi \, f(e),$$

where $f(e)$ denotes a function of the distance. It need scarcely be remarked that ϕ must be measured four times, in such manner that the middle of the deflector must be brought on two corresponding divisions α and β, α' and β', and so on, first with the north pole, and then with the south pole, turned towards the suspended magnet. The mean of the four angles measured then corresponds to the distance $\frac{1}{2}\,\alpha\,\beta$, $\frac{1}{2}\,\alpha'\,\beta'$, &c., in the above formula.

It should further be noticed, in reference to the function $f(e)$, that it depends also on the magnetic moment of the deflector; and if this suffer any alteration, either by gradual loss, or by change of temperature, such alteration must be taken into account.

$f(e)$ must now be determined, for which purpose deflections must be taken at the different distances $\alpha\,\alpha'\,\alpha''$, $\beta\,\beta'\,\beta''$.

Theory gives the following as the most convenient form of $f(e)$:

$$\log f(e) = \log \frac{1}{2}\, e^3 + \frac{p}{e^2} + \frac{q}{e^4};$$

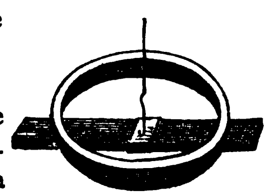
where p and q must be determined by equations of the form

$$\log \frac{\mathrm{M}}{\mathrm{X}} = \log \sin \phi + \log \frac{1}{2}\, e^3 + \frac{p}{e^2} + \frac{q}{e^4},$$

$$\log \frac{\mathrm{M}}{\mathrm{X}} = \log \sin \phi' + \log \frac{1}{2}\, e'^3 + \frac{p}{e'^2} + \frac{q}{e'^4}.$$

The value of $f(e)$ being ascertained, $\frac{\mathrm{M}}{\mathrm{X}}$ is determined. In order then to obtain M X, the deflecting magnet is suspended by a silk thread under a bell glass, and its time of vibration determined: calling this time T, we have

$$\mathrm{M\,X} = \frac{\pi^2\,\mathrm{K}}{\mathrm{T}^2},$$



where K is the moment of inertia of the magnet including the suspension apparatus. For determining K, I employ a very regularly turned glass or metal ring, the moment of which, even if the mass should not be homogeneous, may be determined with all requisite exactness by a method which I will give at a future opportunity. Having obtained the moment of inertia in the manner referred to, the ring is placed on the magnet, and the time of vibration determined afresh with the moment of inertia thus augmented. We have then the equation

$$M X = \frac{\pi^2 (K + R)}{T'^2},$$

where R denotes the moment of inertia of the ring, and T′ the time of vibration with the ring. This equation, combined with the preceding, gives the value of K *.

I subjoin some single measurements of each kind taken from our magnetic journal, in order to show the accuracy practically attainable.

Magnet V.

Observed time of passing the wire †.

	A.	B.	C.	B—A.	C—B.
	h m s	h m s	h m s	h m s	h m s
Nov. 12 ...	8 2 25·7	8 16 51·9	8 27 41·2	8 14 26·2	8 10 49.8
	38·0	17 4·3	53·9	26·3	49·6
	51·0	17·9	28 7·9	26·0	49·0
	3 4·0	30·2	19·9	26·2	49·7
	17·9	43·9	34·0	26·0	50·1
	30·9	56·2	45·9	26·2	49·7
	43·9	18 9·9	59·9	26·0	50·0
	56·0	22·2	29 11·8	26·2	49·6
	4 9·9	35·8	25·9	25·9	50·0
	22·0	48·2	37·8	26·2	49·6
Intensity...	40·3	39·9	39·5	40·1	39·7

* [In a notice inserted in the *Annalen für Meteorologie und Erdmagnetismus* for 1842, Dr. Lamont remarks, that "measurements of the absolute intensity always comprehend two operations, *i. e.* of vibration and of deflection; in the experiments of vibration the bar is in the magnetic meridian, but in those of deflection it forms an angle with the magnetic meridian, which is in all cases rather considerable. Up to the present time it has been assumed that the magnetic force of the bar continues the same in these two positions, which will not however be the case if the induction caused by the earth's magnetism be of sensible amount." From experiments which Dr. Lamont has made, he infers—1st, that in all magnets the induction is so considerable, that it must be taken into account in measurements of the absolute intensity; and, 2nd, that more magnetism is induced in large magnets than in small ones, more in thick than in thin magnets, and more in bars which are less hard than in those which are completely hardened.—E. S.]

† When the time of vibration is small the passages of the needle succeed each other too rapidly for all to be observed; the most convenient mode is to observe every third passage, which has been done in these observations.

The mean interval B – A = 14′ 26″·12 = 200 vibrations, or 1 vibration = 4″·3306; the corresponding intensity by the differential instrument being 40·1.

The interval C – B is = 10′ 49″·71 = 150 vibrations, or 1 vibration = 4″·3314 : the corresponding intensity being 39·7.

The arc of vibration was always so small that a reduction to infinitely small arcs is unnecessary. The two determinations being reduced to the same intensity, we have 4″·3306 and 4″·3313.

The deviation of the single determinations from the mean amounts to $\frac{1}{10000}$th of the whole. When the access of air is carefully prevented* greater accuracy is usually attained.

b. Determination of the Moment of Inertia of the Deflecting Magnet.

	Time of vibration without ring.	Intensity.	Temperature.	Time of vibration with ring.	Intensity.	Temperature.	Ring.
	″		°	″		°	
Oct. 25	4·3003	1·0	+6·0	13·2289	5·5	+5·8	I.
Nov. 4	4·3187	38·4	+6·3	13·4139	42·5	+6·1	II.
... 25	4·3402	46·8	+4·2	13·3634	45·5	+4·2	I.
... 26	4·3376	47·4	+2·4	13·3575	46·5	+2·4	I.

In reference to the calculation it must be remarked,

1. When the intensity increases one unit the time of vibration without the ring diminishes 0″·00047.

2. When the temperature increases 1° the time of vibration increases 0″·00167.

3. For ring I. we have the log. of the moment of inertia . . . . = 8·58728

For ring II. . . . . = 8·59595

4. The torsion force of the thread is in the first experiment,

without ring 0·00087

with ring . . 0·00348

In more recent experiments . . . . without ring 0·0007

with ring . . 0·0028

Both in these moments and in the moment of the bar the expansion of the metal by reason of temperature must be taken into account. Calculation gives the log. of the moment of inertia of magnet V. at 13° Reaumur, from single measurements as follows :—

* In several of the determinations subsequently given, this condition has not been sufficiently attended to. I first became convinced of the necessity of completely cutting off the access of the air from without by the results themselves.

7·65790
7·65785
7·65770
7·65767

In a more exact calculation of the vibrations it would be needful to take into account the mass of air which vibrates with the needle.

Taking the mean, the log. of the moment of inertia may be called = 7·65778.

The single determinations form, as may be seen, a decreasing series, the reason of which is doubtless to be ascribed to a change of the force of torsion of the thread. However, the degree of uncertainty in the final result arising from this cause is so small that I have not thought it necessary to attempt to remove it.

c. Determination of the Function f (e).—The measurements made for this purpose are as follows, the several series having been reduced to the same intensity and the same temperature.

Distance, at 13°.	October 27, at 9°.	October 29, at 9°.	November 26, at 2°·7.	November 30, at 4°.
mm	° ′ ″	° ′ ″	° ′ ″	° ′ ″
e = 277·831	ϕ = 37 32 14	37 23 17	36 49 15	36 43 42
e' = 333·387	ϕ' = 20 36 57	20 31 19	20 16 26	20 13 33
e'' = 388·950	ϕ'' = 12 48 44	12 45 26	12 35 47	12 34 2
e''' = 444·513	ϕ''' = 8 31 44	8 30 17	8 23 37	8 22 33

In the series of the 29th of October the last angle seems to contain an erroneous reading; it appears safest to leave this series out of the account for the present. The remaining series, treated according to the method of least squares, give at 13° Reaumur,

Oct. 27, log (e) = 7·02795
Nov. 26 ... = 7·02771
Nov. 30 ... = 7·02818

the mean, with a temperature t, will give

$$\log f(e) = 7{\cdot}02795 + 0{\cdot}00003\,(t - 13^\circ).$$

d. Measurements of Intensity.—If several measurements of intensity are to be made with the same apparatus, the calculation may be facilitated by taking the result for determinate values of ϕ, T, t, and determining the corrections corresponding to the differences occurring in the measurement. Assuming the elements spoken of above $\phi = 36^\circ\ 50'$, $T = 4''{\cdot}33$, and taking into account the force of torsion of the thread by which the magnet was suspended in the experiments of vibration, *i. e.* 0·0007, we obtain

$$\begin{aligned}\text{absolute intensity} = 1{\cdot}9344 & \\ & - 0{\cdot}000045\ (t - 13^\circ) \\ & - 0{\cdot}000382\ (\phi - 36^\circ\ 50') \\ & - 0{\cdot}004545\ (\mathrm{T} - 4''{\cdot}33) \times 100.\end{aligned}$$

In reference to the following measurements, it must be noticed, that on the 23rd of October new deflecting magnets were applied to the differential instrument for horizontal intensity. These deflectors had not attained a steady magnetic condition, and lost a portion of their magnetism, partly equably, partly irregularly (the latter particularly when sudden rains set in); and in consequence it became necessary several times to bring them nearer the suspended magnet, otherwise the scale would have passed out of the field of view of the telescope. The whole loss in magnetic force from the 23rd of October to the 5th of December, amounted to about $\frac{1}{30}$. On the 12th and 22nd of November sudden changes appeared to have taken place. On the 17th of November the magnets were brought nearer, so that the readings became 46 divisions less.

In reference to the calculations, I must further remark, that a division of the differential instrument corresponds to a change of 0·0002 of the intensity; using this value all the intensities are reduced to division 40 of the differential instrument. The measurements were made at different hours of the day.

	Angle of deflection and intensity.			Time of vibration and intensity.		Tempe-rature.	Absolute intensity reduced to intensity 40.
Nov. 11.	37°	0′·5	41·3	4″·3286	41·5	+2°·6	1·9312
	37	0·5	38·4	4·3326	38·5	+5·1	1·9298
12.	36	59·3	38·7	4·3310	39·9	+4·5	1·9309
	36	58·9	36·1	4·3366	37·5	+6·6	1·9283
13.	36	56·3	48·4	4·3354	47·6	+5·3	1·9282
	36	55·9	50·3	4·3354	50·4	+5·3	1·9279
	36	57·2	47·2	4·3354	48·3	+5·6	1·9280
16.	36	57·2	53·6	4·3326	51·8	+2·7	1·9284
17.	36	56·3	60·1	4·3309	59·7	+2·3	1·9280
Differential-instrument altered.							
20.	36	55·2	11·1	4·3423	8·5	+5·4	1·9331
22.	36	49·8	26·4	4·3407	26·7	+6·3	1·9326
23.	36	49·8	33·7	4·3445	32·8	+5·3	1·9294
24.	36	53·2	30·5	4·3448	32·2	+4·0	1·9285
	36	52·5	36·5	4·3424	36·7	+4·1	1·9289
25.	36	49·2	47·9	4·3410	45·0	+3·5	1·9288
	36	47·8	47·6	4·3408	47·0	+4·2	1·9292

Magnetic Observations on the Hohenpeissenberg.

By the desire of the King's government I proceeded, in the middle of September 1841, to the Hohenpeissenberg, for the purpose of restoring, and supplying with magnetic instruments in particular, the observatory founded by the Palatinate Society, and after the dissolution of that Society adopted by our Academy.

The observatory is at the parsonage-house, exactly on the summit of an insulated mountain, which rises in a conical form to a height of about 1000 feet above the plain, and 3400 feet above the level of the sea.

Under the present arrangements the observatory occupies a space of about 30 feet in length and 12 in breadth, and contains the barometer of the Palatinate Society, a new thermometer for the external air, a wet-bulb hygrometer, and apparatus for magnetic declination and intensity of the construction described in the preceding pages. There is also a rain and snow-gauge, and an apparatus for the measurement of the electricity of the atmosphere is to be put up.

I availed myself of my brief residence on the mountain to obtain a series of magnetic observations, consisting of—

a. Observations of variations, including the term-day of the 22nd of September.

b. Determinations of absolute declination and intensity. The results are subjoined with the view of exemplifying the capabilities of the new instruments, and supporting those views of the relations of the earth's magnetic force which have been expressed in the earlier part of this memoir.

[Dr. Lamont then gives a table occupying five quarto pages, containing the observations of variations of the declination and intensity at Munich and at Hohenpeissenberg from the 20th to the 24th of September. All the intensity observations are reduced to a standard temperature by corrections experimentally determined. He remarks on these observations as follows] :—

The comparison of the observations shows, that for the most part the same changes were found to occur at Munich and at Hohenpeissenberg, but that nevertheless the march of the instruments was not parallel; the differences of declination and intensity at the two places were at one period, namely at the earlier portion of the observations, less, and at another period, namely in the latter portion of the observations, greater. This is quite a characteristic circumstance, and appears to indicate that the magnetic variations of two places are not dependent on each other in the same way as if they were produced by the same current propagating itself through the earth. A better explanation of all the phænomena hitherto observed would be afforded by regarding the earth as a sphere, on the surface of which there exist an infinite number of *isolated* particles magnetized by induction only. In such case the dependence of the magnetism of each particle on all the rest would be as if there subsisted a certain equilibrium; and that every change at any one point produces at other points a change, not of equal amount, but of such amount as would be requisite to restore the previous state of equilibrium.

[In proof that a disturbance proceeding from a very distant source does not produce exactly the same movements at two stations but little removed from each other, Dr. Lamont subjoins the observations made at Munich and at Hohenpeissenberg during the remarkable and widely-extended disturbance of the 25th September 1841; he adds, that he does so also in compliance with the invitation contained in the circular letter of Mr. Airy, and expresses a hope that the interesting results to which these comparisons may lead may soon be discussed by that gentleman, noticing in the mean time that no similarity whatsoever can be traced throughout the Bavarian and the Greenwich observations made during the disturbance in question.

In both the Munich and the Hohenpeissenberg observations, the changes of declination are expressed in angular value, and the changes of intensity are corrected for temperature, and are expressed in parts of the whole intensity. The observations at Hohenpeissenberg comprise only a portion of the duration of the disturbance of the 25th of September, to which portion the comparison is necessarily limited. The memoir concludes with observations of the absolute declination and absolute horizontal intensity on the Hohenpeissenberg, but these are unaccompanied by any remarks of general interest.]

SCIENTIFIC MEMOIRS.

VOL. III.—PART XII.

ARTICLE XXI.

Proposal of a new Nomenclature for the Science of Calorific Radiations. By M. MELLONI.

[From the *Bibliothèque Universelle de Genève*, No. 70, for October 1841.]

THE differences lately discovered between the immediate passage of caloric and of light through solid and liquid media, induced me some years ago to propose certain expressions designed to classify the new properties, and to distinguish them clearly from those which relate to the faculty of transmitting or of intercepting the luminous rays. Further progress has since showed that the force to which we must attribute the absorption of heat by transparent media did not act in the same way upon radiations of different origin; and that the emergent radiations of the media permeable to heat freely traversed certain kinds of screens, themselves permeable to other calorific radiations. This consequence was the result, *that radiant heat was composed of different elements, and that a certain number of these elements existed in a greater or less proportion in the radiation from each source.* All these rays passed, notwithstanding, abundantly and in the same proportion through a certain solid substance; and all the media which gave different calorific transmissions lost their differential property, and became entirely analogous to the substance of equal transmissibility when they were reduced to very thin slices. Thence, in my opinion, the conclusion *that there is a perfect analogy between calorific transmission through colourless bodies, such as water or glass, and the transmission of light through coloured media.* It was therefore necessary to have recourse to other denominations, in order not to confound together these two kinds of action.

Very recent experiments indeed showed that there existed, in the phænomena of the absorption and diffusion of luminous and calorific radiations through opake bodies, a series of facts altogether analogous to the differences observed in their transmission by diaphanous media. In fact we see the whitest substances, as paper, snow, carbonate of lead, wholly absorb certain rays of heat, and disperse others in the manner of the luminous diffusion; whilst coloured substances, like metals, disperse and absorb, in proportions sensibly equal, all kinds of calorific radiations: the first act just as a red body would do exposed in succession to green light and to red; the second like white bodies receiving lights of different colours. Thus *the coloured and the white calorifics exist; but they have no connexion with the colours properly so called, and must be carefully distinguished from them.*

The necessity, then, for a new nomenclature to express the properties of bodies relatively to radiant heat cannot be doubtful.

In the last edition of his *Elémens de Physique Expérimentale*, M. Pouillet proposes to designate by the term *thermanism* the faculty which ponderable substances possess of absorbing and retaining such amongst the several elements of which the incident calorific stream is composed as best suit them, leaving the rest at liberty. Thus the bodies which alter the composition of the stream of heat by a special absorption would be *thermanizing* substances, and the heat which has undergone the action of thermanizing substances would be *thermanized* heat. I own that these denominations are at first sight attractive by their extreme simplicity; but unfortunately they are open to several very strong objections: first, because their radical contains no expression relative to the fact of elective absorption, which however they ought to define or at least indicate; in the next place, because they hardly satisfy all the wants of science: in order to be convinced of this it is sufficient to observe that *black* bodies relatively to heat, as well as *white* bodies, would be *non-thermanizing* substances, so that two contrary actions would be confounded together under the same name.

Having formed the project of publishing a work in which I shall try to bring together all that we positively know at present on the properties of radiant heat, I have found myself stopt, at the first setting out, by the imperfection of the language of this branch of physics, which has naturally led me to the formation of a new system of nomenclature. I will explain the principles which have guided me in its production.

The comparison between the properties of heat in its ordinary state and in the radiant form, furnishes several distinctive characters between these two great classes of natural phænomena. In fact, ordinary heat is propagated slowly; it traverses any direction whatever, straight or sinuous; it undergoes greater or less alteration in the velocity and direction of its motion as well as in its own energy, when we agitate the medium through which the propagation is effected. Radiant heat, on the contrary, leaps the whole extent of the medium in an imperceptible instant; it goes only in a straight line, and always preserves the same intensity and the same direction whatever may be the state of repose or of motion of the particles of the medium traversed.

Each of these three properties belonging to the two methods of transmission, namely, *the velocity of the calorific flow, its direction, and its connexion with the agitation of the medium*, takes, in one of the two cases, a contrary character to that which it affects in the other. All may therefore serve as a basis to the system of nomenclature which we wish to establish; but the corresponding Greek or Latin words, taken as radicals, scarcely lend themselves to the formation of a language capable of expressing with ease, and with a suitable precision, the different actions of bodies on calorific radiations. It is nearly the same with regard to the expression *ray of heat*, upon which we might equally found the new thermological nomenclature, if we were not every instant stopt by the hardness and the complication of its derivations*. Happily, besides the characteristic

* If we were content with the substantive ἀκτὶν (ray), the greater part of these difficulties might be avoided, and perhaps a very simple nomenclature might be formed; but we should fall into the very serious inconvenience of making the denominations applicable to all kinds of rays, which would not fail to occasion frequent mistakes, and thus introduce a real confusion into the science. And here the demonstration immediately follows the principle; for the confusion is about to begin with some new names which have lately been proposed in meteorology. In fact, the members of the Committee of the Royal Society of London who were charged with the scientific instructions for Captain Ross's voyage, call an instrument which serves to measure the heating power of the solar rays an *actinometer*. M. Pouillet describes under the same name his thermoscopic apparatus, designed for the exploration of the nocturnal cooling of bodies under a serene sky. Sir J. Herschel indeed uses *actinograph*, a denomination quite analogous, to indicate a very ingenious little machine of his invention, by means of which, according to him, the different degrees of light which succeed each other during the day are measured. Now why should not some other physicist wish to apply, and with as good reason as his predecessors, the word *actinology*, not to such and such a branch of radiant heat,

properties drawn from their modes of propagation, ordinary heat and radiant heat present a fourth distinctive and well-defined character, to which I now call the attention of the reader.

We know that ordinary heat, that is to say the heat which is propagated from layer to layer in the interior of bodies and produces their elevation of temperature, possesses a homogeneous constitution, and that consequently two calorific streams of this kind, endowed with the same intensity, are necessarily identical. Two effluxes of radiant heat of the same force but of different origin, far from being equal in all points, possess, on the contrary, very distinct properties; for they suffer a more or less abundant dispersion at the surface of opake bodies, and penetrate in greater or less proportion into the interior of colourless media. Moreover, the different refrangibility of the elements which constitute each flow of radiant heat, and their more or less perfect absorption under the action of the same substances, establish, as we just now said, a complete analogy between the calorific rays and the luminous rays of different colours.

Thus the variety of species, the heterogeneity of the elements which compose them, and above all their great analogy with the coloured rays, form an entire body of properties which belong exclusively to radiant heat. These properties are therefore sufficient to distinguish it from ordinary heat, always homogeneous, and not presenting any point of resemblance with light. For this reason I propose the name of *Thermochroology**, that is to say, *Doctrine of coloured heat,* in order to designate the science of radiant heat.

If it be alleged that you cannot apply to an invisible agent, as heat, the name of a quality which in another agent is visible, I would say that sound also is in the same case in relation to light; and although Acoustics have by no means the relations of analogy with Optics which radiant heat has, yet the introduction of the term *chromatic scale* has been allowed, thus comparing a series of sounds more or less grave to the principal colours of light†. The radical *chroma,* however, signifies

and still less to a collection of photometric measures, but to the science which treats of the chemical radiations of the sun and of incandescent bodies?

* From *θερμὸς hot,* [*heat*] *χρόα colour*, by changing *α* into *ο*, and *λόγος discourse.*

† Some artists pretend that the term *chromatic scale* is derived from an ancient custom of marking the notes with red ink; but the origin drawn from the comparison of sounds to colours appears much more likely.

colour of painting, and cannot have, for our purpose, the same propriety of expression as the word *chroa, colour of light*, which is precisely the phænomenon to which allusion is made in the comparison in which we are engaged. But I will reply still more directly to the question:—First, I shall observe that it is possible to define coloration otherwise than by the immediate testimony of sight; in fact the coloured rays are not distinguished from one another by the mere difference of the sensations produced on our eyes, but by differences of strength between the modifications of which these rays are susceptible under the action of bodies: it is thus that the red rays are less refracted than the green, that they are sent back or transmitted by red bodies in greater quantity than the green rays, or *vice versâ* relatively to green bodies. Moreover, in certain cases these differences constitute the only distinctive characters of luminous rays. We know, for example, that certain individuals are insensible to red light, or to speak more exactly, we know that red light seems to them a colour perfectly similar to that of green light; the red and green rays can then only be distinguished in the eyes of these individuals by the differences of diffusion, of absorption, and of transmission, as just alluded to. Thus, when illuminating a room successively by a light transmitted first by a red glass and then by a green glass, and presenting in both cases a green cloth and a red cloth to the person who confounds the two colours together, he will immediately comprehend that the two kinds of light introduced in the ambient space, although perfectly alike to his eyes when they receive the ordinary light of day, are scarcely identical, since the red cloth, which is very bright when the room is lit by the red light, becomes sombre when the ambient space is illuminated by the green light; and that, on the contrary, the green cloth, which took a dark tint in the first experiment, appears very bright in the second. Analogous proofs might be had by means of two coloured media, the one green and the other red, which would furnish a strong and a feeble transmission, first of one kind, then of the opposite kind, according to the red or green colour of the illuminating light. But calorific radiations give precisely differential characters of the same kind; the expression *colour of heat*, then, has in it nothing which should offend, and it even satisfies every principle of the most rigorous logic.

The order adopted for the study of light, and the classification of this science, will perhaps cause a second objection. "You say that rays of heat are perfectly analogous to coloured rays: the science of radiant caloric should then be treated like light. Why do you apply to it a denomination which belongs to a single branch of optics?" Because radiant heat is manifested with data very different in many respects from those which preside at the manifestation of light. The sun sends to us, mixed up in a single ray, all the elements which constitute white light, the general properties of which may, and indeed ought to be completely known before we show that this light contains an infinity of different rays. But *white heat* does not exist; at least the complete series of the calorific elements is never met with in a single pencil; so that all effluxes of heat are *chromatic*, or, to be more exact, *chroïc*. In fact, the radiations of bodies feebly heated are destitute of several elements which are found in the radiations of heat emitted by flames and incandescent bodies; and, on the contrary, several elements contained in the calorific effluxes of sources at a low temperature do not exist in the effluxes of sources at a high temperature; the light of the sun itself, which contains all the colours and different calorific radiations, does not contain any of the elements peculiar to the radiations of sources at a low temperature. Radiant heat, of whatever origin, is then constantly *coloured*, comprehending also solar heat, which, according to what we have just said, possesses here, at the surface of the earth, a much more vivid coloration than that of the radiation of flames and of incandescent bodies. It follows from this that the qualities peculiar to each of these radiations constitute the first notions which must be acquired for the science of calorific radiation. Doubtless all the elements of radiant heat present common properties with respect to the mode of free propagation, and to the laws of refraction, of reflexion and of polarization; but these general properties can only be rendered apparent by a sustained comparison of particular properties, which constitute in ultimate analysis what I call the *colour of heat*. Thus *coloration* forms the true framework (*charpente*) of radiant heat; the term which represents it is then eminently fit for characterizing the science.

I will add, lastly, that in giving the name of Thermochroology to the science which relates to the whole of the properties which calorific radiations possess, we not only employ a more significant expression than that which is at present used, since the idea of colour necessarily includes that of the radiating form and of a heterogeneous constitution, but we introduce into the study of physics a term more conformable to the great end to which scientific nomenclatures tend, namely the property of continually calling to our mind the generalities of the group of phænomena which is under consideration. In fact, the intimate bond established between the idea of colour and that of calorific radiation, does not allow us for a single instant to lose sight of the principle which serves as a basis and summary for the properties newly discovered regarding radiant heat, a simple and prolific principle, which brings facts the most unlike near to each other, so that it is sufficient to call to mind that there exists in the calorific rays, and in bodies endowed with the greatest limpidity, or with the greatest degree of whiteness, a quality invisible, but entirely analogous to coloration, in order to predict or explain all the series of the phænomena of transmission, of diffusion and of absorption, which the science of calorific radiations now presents*.

Coloration once adopted as a distinguishing character of radiant heat, it is evident that it should form the fundamental basis of the language relative to this branch of science: I have endeavoured to fulfill this condition in my attempt at nomenclature. In fact the word *thermochrosis*, which I use to designate this *coloration* of bodies or of calorific rays, comes from the same radicals whence the expression Thermochroology † is derived, as well as the adjectives *thermochroic*, coloured for heat, and *athermochroic* ‡, without colour of heat. From the Greek word for black §, I name those bodies *melanothermic* which absorb all kinds of calorific radiations energetically and in equal proportions, acting thus upon heat as black substances act upon light. Those substances, on the contrary, which disperse the different

* "Every physical science," says Lavoisier, "is necessarily formed of three things,—the series of facts which constitute the science, the ideas which call them to mind, the words which express them. The word should give birth to the idea, the idea depict the fact: they are three impressions of one seal." The denomination thermochroology and its derivatives answer exceedingly well, if I do not deceive myself, to the three conditions proclaimed by the great legislator of chemistry.

† From θερμὸς *hot*, [*heat*] and χρόα *colour*; whence the verb χρόω *to colour*, and χρῶσις *coloration*.

‡ From α privative, and θερμοχροϊκὸς *coloured for heat*.

§ From μέλας, gen. μέλανος *black*.

species of radiant heat powerfully and equally, as white bodies do with respect to light, bear the name of *leucothermic**, that is to say *white* for heat.

As to the media which transmit or which intercept the calorific effluences, I should advise that the first words *diathermanous* and *athermanous* be thrown aside, and that the denominations *diathermic* and *adiathermic*, which have a more regular formation, and are more conformable to the terminations of the new nomenclature, should be substituted for them. Then the *calorific transparence* of the bodies, or *transcalescence*, to use a term of Sir J. F. W. Herschel's, would be called *diathermasy*†, and we should call the contrary property *adiathermasy*‡, that is to say, opacity in relation to the radiations of heat§.

The substances which only transmit certain kinds of heat, constitute, according to this new system of nomenclature, *diathermic thermochroic* bodies; those which transmit all kinds of radiations without distinction, are *diathermic athermochroic* bodies; denominations, however, which may be abridged by simply calling the first *thermochroic media* and the second *athermochroic media*. A similar abbreviation may also be used in reference to opake substances, which should be called, strictly speaking, *adiathermic melanothermic*, *adiathermic leucothermic*, *adiathermic thermochroic bodies*, according to the nature of their tint, whether *black*, *white*, or *coloured for heat*, but which will always be sufficiently characterized by the latter expressions, if they are designated simply as *thermochroic*, *leucothermic*, or *melanothermic* substances, as the practice is in relation to light, in ordinary language, or when, in treating of opake bodies, the custom is to suppress all allusion to transparency; which is not only expeditious but very philosophical, for transparency is but an exception to the general law of opacity, and forms, so to speak, a character of transition between ponderable matter and æthereal substances.

I conclude by offering the following synoptical outline, in which will be found brought together all that concerns the new nomenclature, and some applications.

* From λευκὸς *white*.

† From θερμάω *to heat*, and διὰ *through*, *across*.

‡ From α privative, and διαθερμασία *calorific transparence*.

§ These changes, as well as some other etymological modifications, are made by the advice of M. Antonio Ranieri, a young Neapolitan, well known in Italy by his touching and generous friendship towards that fine genius Jacopo Leopardi, snatched by a premature death from the admiration of his countrymen.

Systematic and Etymological View of the New Nomenclature of Radiant Caloric.

Thermochroology (from θερμὸς *warm,* [*heat*] χρόα *colour,* changing the *a* into *o,* and λόγος *discourse*), *doctrine of coloured heat,* and, according to us, science of radiant heat; 1st, because it is only radiant heat which is composed of different elements, quite analogous to the coloured rays of light; 2nd, because *white heat* does not exist at the surface of the earth; 3rd, because the *colour of heat* not only indicates the radiant form and the heterogeneity of the elementary pencils (*filets*), but also because this phrase constantly brings to mind the supposition of a particular coloration, quite distinct from ordinary coloration; a supposition which comprises all the properties newly discovered in bodies with regard to calorific radiations.

Diathermasy (from θερμάω *to heat,* and διὰ *through, across*), *transcalescence,* or *calorific transparency of bodies.*

Adiathermasy (from *a* priv., and διαθερμασία *transcalescence*), *calorific opacity.*

Diathermic (from διὰ *through,* and θερμὸς *warm* [*heat*]), *transcalescent, diaphanous for heat.*

Adiathermic (from *a* priv., and διαθερμικὸς *transcalescent*), *destitute of calorific transparency, opake for heat.*

Thermochrosis (from θερμὸς *warm,* [*heat*] and χρόα *colour*; whence χρόω *to colour,* and χρῶσις *coloration*), *coloration of heat.*

Thermochroic (from θερμὸς *warm,* [*heat*] and χρόα *colour*), *coloured for heat.*

Athermochroic (from *a* priv., and θερμοχροϊκὸς *coloured for heat*), *without calorific colour, colourless for heat.*

Melanothermic (from μέλας, gen. μέλανος *black,* and θερμὸς *warm* [*heat*]), *that which is black for heat,* because it absorbs nearly all the incident heat, and thus acts on the calorific rays as black bodies do with regard to light.

Leucothermic (from λευκὸς *white,* and θερμὸς *warm* [*heat*]), *white for heat,* because its property is to *diffuse* or disperse every kind of calorific radiation with the same intensity; which maintains the composition of the incident stream diffused in the calorific flow, and has an effect absolutely similar to that which white bodies produce on light.

Examples.

Black mica, obsidian, black glass, reduced into very thin laminæ, without however losing their complete opacity in respect to light, still allow a certain portion of radiant heat to pass through them; these bodies consequently are opake and *diathermic*. Certain glasses of a green colour, in combination with a layer of water or a very clear plate of alum, are on the contrary *adiathermic*, that is to say opake for heat, notwithstanding their transparency for light.

Atmospheric air and rock-salt, which, within the limits of our experience, transmit every kind of calorific radiation, evidently constitute *athermochroic media*. Glass, water, alcohol, and other colourless liquids, being permeable only to certain species of heat, and intercepting the other calorific rays in a lesser or greater proportion, form, on the contrary, *thermochroic media*.

Paper, snow, whitening, or white lead, which, notwithstanding their extreme whiteness, do not send back the rays peculiar to the different sources of heat with the same intensity, and even totally absorb certain calorific radiations, should, strictly speaking, be called *adiathermic thermochroic* substances; but it will suffice to designate them by the latter word, as is daily practised in ordinary language, in which the adjective *coloured*, being applied singly to a word, quite naturally carries with it the want of transparency.

Very pure metals, in any mechanical state, and more particularly dead gold and silver, diffuse every kind of calorific radiation vigorously, and in the same proportions: these bodies, then, should be classed amongst *leucothermic* substances, although generally coloured. In short, lampblack, which absorbs almost all incident light and heat, will form a substance which is at the same time black and *melanothermic*.

Naples, October 7, 1841.

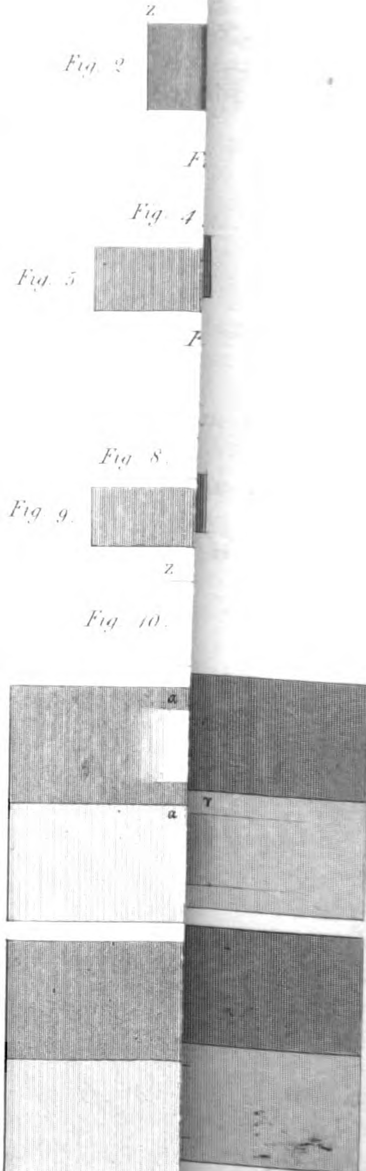

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Fig. 2
Fig. 4
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Fig. 11.
Fig. 12.

ARTICLE XXII.

√

Memoir on the Constitution of the Solar Spectrum, presented to the Academy of Sciences at the Meeting of the 13*th of June,* 1842, *by* M. EDMOND BECQUEREL.

[From the *Bibliothèque Universelle de Genève*, No. 80, for August 1842.]

THE faculty of illuminating bodies is not the only one possessed by solar light; it has also different properties which are manifested to us by the changes which it produces on some of these same bodies, such, for example, as raising their temperature, of causing chemical modifications to take place among the elements of some of them, and, lastly, of giving to others the faculty of being self-luminous or phosphorescent. These different properties, long since known, have given rise to a multitude of beautiful investigations by natural philosophers and chemists; but as these are described in works on physics, I shall not enter into any detail upon this subject.

The chemical action of light has long been the object of my favourite studies, which have led me to recognise a property common not only to the spectra formed by the chemical rays, but also to those which are owing to the phosphorogenic rays. This is the motive which has induced me to bring together in a single memoir all the observations which I have recently made on this subject.

It is not my intention to enter upon the constitution of the luminous spectrum, as the title of the memoir appears to indicate, but on that of the spectra formed by the radiations which accompany solar light and light in general; that is to say, of chemical, phosphorogenic and calorific spectra.

In this memoir, by *chemical spectrum* will be designated the whole of the rays which, in the solar spectrum, impress a substance which has the property of being modified under their influence; and by *phosphorogenic spectrum*, the whole of the rays which excite phosphorescence on a sensible substance.

This memoir will be divided into four parts, as follows: the first will comprehend a rapid description of the formation of the luminous spectrum and of the discovery of Fraunhofer; the second, the results of my late researches on the chemical rays; the third, the results obtained on the phosphorogenic rays; and, lastly, in the fourth will be summed up the principal properties of these different rays, and some considerations on their mode of action.

When I began this work my aim was to comprehend in it the study of the calorific rays, in order to complete the question, by studying all the radiations which accompany light: not having been able to complete this design, I have chosen, in the first place, to publish the results which I have obtained respecting the other radiations.

§ I.

Without entering upon the composition of light which is to be found in all works, I will confine myself to the relation of the following facts. If we refract a pencil of solar rays through a prism of flint-glass, and receive the oblong refracted image on a white card, seven kinds of colours may be pretty clearly distinguished, or seven parts of the image, each of which is coloured nearly of the same tint; these colours are red, orange, yellow, green, blue, indigo and violet; this last being that of the most refrangible rays. Figure 1. Plate IX. gives an idea of the luminous spectrum. The limit on the side of the red is evident enough, whilst on the side of the extreme violet it is more confused; and even beyond the extreme violet, which is at H, we still see a slight gray tint, which becomes weaker and weaker.

If we compare the luminous spectra obtained by means of prisms of different substances, we see that the same colours do not occupy proportional spaces.

The luminous spectrum is not continuous throughout; it is traversed by dark lines which separate the coloured bands. This phænomenon appears to have been first observed by Wollaston, but it was Fraunhofer who gave an accurate description of it.

The last-mentioned philosopher having introduced a pencil of solar rays into a darkened room through a narrow and vertical aperture made in the shutter about one millimetre in width and several centimetres long, made these rays fall on a prism of flint-glass placed before the object-glass of a theodolite, so that the edge of the prism was parallel to the aperture of the shutter, and the angle of the incident ray on the prism was equal to that of the emergent ray; that is to say, that the prism was placed at the minimum of deviation of the luminous ray: he then saw an infinite number of vertical stripes or lines of different thicknesses. These lines are darker than the rest of the spectrum; some amongst them even appear quite black. The distances of these lines, or their relative positions, do not undergo any change, neither by that of the aperture of the shutter, nor by the removal of the telescope from this aperture. Neither the nature of the substance of the prism nor its refractive angle prevent these lines from being distinguished; they only become stronger or weaker; but the ratios of their respective distances seem to be the same for all refractive substances.

Fig. 1, which has been made from the drawing of Fraunhofer, and which is a quarter its size, represents the luminous spectrum with all its lines. The red nearly terminates at A, the violet, or the limit which follows the violet, at I; we cannot speak with certainty respecting these limits, which are more distinct at the red than at the violet.

If the light of an illuminated cloud fall on the prism through the same aperture, the spectrum appears to be limited on one side between G and H, on the other at B: the direct light of the sun therefore extends the spectrum nearly the half of its length.

At A there is a well-marked line; the red, however, continues a little further; at *a* is a mass of fine lines. The line B is of a rather considerable tint and thickness. From B to C there are nine well-determined lines. C is thick and black like B. Between C and D there are thirty fine lines, which, if two of them are excepted, can only be perceived by being strongly magnified, or with prisms of great dimensions. D is formed by two strong lines separated by a bright line. Between D and E we meet with about eighty-four lines of different size. E consists of several lines, of which the middle one is the strongest: from E to B there are about twenty-four lines. *b* is formed of three lines, two of which are separated by a fine and bright line; *b* F contains fifty-two stripes. F is very large; between F and G there are 185 stripes of different dimensions; at G are crowded together many lines, several of which are remarkable for their size; from G to H we reckon 190 stripes; the two bands H are nearly equal, and formed of several lines, in the midst of which there is one very strong one; from H to I they are in very great numbers.

The lines A, B, C are in the red and orange; D is at the limit of the orange and the yellow; E and *b* are in the green; F is in the commencement of the blue; G between the blue and the indigo; and H at the end of the violet; but beginning from H as far as I, the tint of the light is ashy-gray, and at H it departs from the violet tint. The luminous limit of this side is very difficult to perceive.

If we examine the spectra formed by the light of the planets and that of the moon, the same lines are found as with the solar light and at the same intervals, which proves that they have the same origin; but this is not the case with the stars, such as Sirius, &c.: there are indeed black lines, but they are no longer the same. These phænomena, which belong to the very nature of light, are very interesting to study.

Artificial lights only afford black lines after having traversed differently coloured bodies. We, however, see in their spectra one or two brilliant lines towards the yellow, and, in the electric light alone, several brilliant lines distributed throughout the spectrum. These lines no doubt arise from rays of different degrees of refrangibility, which are absorbed by the media through which they pass. Fraunhofer operated, as we have seen, by receiving on a telescope, which magnified fifteen or twenty times, the rays refracted through the flint-glass prism placed before the object-glass very near it, and in the position of minimum deviation.

They can also be seen by projecting the spectrum on a white screen; but as the finest lines cannot then be distinguished, we shall point out the manner of operating in order to study them, having ourselves had occasion to project them upon different substances in other experiments, the results of which will hereafter be given.

In the path of a solar ray reflected from a mirror and passing through a narrow vertical aperture, is placed a very pure prism of flint-glass, having its edge vertical, and disposed in the direction of the minimum deviation; then immediately behind the prism a lens of about one metre in focal length is interposed. If the prism is situated at a distance equal to two metres, or twice the focal distance of the lens from the aperture, and if a white surface is placed at two metres from the prism, then the lines of the spectrum will be very well depicted upon its surface. Without a converging lens we could not distinguish the lines by projection. If the distance of the white surface with respect to the lens is a little modified, we see the lines depicted with more or less distinctness. In order to see the fine lines of the less refrangible parts of the luminous spectrum, it is necessary to have an aperture of only one-fourth of a millimetre in diameter at the utmost; but in order to study the great lines, and especially the lines H and those beyond, the aperture must have a diameter of one to two millimetres. In this case the two lines H of the extreme violet are very distinct, and we see some lines beyond in the rays whose colour is of a weak gray-lavender tint.

Moreover, and this remark is important, as the position of the lines depends solely on the position of the aperture and on that of the prism, the apparent movement of the sun does not make them change place, so that it is useless to have a heliostat for the purpose of keeping them in the same position; a simple light-director which is turned with the hand serves the same purpose, only the spectrum is more or less illuminated.

Thus, from what we have just related, an idea of the formation of the solar spectrum and of the dark lines which separate the luminous bands may be conceived. We will not enlarge any more on this head, nor on the primitive colours of light, as this question does not belong to the subject of which we have to treat.

§ II. *Of the Chemical Spectrum.*

If we introduce a pencil of solar rays into a dark room through an aperture made in a shutter, if we refract it through a prism of flint-glass, and project the spectrum produced upon different substances which undergo chemical modifications resulting from the action of the solar rays, the chemical rays which act on these substances are not situated, as we know, in the same parts of the spectrum, so that the chemical spectrum has not the same extent for each impressible substance. We shall see examples of this further on. There exist besides a great number of substances which undergo chemical modifications under the influence of the agent which accompanies light; but amongst the most impressible we must without doubt reckon most of the salts of silver. In a former memoir, I showed that, with regard to some of these salts, and probably with all, a very curious phænomenon took place: in the solar spectrum there are rays which do not begin a chemical action, but which continue it if it is once begun; that is to say, which do not act except upon a substance already impressed; and these rays extend, for each substance, from the limit of the least refrangible chemical rays, that is to say, nearly from the blue to the red.

As I intended to study the properties of the chemical rays, as well as those of the luminous rays, I had to ascertain whether the chemical spectrum was continuous on the different impressible substances, or if, as in the case of the luminous spectrum, it was interrupted by more or less thick, black lines. I was led to this curious result, that the chemical spectrum has the same lines as the luminous spectrum, provided we only consider the parts of the same degree of refrangibility of these two radiations.

To make this fact evident, I took a frame capable of receiving another, on which I stretched different papers covered with impressible substances, or surfaces of different nature. By using a plate of silver iodized, according to M. Daguerre's method, and by exposing it a minute or two in the frame, placed at the distance above mentioned, so as to receive the spectrum by projection, and afterwards subjecting it to the mercurial vapour, we see that the plate has been more or less impressed, according to the intensity of the chemical action of the rays; generally it will be from the indigo to within a certain distance of the extreme violet. We then see a great quantity of lines, as in the luminous spectrum; and by drawing these lines with their position and respective sizes, we find that they correspond to those of the luminous spectrum, at least in the visible parts; the lines which are best seen and which appear the strongest, are the two marked H.

By using a plate of silver iodized, and afterwards submitted to the vapour of chlorine or of bromine, which, as we know, gives a very great sensibility to the iodide of silver, the same effects are obtained in a few seconds. If the plate remain exposed to the spectrum for a longer time, say a few minutes, the image becomes more marked and more extended beyond the violet, whilst in the vicinity of H the too long continued action of the chemical rays blackens all the plate and prevents the lines from being distinguished in this part. On the other side of H, beyond the visible parts, we find an infinite number of lines, which I will indicate further on in fig. 2.

From what has just been said they cannot all be drawn by means of the measures taken upon the same plate; it is therefore necessary to take a great number of impressions by exposing the plates, during different times, with different apertures of the shutter, in order to obtain fine lines. If the plate remains exposed during an hour in the spectrum, we see the action extend from the limit of the green and of the blue as far as P beyond the violet (fig. 2).

A longer time does not give any action beyond, we merely perceive a species of second spectrum from the green as far as the red; but it is the spectrum of the continuing rays, which merely act because the plate exposed during a certain time to the action of the solar spectrum is in a small degree impressed by the diffused light which always accompanies the spectrum.

The action of the exciting rays, that is to say of the rays which are ordinarily considered, and which commence and continue a chemical action, ceases with regard to the simply iodized plates towards the limits of the indigo at G; but with respect to the plates which are iodized and afterwards exposed to the vapour of chlorine or of bromine, it goes as far as F in the extreme blue by the side of the red.

The spectrum of the continuing rays does not extend beyond the extreme red, for its action goes on diminishing as far as this extremity; but it is especially with the simply iodized plates that it shows itself best. In fact, these plates being less sensible than others, the diffused light which accompanies the solar spectrum does not suffice to impress them enough at first. In order to recognise the existence of these rays, it is necessary to impress these plates before the experiment, by exposing them some seconds to the daylight; I have also shown in a memoir that the action of the continuing rays was so much more energetic in as far as the substance was more impressed, but only to a certain limit.

Figure 2 represents the spectrum with all its lines, drawn according to experiments made with different plates iodized and then exposed to the vapour of the chloride of iodine. The portion of the image which first appears is that which is contained between H and G; the maximum of action is then nearly between these two lines. If the action is left to continue, it does not extend beyond F, and indeed it suddenly ceases nearly at this ray, but extends, diminishing by degrees in intensity as far as Z beyond the visible rays. If these plates remain exposed for a long time to the action of the spectrum, there is an action which is manifested from F as far as the extreme red; this effect arises only, as I have already said, from the action of the continuing rays, which act upon the impressed substance by means of the diffused light which accompanies the solar spectrum. This action may be obtained in a higher degree by impressing the whole plate at first.

If we draw the lines and measure accurately their respective distances, we find that between H and A there are the same lines as in the case of the luminous rays and identically at the same distances; but in the chemical spectrum we only see the great and moderate lines. It is impossible to produce the finest lines by projection, for the spectrum thus obtained is never so distinct as when seen through a telescope, and the reason of this is obvious, for the focus of the lens changes for each ray. We see, however, according to the drawing represented in fig. 3, which I have endeavoured to render as faithful as possible, that it is certain that the same lines exist in the luminous spectrum and in the chemical spectrum, and that the same cause which has determined the production of the lines in the solar spectrum has also determined those of the chemical spectrum.

Beyond H the chemical spectrum has an infinite number of lines. About as far as M they are still the same as those of the luminous spectrum; but further on the luminous rays always become feeble; they are no longer comparable: it is probable, however, that, if these last extended beyond, we should find the same lines as those of the spectrum which we are about to describe.

At first, after the two lines H, we see a great number of lines, and among these a very broad line I, formed by the union of a great number of smaller lines. After it, at M, come four lines remarkable for their distinctness; they are of the same size and nearly equally distant; the last only is a little further off. These four lines occur towards the limit Y of the last gray-violet luminous rays. Beyond these lines, at N, are four other lines stronger than the preceding, nearly equal to each other, the fourth being rather broader than the others. There are at O two great lines at the same distance from each other as the two lines H, the first being a little stronger than the second; then come other strong lines, amongst which we distinguish the line P, which is very strong and very black; after the latter there are others, but hardly distinct, for P is nearly the limit to which the chemical action extends.

We have only described the most remarkable; there are a quantity of others which are represented, and probably a great number of fine lines which cannot be shown by projection.

This is the extent of the chemical spectrum obtained with the iodized plates, that is to say from Z as far as X, fig. 3, this spectrum containing the continuing rays from F as far as X. When we employ plates simply iodized, the action of the continuing [exciting?] rays, instead of extending from Z as far as F, has its limits between O and P on one side, and on the other between G and H; the continuing rays begin between G and H at the boundary of these, and go as far as the red.

Figures 4 and 5 represent the extent of the spectra obtained in these two circumstances.

In the following manner we may observe the lines of the chemical spectra produced during the action of the solar light on different sensible substances. For example, we take paper prepared with bromide of silver, chloride of silver, or any salt of silver whatever. It is placed upon a slate if we wish to act upon it whilst it is moist, or in a wooden frame in case it is to be operated upon when it is dry. It is then put in the frame, so that the solar spectrum by projection shall be exactly at the focus of the lens. We then see the lines of the luminous spectrum, and, by allowing the action of the light to continue during an interval of time, depending on the sensibility of the salts, the image is seen to form upon the paper. With these salts the effect is generally the inverse of the natural spectrum, that is to say that the parts corresponding to the chemical rays appear black on the paper, and that the lines remain white; they are less easily distinguished than upon the iodized plates, but, with a little attention, the lines of intermediate size are found in the same positions as formerly, and consequently in the same places as the lines of the luminous spectrum; thus the same figure (3) gives all the rays of the chemical spectra. The action of the continuing rays, of which I have already spoken, on these different salts is also seen; these rays extending from the least refrangible limit of the exciting rays as far as the red.

With these substances *the maximum of action does not occur at the same place in the spectrum*; but it is always situated nearly between H and G. The lines H are therefore those which first appear.

Fig. 5 represents the action of the spectrum on paper prepared with bromide of silver, with the omission of the lines.

If we use different sensible substances the chemical spectrum is not of the same length, and extends a greater or less distance on the side of the red. It is thus that Sir J. Herschel, when studying the action of the iodide of potassium on paper prepared with a salt of silver already acted upon, saw an action produced beyond the red rays.

I shall speak somewhat more in detail of the action produced by light on paper prepared by this method.

We know that a sheet of paper done over with a salt of silver generally becomes black when exposed to the light; but if this paper is afterwards washed in a solution of an alkaline iodide, such as the iodide of potassium or of sodium, and if it is exposed when still moist to the action of the light, it directly becomes white: this reaction even takes place between the alkaline iodide and the metallic silver, under the influence of the solar rays potash is liberated, and iodide of silver is formed. If, as Sir J. Herschel did, we prepare a paper with a strong solution of acetate of lead, of bromide of potassium, then of nitrate of silver, this paper then done over with bromide and acetate of silver will become black in the light; but if, when it has become black and has imbibed a weak solution of iodide of potassium, it is exposed in the spectrum, it directly becomes white towards the most refrangible extremity, and this action even continues beyond, whilst under some circumstances the paper will blacken more in the least refrangible parts; this action then extends as far as the red, and even beyond. In order that this experiment should succeed, it is necessary that the solution of alkaline iodide should be very much diluted. When it is stronger the paper begins to become white in the least refrangible rays, whilst between the white and obscure part there is a neutral space. With another dose of iodide, the neutral line approaches the red; in short, when there is a sufficient solution of iodide, the paper becomes of a distinctly white nearly toward the red; with a very strong solution, the paper even becomes white in the dark.

In this reaction there are some very complicated effects, for not only does the iodide of potassium exercise an action on the reduced salt of silver, but the light also acts on the salt of silver already affected, as we have established for the salts of silver. If then the layer of iodide is weak, this second reaction may prevail over the first and blacken the paper in the least refrangible parts of the spectrum.

In order to guard against this effect, when once the paper is affected, care must be taken to wash it with common water and then to plunge it into a solution of iodide of potassium strong enough to avoid these two reactions. The effect also depends on the manner in which the paper has been at first affected. If it has been strongly blackened, the action does not extend beyond the blue; but if it has been less black, it extends as far as the red.

Fig. 6 represents the image of the spectrum obtained by projection on a sheet of paper prepared with the precautions before indicated, and by affecting the paper slightly before the experiment, an idea may be obtained of the distance to which the chemical action extends. By means of this paper, all the lines which we have already described are depicted with tolerable clearness.

By help of Mr. Talbot's method, which consists in bringing gallic acid to act by means of heat on a paper done over with iodide of silver, and which has received the impression of the image intended to be produced, tolerable results are obtained; but as the sensibility of this paper is very great, it is necessary that the action should be of as short duration as possible, if not the lines would not be distinct. This method gives an inverse image, that is to say, the shadows are represented by bright places, and *vice versâ*.

We have not only made use of papers prepared with salts of silver, but also of papers done over with other sensible substances, such as the bichromate of potash, the resin of guaiacum, &c.

We know that a sheet of paper prepared with bichromate of potash, when it is dry, changes its colour more rapidly in the light, and from a pale yellow becomes a dark wood colour. This coloration, which arises from the conversion of the chromic acid into oxide of chrome through the action of organic substances under the influence of light, has its maximum of action in the spectrum at the limit of the green and the blue, or towards the line F. Fig. 7 gives an idea of the action of the spectrum on this paper, it being understood that the lines are omitted. The action ceases almost instantaneously at the extremity of the green, beyond the lines F and *b*; but on the most refrangible part of the spectrum it continually becomes weaker, so that towards M the coloration is scarcely sensible. When the action of the light is prolonged, whilst the spectrum is projected on paper prepared in this manner, we still see the image of the same lines which we have indicated, but feebly marked in white, for it is a paper producing inverse effects. If it is soaked in a dilute alcoholic solution of iodine, the light spaces become blue from the reaction of the iodine on the starch; the lines are then more visible.

We have also used a paper done over with a layer of guaiacum resin, which becomes blue in the light. It gives the same results with respect to the rays as the other sensible substances, only in a weaker degree, for it becomes blue but slowly under the influence of the solar rays. The blue colour which this resin assumes under the influence of the light arises from its oxidation, since all the oxidizing bodies, such as chlorine, bromine, &c., give it this colour. A greater degree of sensibility may be given to this resin by soaking the paper in a weak solution of nitrate of silver before putting a layer of guaiacum on it; the results obtained in the spectrum are more apparent, but they do not arise from the action of the solar rays on the pure guaiacum.

M. Biot has shown that the matter which is soluble in water in this resin does not become coloured in the light, whilst that which is soluble in alcohol alone took the blue tint when it was subjected to this influence. Having therefore taken an alcoholic solution of guaiacum after this substance had been several times boiled in water, I covered a very white paper with it, which I exposed in the spectrum from two to three hours. The coloration in blue showed itself beyond the violet rays, as in fig. 8; the maximum was between M and N. These lines were feebly marked on the paper; it is probable that by leaving it longer exposed in the spectrum a stronger action would have taken place.

We know that in light there are rays which restore the guaiacum that had become blue to its primitive colour, which is yellowish. These rays, as Wollaston has shown, are situated towards the least refrangible part of the solar spectrum. Instead of acting upon the guaiacum rendered blue by the action of light, we operated upon some resin coloured blue by an aqueous solution of chlorine. Then, by exposing this paper in the spectrum, we had (fig. 9), after some hours, a white space extending from the red as far as the violet. This action arises from rays which act in an inverse manner to the preceding, the maximum of action being at F.

From all that I have shown in this section we may conclude, that for all the substances sensible to the action of the solar rays, the lines of the chemical spectra which act on these substances are the same as the lines of the luminous spectrum, when we only consider the corresponding spaces. Moreover, as generally happens for many substances, the chemical action extends much beyond the extreme violet. There are an infinite number of lines which are the same for all the spectra, and which we have represented at their respective distances (fig. 10).

The chemical action, in certain cases, may extend beyond the red, but it is very difficult to represent the lines beyond *a*, for they are very confused by projection; on the contrary, beyond the violet they are very numerous and very distinct. The mode of performing the experiment, then, makes us acquainted with the constitution of the solar spectrum beyond the visible parts, and that as far as a distance equal to the half of the length of the luminous spectrum.

We employed only a prism of very pure flint-glass in our experiments, but the effects would be the same if other prisms were used; the reason for this is as follows:—In the first place, Fraunhofer found that the nature of the lines in the luminous spectrum was independent of the substance of the prism, only their respective position changed a little from the greater or less dispersion of this prism. In the next place, in a memoir which I published on the electric effects accompanying the chemical action of light, and on the use of the electro-chemical actinometer, I showed that by employing a prism of glass, of water, of alum, or of rock-salt, results were obtained which varied but little relative to the chemical rays. Thus the conclusions which we have above announced are exact in all their generality.

§ III. *On the Phosphorogenic Spectrum.*

If there are any phænomena which excite the attention to a high degree when they are studied with care, these are certainly the phænomena of phosphorescence. Everything seems to prove that their origin is electric, and, in fact, most of the causes which disturb the molecular equilibrium of bodies seem to give rise to these phænomena. Thus all bodies which are non-conductors of electricity, if they are not phosphorescent after a short exposure to the solar light, are so by the action of electric light, when the discharge passes at a short distance from their surface; moreover a great number of minerals become luminous, some when their temperature is raised, others when they are broken, struck, &c. It is probable that in these different circumstances, at the time of action of external agents, the electricities essential to the constitution of the bodies separate when there is any disturbance in the position of the molecules, then reunite more or less rapidly so as to give rise to the flashes which are observed.

In this section, as we only treat of the action of the solar rays on phosphorescent bodies, nothing relative to bodies spontaneously phosphorescent, such as wood, fish, &c., will be included.

Amongst the substances which, after a short exposure to the solar light, become luminous in the dark, are Canton's phosphorus and the Bolognian stone; the one is the sulphuret of calcium, the other of barium. These two substances are the most luminous by insolation. There are some others, such as the melted chloride of calcium (Homberg's phosphorus), the melted nitrate of lime (Baldwin's phosphorus*), the sulphuret of strontian, &c.; but these bodies are less sensible than the preceding ones. We shall only attend to the two first, because they are sufficient for the researches we propose to make, and for the deductions we wish to draw from them.

The phosphorescent sulphuret of calcium is prepared in different ways; it is obtained directly by calcining crystals of sulphate of lime with powdered charcoal at a white red heat; this sulphuret thus prepared is luminous with a beautiful green light, after a previous exposure to the light. It may also be formed by calcining oystershells at a white red heat. In the same shells we thus obtain nearly all the prismatic colours. The two predominant tints are the orange and green. But the intensity of the phosphorescence may be increased by calcining these shells afresh with a mixture of per-sulphuret of potassium. They then, by means of insolation, generally present a very intense green phosphorescence.

The sulphuret of barium is also obtained with facility in the following manner: a paste is made with a mixture of sulphate of barytes and gum tragacanth, or with an organic mucilage, such as the white of egg, and this mixture is heated in a charcoal

* The nitrate of lime is decomposed when heated. The phosphorescent substance is therefore a mixture of lime, nitric oxide, and nitrous acid.

fire; after cooling, these pieces are luminous in the dark after having been exposed to the light. It is also prepared by calcining in bones the sulphate of barytes reduced to powder. In every case it is the sulphuret obtained which possesses this faculty of being luminous under the influence of the solar rays.

In general, all bodies which become phosphorescent by an elevation of temperature lose this property when they are kept a certain time at a temperature higher than the limit of temperature at which they become phosphorescent. The greater number regain this property by exposure to the solar light, others require being exposed to the influence of the light of an electric spark passing at a small distance from their surface. In the first class are the sulphurets of calcium, of barium, &c.; and in the second the crystallized minerals, as fluoride of lime, phosphate of lime, &c. We are thus able to foresee the following results, which are confirmed by experiment: the sulphurets of calcium and of barium, prepared as above mentioned, become phosphorescent in a very high degree by insolation; if when they have ceased to be luminous in the dark, which happens at the end of a quarter or half an hour, we raise their temperature, they cast a bright light during a few minutes, and are no more phosphorescent by means of heat unless by a fresh exposure to the light. This exposure need be but of very short duration, since the action of the light of an electric spark passing at some distance is sufficient to render them phosphorescent, as my father showed, and that the time which a spark lasts is $\frac{d}{v}$ (v being the velocity of electricity, or 700,000 leagues in a second, and d the distance of the two balls), a portion of time which does not exceed 0·0000000000025.

The solar rays exciting phosphorescence in these sulphurets, it is necessary to examine in what parts of the spectrum the active rays which have been termed *phosphorogenic* are situated. Some of the older natural philosophers, and especially Beccaria, said that the violet ray was the most apt and the red ray the least apt for exciting phosphorescence. Some experiments which M. Biot and my father made on the phosphorogenic action of the electric spark, gave for result that the phosphorogenic rays differ from the luminous rays. In reconsidering the action of the solar spectrum on phosphorescent bodies, I wished to observe if in the spectrum of the phosphorogenic rays, which I shall call the *phosphorogenic spectrum*, there were lines, as in the luminous spectrum and the chemical spectrum.

I have been led to an affirmative conclusion, that is to say, that the lines are absolutely the same in the spectra arising from these three radiations, at least taking into consideration only the parts of like refrangibility; but in making these experiments I have discovered new rays, of which I shall speak before describing the lines of the phosphorogenic radiation. We begin by reducing to powder the sulphuret which is to be used, for example the sulphuret of calcium; a layer of gum-arabic is put upon a sheet of paper stretched in a wooden frame, and this paper is powdered with the sulphuret; when it is dry we have a surface equally phosphorescent in every part. The paper thus prepared is placed at the focus of the lens which gives by projection the solar spectrum with all its lines. Let us for the moment leave the lines out of consideration; if, whilst the surface which is done over with the sulphuret of calcium receives the action of the spectrum, we keep our eyes shut, and then, having closed the aperture of the shutter, we look upon the surface, two luminous bands are visible, $\alpha\,\beta$, $\gamma\,\delta$, fig. 11. Pl. IX., separated by an interval which is not much illuminated. The part of the spectrum which gives the phosphorescence to the sulphuret of calcium, extends therefore from G to Z, beyond P. There are two maxima of action, one between G and H, but nearer to H, and the other at O.

In the upper part of fig. 11, which represents this effect, the light portions are white, and those which are dark are black. We see that they are as it were two phosphorogenic spectra. But this is not all; if we let the action continue during a certain time, a quarter of an hour for example, the diffused light which accompanies the spectrum impresses the remainder of the surface almost everywhere, so that by closing the aperture of the shutter again, we see nearly the whole surface luminous, the parts $\alpha\,\beta$, $\gamma\,\delta$ being most bright; but what is remarkable, is that the space from the line G to beyond A is completely dark. The lower part of fig. 11 represents the effect produced. This experiment seems to show the existence of rays in this part of the spectrum which act in an opposite manner to the phosphorogenic rays, that is to say which destroy the phosphorescence. I have succeeded in placing this fact beyond doubt by means of the two following experiments:—

Before exposing the phosphorescent surface to the action of the spectrum, we expose it during some seconds to the solar rays, or to the diffused light; this surface is then luminous in all its extent; but by projecting the spectrum during some minutes, reshutting the aperture of the shutter, then again looking at the surface, we see that it has remained luminous, with the exception of the part $\mu\ \nu$, which has become perfectly dark, and of the two parts $\alpha\ \beta$, $\gamma\ \delta$, which have increased in intensity. This part, $\mu\ \nu$, therefore contains rays which act in an opposite manner to the others, and which destroy the phosphorescence; besides, if we raise the temperature of this surface by heating the paper with a spirit lamp, *all the parts previously brilliant acquire, by elevating the temperature, an exceedingly vivid phosphorescence, whilst the part $\mu\ \nu$ remains completely dark.*

Thus the rays contained in this latter part not only take away the phosphorescence from the molecules, but they also completely destroy this faculty, so that, by the elevation of temperature, the parts on which these rays fall are no longer phosphorescent. These rays, with respect to the sulphuret of calcium, at their least degree of refrangibility begin between G and H, but nearer to G, and extend as far as ν beyond A. By the help of screens these latter rays may be rendered evident.

We know that glass coloured red by means of the protoxide of copper allows only red and orange-coloured luminous rays, and the chemical rays which accompany them, to pass; it was therefore natural to suppose that this glass would only let pass rays which act to destroy phosphorescence; and this in fact is what I have verified. If we take a sheet of paper prepared as before with a layer of powder of sulphuret of calcium, if we expose it to the light of day and bring it back into darkness, it will then be luminous; but placing immediately on this surface a card which only covers a certain part of it, then above a red glass, and again exposing the whole to the action of the solar rays during a minute at most, if we afterwards look at the paper in the dark, we see that all the part which had been sheltered from the action of the rays which have passed through the red glass is still very brilliant, whilst the remainder of the surface is become dark. The rays which pass through the red glass possess the property of destroying phosphorescence, as in the least refrangible parts of the spectrum; and we cannot say that they are the calorific rays, since these rays, on the contrary, possess the property of increasing the luminous faculty of bodies.

In the same manner I examined the action of the spectrum on the sulphuret of barium (Bologna phosphorus). By preparing a paper with the powder of this substance, and placing it as the preceding one in the spectrum, we soon see the part $\alpha\,\beta$, fig. 12, become luminous; but there is only one maximum, situated between the lines I and M; the limits of this spectrum of phosphorogenic rays are towards G on one part, and on the other towards P.

Moreover, if the whole surface is affected before the experiment, and it is then subjected to the action of the spectrum, we observe, as in the case of the sulphuret of calcium, that there is a space which becomes luminous from μ as far as ν, fig. 12. This part therefore contains rays which destroy the phosphorescence; they extend nearly as far as those which act on the sulphuret of calcium, and their effects are the same on these two sulphurets.

I wished to see if, in the spectra of the rays which act on phosphorescent bodies, there were lines as in the luminous and chemical spectra. In order to ascertain this, we are obliged to enlarge the spectrum by projection. That which had been before employed was half the size of those which are represented in the plates*; but when a spectrum of this size is employed, we cannot distinguish the lines on phosphorescent substances. Is this effect caused by the phosphorescence being propagated from molecule to molecule, as an experiment made by M. Biot and my father seems to show? I do not at all know. Whatever may be the cause, it is necessary to increase the spectrum. I augmented it ten times in length, or at least each part of the spectrum separately. This is easy to effect: instead of receiving the spectrum by projection at the focus of the great lens on a surface covered with a phosphorescent matter, we place in the path of the rays a lens having a focus of not more than one decimetre, in such a manner that the focus of the great lens falls between the focus and the surface of the second; then, somewhere on the other side of the latter, we have an enlarged spectrum; by moving the second lens in all the parts of the primitive spectrum, then seeking the place of the image by projection with a white card, we have a spectrum in which the lines are broad and very distinct; by receiving it on the surface covered with phosphorescent matter, we again find the same lines as in the luminous and chemical spectra.

In order to study them with advantage, we receive the spec-

[* N.B. The Plate which accompanies this translation is reduced to one-half the size of that in the original.—Ed.]

trum during a few seconds on the surface, then we again close the aperture of the shutter, and immediately raise the temperature to two or three hundred degrees; then the parts which have been affected by the phosphorogenic rays become strongly luminous, whilst the other portions corresponding to the dark lines remain perfectly black.

By employing greater or lesser spectra, or at least parts of the spectrum more or less enlarged, I have proved the existence of the same lines in the spectrum formed by these rays as in the luminous and chemical spectra; it is therefore useless to represent them; they are easily distinguished in the most refrangible rays, but more difficult in the less refrangible rays, for they are less distinct.

If attention be paid to the facts which I related when I began to speak of phosphorescence, before mentioning the lines, we see that with rays situated beyond the visible rays, that is to say with obscure rays, light is produced, since the sulphurets become phosphorescent under their influence, and that afterwards, in causing some of the luminous rays—red, orange, yellow, green, blue—to fall upon them, we destroy this faculty, and the bodies become dark.

§ IV.

According to the results which I have related in this memoir, we see that, by the chemical and phosphorogenic radiations which act upon any sensible substances whatever, if each part of similar refrangibility of these different spectra is considered, we find it traversed by the same lines or spaces without rays, as the corresponding part in the luminous spectrum. It is therefore probable that the rays of similar refrangibility are absorbed at the same time by the different substances through which they pass, and that the cause which occasions the absence of certain rays in the solar light is also that which produces the disappearance of these rays in the other radiations.

I have not yet quite resolved the question relative to the radiant heat and the determination of the lines in the calorific spectrum, but I am at present engaged on this subject, and I hope shortly to publish all the results to which these researches will have led me.

It has been generally admitted that these radiations which accompany light are different from each other, and that, according to such or such a sensible substance, the active rays were also different; but I do not suppose that the question is so complex. In fact, the luminous phænomena, according to the theory of undulations, depend on the vibrations of the molecules of the illuminating body, which are transmitted to the retina by the intermediation of the æther, the molecules of which are themselves in vibration. Fresnel, whose beautiful investigations have contributed to the triumph of this theory, had stated that the chemical effects produced by the influence of light are owing to a mechanical action exerted by the molecules of æther on the atoms of bodies, so as to cause them to assume new states of equilibrium dependent on the nature and on the velocity of the vibrations to which they are subjected. This idea had been suggested to him by a remarkable experiment of M. Arago's, the result of which was to show that the chemical rays which influence the chloride of silver interfere in the same manner as the luminous rays.

I think that the hypothesis of Fresnel is accurate, and even that it may be extended further, especially if we consider that the chemical and phosphorogenic rays possess the same physical properties as the luminous rays; thus they are subjected to the physical laws of reflexion, of refraction, of double refraction, of polarization, and of interference, in the same manner as are these rays; and moreover, as we have seen in this memoir, the spectra of these different radiations have the same lines. Thus it would be more simple to suppose,—

1st. That a pencil of solar rays is the union of an infinite number of rays of different refrangibility, each ray arising from undulations of æther not having the same velocity.

2nd. That by refracting a pencil of solar rays through a prism, we have the solar spectrum which possesses different properties on account of its different action on external bodies.

3rd. That with respect to certain substances the molecules of which are united by weak affinities, such as salts of silver, of gold, of mercury, &c., the solar rays act according to the velocities of undulation which may be transmitted to the molecules of matter, and consequently between certain limits of refrangibility. I have called the whole of the rays which affect a substance a *chemical spectrum*.

4th. That phosphorescent bodies becoming luminous by means of the molecular movement impressed on their molecules, a movement which gives rise to a separation of the two electricities necessary for maintaining the molecular equilibrium, and the neutralization of which forms the flashes we observe, we may consider the action of the solar rays on these bodies as analogous to that of these rays on bodies chemically sensible, with this difference only, that the mechanical action of the molecules of æther is transmitted to these bodies without chemical decomposition. According to their nature, therefore, these phosphorescent substances are sensible between certain limits of refrangibility in the solar spectrum.

5th. Besides, if we consider the retina as an organ which perceives the vibrations of the æther, it is only sensible to rays contained between certain limits of refrangibility, and the active rays form a spectrum, which in this case is found to be the luminous spectrum.

According to this hypothesis we shall bring back all the effects produced under the influence of light to the action of one same radiation upon different bodies, and there will be as many spectra as there are sensible substances. This mode of viewing the subject is verified on all the phosphorescent bodies, and on those whose molecular state changes under the action of the solar rays. As to the luminous rays, or those which act upon the retina, we can only judge of them by our own sensations; but it is probable that the retina of the different beings which exist on the surface of the globe are not all sensible between the same limits of refrangibility. We have some examples of this; amongst others of fish which live in the depths of the sea, and which see how to find their way where none of the rays which would be perceptible to our organs penetrate.

E. BECQUEREL.

N.B. M. Arago, after having communicated my memoir to the Academy, observed, that in Gœthe's 'Optics' a fact was related, the observation of which was due to Seebeck, and which is relative to the destruction of phosphorescence by certain rays. This natural philosopher has proved that the red rays can extinguish phosphorescence in some bodies which are endowed with this property.

Article XXIII.

Considerations relative to the Chemical Action of Light. By M. Arago.

[From the *Comptes Rendus*, No. 17, for Feb. 13, 1843.]

A LETTER from M. Edmond Becquerel gave rise to a verbal communication from M. Arago, which we shall give as faithfully as possible.

A short time after the vote of the legislature which awarded a national recompense to MM. Daguerre and Nièpce, very erroneous opinions, according to my idea, were manifested by a small portion of the public, which however imposed on me the duty of showing that the new discovery should not be considered merely in an artistical point of view, but that it would enrich physics with very valuable means of investigation. Such was the intention of a Note which appeared in the *Comptes Rendus* of the sitting of the 19th August, 1839. It was worded as follows:

"Here is an application of which the Daguerreotype will be susceptible, and which appears to me very worthy of interest.

"Observation has shown that the solar spectrum is not continuous, that there exist transversal solutions of continuity, lines entirely black. Are there similar solutions of continuity in the obscure rays which seem to produce the photogenic effects?

"If there are any, do they correspond with the black lines of the luminous spectrum?

"Since several of the transversal lines of the spectrum are visible to the naked eye, or when they are painted on the retina without any amplification, the problem which I have just laid down will easily be resolved."

This very easy solution of the problem which I had proposed I was not able in 1839 to search for experimentally myself, the old camera obscura of the observatory being at that time destined to another purpose, and the new one was not yet constructed. However, I must suppose that my appeal was attended to. I have learnt in fact that the Royal Society received a memoir the 20th February, 1840, of Sir John Herschel's, where the question is glanced at, and every one here remembers that M. Edmond Becquerel brought this same subject before the Academy at the meeting of the 13th of June, 1842. Sir John Herschel not having been able to fit up a heliostat, thought that he ought not to state positively the existence of the lines in the photographic image of the spectrum. M. E. Becquerel, on the contrary, projected on his iodized plate a stationary spectrum, and saw distinctly, after the experiment, in the region of the plate which this spectrum occupied, transversal lines, along which the chemical matter had remained untouched, or at least had not received any perceptible modification. He also recognised that these lines corresponded exactly to the dark lines of the luminous spectrum.

At first sight, the experiment which I have just mentioned might have seemed superfluous; in short, was not the result obtained necessarily true? How should we expect photogenic actions where light is entirely wanting?

My reply is this: It is by no means proved that the photogenic modifications of sensitive substances result from the action of solar light itself. These modifications are perhaps engendered by invisible radiations mixed with light properly so called, proceeding with it and being similarly refracted. In this case the experiment would prove, not only that the spectrum formed by these invisible rays is not continuous, that there are solutions of continuity as in the visible spectrum, but also that in the two superposed spectra these solutions *correspond exactly*. This would be one of the most curious, one of the most strange results of physics.

Let us introduce into the discussion an element depending on the velocity of light, and the consequences of the observation will not be less interesting.

Many years ago I showed that the rays of the stars towards which the earth advances, and the rays of the stars from which the earth recedes, are refracted exactly in the same degree. Such a result cannot be reconciled *with the theory of emission*, but by the help of an important addition to be made to this theory, the necessity of which presented itself formerly to my mind, and which was generally well received by philosophers, we must admit that luminous bodies emit rays of every degree of velocity, that rays of a determined velocity alone are visible, and that they alone produce the sensation of light in the eye. In the theory of emission, the solar red, yellow, green, blue and violet are respectively accompanied by similar rays, but invisible from deficient or excessive velocity. To a greater degree of velocity corresponds a less degree of refraction, as less velocity is attended with a greater refraction. Thus each visible red ray is accompanied by obscure rays of the same nature, some of which are refracted more, some less than it: thus *there exist rays in the black lines* of the red portion of the spectrum: the same thing should be said of the lines situated in the yellow, green, blue and violet portions. Experience having shown that the rays contained in the lines are without effect on sensitive substances, it is established that all increase or diminution of velocity takes away from the luminous rays the photogenic properties with which they were before endowed; that the solar rays cease to act chemically at the same instant when by a change of velocity they lose the faculty of producing luminous sensations on the retina. I need not point out how much there is that is curious in a chemical mode of action of light depending on the velocity of the rays.

The same Monday on which M. E. Becquerel presented the Academy with the result of the experiment which I had proposed two years and ten months before, I publicly invited him to recommence, imposing upon himself new conditions, which seemed as if they must help to explain the manner in which the velocity modifies the chemical action of light. I made the remark, that the solar rays moving quicker and quicker, according as the media through which they pass are more refractive, some useful result would be attained by studying, comparatively and simultaneously, the action of the spectrum on the iodized plate, the halves of it being plunged into two very dissimilar media, for example into water and into air. M. E. Becquerel readily complied with this idea. The following is the letter which he wrote to me on the 25th November, 1842:—

"When you were so kind as to present my 'Memoir on the Constitution of the Solar Spectrum' to the Academy of Sciences last June, you pointed out an experiment to be made for the purpose of knowing if, when a substance sensible to the action of the solar rays is plunged in any other medium than air, the change of velocity of the solar rays, at the transition from the air into this medium, did not alter the position of the transversal lines of the spectrum of the chemical rays.

"I immediately hastened to make these experiments, beginning by using water as a new medium. My departure for the country compelled me to interrupt them. I expected to resume them at my return, before acquainting you with the result, but the badness of the season has not allowed me to fulfill my intention; I have nevertheless the honour of sending you the result of two experiments which I have made, with the description of the process I have followed.

"I made use of a small glass reservoir, the sides of which were very flat, and of a plate prepared according to M. Daguerre's method, which could be placed vertically in the reservoir so that its surface should be parallel with the front face of the reservoir. In these two experiments the distance between the iodized plate and this face was one centimetre. A pencil of solar rays is then introduced into a dark chamber through a narrow aperture made in the shutter; these rays are refracted through a prism of very pure flint-glass, before which is placed a lens having a long focus, so as to obtain a solar spectrum by projection with all its lines. This result once obtained, I placed the reservoir before the path of the refracted ray, so that the spectrum is depicted horizontally with all its lines on the iodized plate and so that the violet rays enter perpendicularly at the anterior face of the reservoir. Before beginning the experiment care had been taken to pour water into the vessel until its surface cut the image of the spectrum longitudinally into two equal parts.

"If the plate be taken away after one or two minutes of action, by exposing it to mercurial vapour the image of the spectrum is seen depicted from the limit of the green and of the blue to beyond the extreme violet, and, as I said in the Memoir, this image has all its lines similar to those of the luminous spectrum as regards the portions of the same refrangibility. Well! no perceptible difference is to be seen between the image of the spectrum on the portion of the plate which remained in the air and that which was formed on the portion which had been kept in the water; the lines of these two portions of spectrum seem very exactly to be prolongations of each other, excepting, however, in the extreme portions of the chemical spectrum towards the right and the left, where the lines of the image which was produced in the water seemed to become a little closer to each other. This it appears to me should be attributed to the refraction of the oblique rays.

"These two experiments tend to show that the nature of the medium into which the substance chemically sensitive to the action of the solar rays is plunged, does not modify the action of these, so that the impression of the solar spectrum on this substance always presents the same lines and at the same places.

"As soon as I have time I intend to make these experiments again, to vary them, and perhaps I shall come to more conclusive results. I have the honour to be," &c.

Here then are the solar rays acting exactly in the same way in air and in water. In air, however, according to the system of emission, light moves much less quickly than in water. The velocity here, therefore, is without influence, a result which at first sight seems in manifest contradiction to what we deduced from the first experiment. The two results however may be reconciled. A new hypothesis may, it seems to me, make them agree; but every one will form his own judgement about them.

The velocity with which a luminous ray *passes through* a given body depends exclusively on the refringency of this body and on the *velocity of emission of the ray*, on the velocity it had *in vacuo*. The ray which reached the surface of the stratum of iodine through the water at the point where it meets this surface, possesses a velocity superior to that which the ray that moved through the air had at the same point; but *in the interior itself of the stratum*, at a sufficient depth, the two rays possess exactly the same velocity. Let us make the photogenic phænomena depend not upon an action exerted at the surface, but upon an action originating in the interior of the stratum, and every difficulty disappears; only,—a singular result,—we are compelled to establish an essential distinction between the interior and the surface of a stratum, the thickness of which is incredibly small.

By thus considering the photogenic phænomena as examples of *molecular* actions susceptible of exact estimation, every one will feel how interesting it would be to intercalate figures in the general arguments which I have just offered. We shall attain this object by first completing the experiments by help of which M. Dumas had begun to determine the thickness of the stratum of iodine on which the Daguerrian images are formed from the comparative weight of a large silvered plate before and after its iodation. Afterwards as much accuracy as possible will be used in observing the relative positions of the dark lines traced on the sensitive matter, even by using the microscope if necessary; lastly, instead of making a sudden transition from air to water, we shall compare the relative positions of the lines produced in two media slightly different in density or in refrangibility. *In the system of emission* the following consequences are rigorously deduced from the discussion into which I have just entered:

If the photogenic effects of solar light result exclusively from the action of obscure rays mixed with the visible rays, proceeding like them and with velocities of the same order, the superposed spectra of these two species of rays have their solutions of continuity exactly at the same places;

If the visible rays produce the photogenic effects totally or in part, this property is so inherent in their velocity that they equally lose when this velocity increases and when it diminishes;

The photogenic effects of solar light, whether they proceed from visible or invisible rays, cannot be attributed to an action exerted at the surface of the sensitive stratum; it is in the interior of the substance that the focus of this kind of action is to be sought for.

The preceding conclusions may be extended when we know the thickness of the least stratum of iodine in which the Daguerrian phænomena are produced, when it is possible to compare this thickness to the length of the *fit* or to that of the luminous waves.

Article XXIV.

On the Action of the Molecular Forces in producing Capillary Phænomena. By Prof.* Mossotti.

1. THE application of the theory of the molecular forces to the explanation of the effects of what is called capillary attraction, constitutes one of the most delicate branches of physical mechanics. The capillary phænomena set forth in the first instance by Hauksbee before the Royal Society in London, were theoretically discussed by the said Hauksbee†, by Newton‡, by Jurin§, by Vietbreckt‖, by Segner¶, by Clairaut** and others. Dr. Young, one of the most sagacious minds that modern times have produced, was the first to give a correct theory of these phænomena, though incomplete in some of the fundamental principles††. Dr. Young's theory was thrown into the shade by the more brilliant but less accurate theory of Laplace, which appeared a short time after‡‡. Finally, Poisson, in his *Nouvelle Théorie de l'Action Capillaire,* has remedied the defects which still remained in Laplace's theory, by deducing the explanation of the capillary phænomena from an accurate investigation of the action of the molecular forces that produce them. Poisson's theory, however, is founded on very abstruse analysis, which cannot well be translated into language suited to the comprehension of more common understandings. I hope therefore that I shall not be doing an unacceptable thing, if, starting with

* Translated from the Italian, and communicated, at the Author's request, by E. H. J. Craufurd, B.A., Trin. Coll., Cambridge. This Article is one of a course of lectures on Natural Philosophy delivered by Mr. Mossotti when Professor in the University of Corfu. It was published first in the *Biblioteca Italiana*, and afterwards reprinted in the *Nuovi Annali delle Scienze Naturali*, together with a Note by the same author on a Capillary Phænomenon observed by Dr. Young, which forms the subject of Article XXIV. of this Journal.

† Hauksbee. *Sperienze fisico-meccaniche.* Firenze, 1721.

‡ *Newtonis Optices.* Quæstio 31.

§ *Leçons de Physique Expérimentale par Côtes*, p. 410 *et suiv.*

‖ *Tentamen Theoriæ qua ascensus aquæ in tubulis capillaribus explicatur.* Comm. Acad. Petrop. tom. viii. et ix.

¶ *Commentarii Soc. Reg. Scientiar. Gottingensis.* Tom. i.

** *Théorie de la Figure de la Terre*, p. 105, *et suiv.*

†† Young. An essay on the Cohesion of Fluids. Phil. Trans. Dec. 20, 1804.

‡‡ *Supplément au deuxième livre de la Mécanique Céleste, et Supplément à la Théorie de l'Action Capillaire*, vol. iv.

the latest notions which we have already expounded on the real structure of fluids, I shall attempt to explain the capillary phænomena with the ingenious ideas set forth by Dr. Young.

2. The capillary phænomenon which can most easily be observed, is produced by immersing in a fluid a slender tube of small diameter (from about $\cdot 5^{mm}$ to 3^{mm}). If the liquid be such as to wet the sides of the tube, the small column of fluid will be seen to assume a concave shape at its upper surface and to rise to a greater height than the liquid outside the tube; whereas if the liquid be of a kind that will not adhere to the sides, the small internal column will assume a convex shape at the upper surface, and will stand at a lower height. Comparing the elevations or the depressions of the small columns of fluids in tubes of different diameters, we find that they vary approximately in the inverse ratio of the diameters of the tubes employed. It is from the minuteness of the diameters of these tubes, which may be compared to a hair, that these and other phænomena depending on the same causes have been called capillary phænomena.

It is not necessary that the fluid that rises above or is depressed below the level of the external portion should be entirely enclosed as by the sides of a tube. If we immerse two planes at a small distance from one another, the liquid will be seen to rise or be depressed between them; but in this case the elevations or depressions are only about half the amount of those produced by a tube of a diameter equal to the distance between the two planes.

3. It would appear at first sight that these elevations or depressions were an exception to the general principles of hydrostatics which we have explained, and according to which a liquid ought to rise to the same level in all communicating vessels; but in giving that demonstration we did not consider a peculiar circumstance, which did not concern us then, but which, if now taken into account, will show us clearly that these variations of level, instead of being an exception, are a direct consequence of the principles according to which we have characterized the molecular forces, which have led us to discover the transmission of equal pressure in every direction. (*Vide* note 1, p. 575.)

We then showed, that if we suppose a plane passing through the liquid mass, and upon this plane a small fluid prism perpendicular to it, and whose height is equal to the distance at which the molecular forces act, if the liquid be not acted on by any external pressure, its molecules are situated at such a distance from one another that the sum of the respective repulsions of the molecules upon the opposite side of the plane on those of the nearer portions of the small prism, is exactly equal to the sum of the respective attractions of the same molecules of the fluid upon the opposite side of the plane on those of the further portions of the small prism: hence the small prism has no tendency either to press on the plane or to move from it, and the fluid throughout is in equilibrium and exerts no pressure. This holds for every portion of the fluid situated at a distance from the surface greater than that to which the molecular forces extend; but if we suppose a plane intersecting the fluid parallel to its surface, which we shall now suppose horizontal and infinite, at a depth less than the distance of molecular action, and if we suppose a small prism perpendicular to the plane upon the side towards the external surface, this prism not being of sufficient height, will not contain a sufficient number of more distant molecules to counteract the repulsive action of those nearest the plane, consequently there will be an excess of repulsion on the former molecules, and they will tend to separate from each other. The separation of the molecules will be so much greater as the plane is nearer the surface of the liquid, so that on approaching this surface we shall find a rapid decrease in density, regulated by the law that the repulsive action of the fluid beneath the plane on the molecules of the portion of the prism between the plane and the surface, should be always counteracted by the attraction of the parts reciprocally more distant, so that the pressure remain zero for every plane.

The depth of the stratum in which this rapid decrease in density will take place will be very small, since the molecular action extends only to inappreciable distances; but we may suppose it divided into several very thin strata, in each of which the density may be considered uniform, that is the molecules may be considered equidistant one from the other. Now while near the surface the equilibrium of the molecules in a vertical direction requires that the density of the fluid shall decrease rapidly, the equilibrium in a horizontal direction will remain unaltered, although the molecules are distributed with a uniform density in each stratum, since each molecule will be situated in the midst of a number of horizontal forces all equal, arising from the molecules by which it is surrounded. But the existence of this individual equilibrium of the molecules, depending upon their uniform distribution, does not imply that the horizontal attraction between the different parts of the fluid is zero. On the contrary, since we find the distances between the molecules in the upper strata greater than when the fluid is in a natural state, or greater than in the interior of the fluid where the pressure is zero, it follows, by the principles of hydrostatics (see note 1, p. 575), that if through any point in the surface we draw a vertical plane, a thread of molecules perpendicular to this plane, situated in one of the above strata, and equal in length to the distance of molecular action, will be attracted toward the plane: hence at every point along the surface of the fluid a mutual attraction will take place between the parts, and this will produce a sort of force of contraction at the surface, which Segner, Monge and Young had indeed foreseen, though they could not accurately explain the cause.

4. Let us now suppose the liquid surface to be limited, and to terminate at two opposite extremities in two planes perpendicular to it and formed of some solid substance. If the action of either of these planes on a horizontal prism of the fluid, equal in height to the range of the sensible molecular action, were equal to the action of the fluid, it is clear that no change would take place near that plane; but in general the action of the plane on the fluid is different from that of the fluid. If it be less, the surface of the fluid, in consequence of the force of contraction, will detach itself from the plane; and if it be greater, the fluid will be attracted and compressed to the plane and will rise along it. Let us consider these two cases severally.

In the first case, the fluid detaching itself from the plane will, near the points where it detaches itself, increase its free surface, along which an equal force of attraction will successively be produced; and if the action of the solid plane on the liquid be zero, the separation of the fluid from the plane would cease when the cylindrical and free surface of the fluid had become convex to such a degree that the plane should be tangent to it; below that point the liquid would remain in juxtaposition with the plane, and would possess a force of attraction along it equal to that of its free surface, since we suppose the plane to have no action on the fluid. If, on the contrary, the plane exerts a small action on the fluid, the attraction along the liquid surface contiguous to the plane will be less, for in that part the fluid will be less rarefied, and we can easily see that it will not detach itself from the plane until the resolved part of its attraction, acting in the vertical at the free surface, shall be equal to the attraction along the surface touching the plane. These two forces will then counteract each other, and the free surface will join that which is in contact with the plane at an angle which, as we shall see presently, remains constant for the same fluid, whatever be the solid substance employed.

What takes place on one side near the surface of one of the two planes must happen equally at the opposite side near the other plane. Thus the cylindrical free surface of the fluid will be as it were united at its extremities to the two plane surfaces in contact with the solid planes; and since a force of contraction exists along them and at their points of conjunction with the free surface, this surface will be drawn downwards and will compress the fluid beneath; and if the two planes are very near, the effect produced will be very sensible, and the liquid will descend between the two planes, below the level of the external liquid, until the above-mentioned forces of attraction be counteracted by the increase of pressure which the fluid, at a higher level without the planes, exercises in consequence of its gravity.

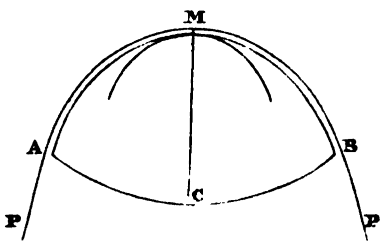
In the second case, the attraction of the solid planes on the fluid in contact with them being greater than that of the fluid on itself, the fluid in contact with the planes will be compressed and will rise along the surface of the planes, which will thus be covered with a fluid sheet, which on either side will be united below with the free surface of the fluid. The two parts will together form a continuous free surface, concave externally, tangent on either side to the planes, and along which there will be a force of attraction. This force at the two opposite extremities will draw the concave surface of the liquid upwards in a vertical direction, and will tend to detach it from the liquid underneath; the contiguous particles at the lower points will therefore become slightly more separated from one another, the adjacent fluid will hence acquire a force of tension of its own, and will follow the ascending motion of the free surface. When the weight of the raised column of fluid shall counteract the force of tension of the two lateral sheets, the motion will cease and there will be equilibrium.

The object of the capillary theory is to determine the conditions of equilibrium of a fluid in these and in other similar cases.

5. To give some idea of the manner in which these conditions must be considered, we must premise some notions on the properties of curved surfaces subject to any pressure or tension. It is proved in statics, that if any surface be acted on at every point by forces normal to it, it will suffer a constant pressure or tension in every part, and the force which acts at each point is equal to the product of this tension by the sum of the inverse values of the radii of maximum and minimum curvature, or in general of the radii of curvature of two sections at right angles.

To explain this proposition by an example, suppose that on a solid cylindrical surface a piece of cloth or any flexible surface be kept stretched by means of forces applied at its extremities perpendicular to the axis and tangent to the surface. It will be sufficient in this case to consider the equilibrium of a single zone or section made perpendicularly to the axis, since what is said of this section will be equally applicable to any other, and hence to the whole cloth. Let A M B then (fig. 1) be this section, P the tangential forces applied at A and B, that keep the corresponding belt of cloth stretched:

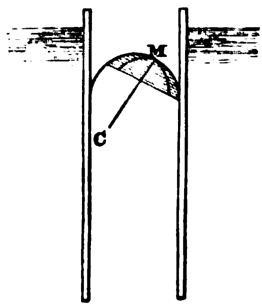
Fig. 1.



since this belt can exert only a normal pressure on the arc beneath it, its tension must be constant along its whole length and equal to P, and the force of pressure which it exerts on any point M of the arc A M B will be in the inverse ratio of the radius C M of the osculating circle of the curve at the point M; the other radius of curvature being in this case infinite, its inverse value is therefore zero.

The tension of this belt may serve to represent an image of the force of contraction of the surface of a fluid in a section contained between two solid planes parallel and very near to each other, and made perpendicular to them. As the molecular

Fig. 2.



action extends only to inappreciable distances, if we suppose that at any point M (fig. 2) of such a section, and at a sensible distance from the planes, an osculating circle be described, all the molecules which in the same section exert a sensible action on a thread of fluid normal to the surface at the point M may be considered as contained within this circle; and since the density along the surface at a sensible distance from the planes varies only by insensible degrees, the resultant of the actions of all these molecules will be in the direction of the radius of curvature C M, that is normal to the surface, since everything may be supposed symmetrical on either side. Hence it follows that the tension of the fluid along the surface, arising from the greater rarefaction of the molecules, will be the same at every point, and the resultant of the excess of the attraction above the repulsion between the parts of the fluid will be in the inverse ratio of the radius of curvature.

Nor can the tension be different at different surfaces of the same fluid, since as the molecular forces act only at insensible distances, the resultant of the forces corresponding to any one point of the surface cannot vary as long as the position of the fluid about that point continues the same, that is to say as long as the radius of curvature does not change. In order then that the tension may be the same for two surfaces, it will suffice that they meet so as to have for one of their points of intersection two equal radii of curvature. The tension is therefore independent of the nature of the surface, and is equal to that which we have found to exist in a plane surface. We shall call T the value of this tension.

6. Now let us consider the case in which the fluid is depressed between the two planes. In this case the action of the substance of the planes on the molecules of the liquid must be less than that of the fluid on itself. If the substance of the planes exerted an equal action, the fluid in contact with them would have the same density as it has internally; if their substance exerted no action, the fluid along the planes would be equally rarefied with the free surface. The action of the planes being intermediate between these two limits, the contiguous stratum of the fluid

will acquire an intermediate degree of rarefaction, and will therefore be endowed with an intermediate force of tension. If we call Θ the diminution in the force of tension, produced on the contiguous stratum of fluid by the action of the planes, $T - \Theta$ will be the expression for the force of tension of that stratum.

At the point of junction of the free surface with the surface contiguous to the planes, the passage from one to the other will be along a curve of very great curvature. The resultant of the attractions on a small prism in the free surface will not be any longer normal to it, since this resultant will be influenced by the action of the planes, and the tension will pass rapidly from the value which it has at the free surface to that which it has along the planes. At a scarcely perceptible distance from the extremities of the arc of junction the forces will again act in a direction normal to the surface, and the two tensions will be constant. Now since the resultant of the actions of the planes upon each molecule of the arc of junction is clearly always perpendicular to those planes, and on the other hand the internal part of the fluid varies in density only so as to resist the actions which take place at its surface, we may compare the equilibrium of the arc of junction to that of a portion of catenary of variable density acted upon by gravity in a direction perpendicular to the planes, and we know that in this case the tension at the lowest point, and the part of the tension at the extremity of the curve resolved perpendicular to the direction of gravity, must be equal for equilibrium. The part of the tension at the free surface resolved vertically must therefore be equal to the tension at the stratum contiguous to the planes; and if we call ω the angle between the planes and the tangent to the free surface at the upper extremity of the arc of junction, we shall have for our first equation

$$T \cos \omega = T - \Theta. \quad \ldots\ldots\ldots\ldots \quad (\alpha.)$$

Now $T - \Theta$ being constant for the same liquid and for planes of the same substance, ω must also be constant, whatever be the free surface of the fluid.

A force of contraction equal to $T - \Theta$ will act also on the other side contiguous to the other plane, and the free surface will be drawn down by these tensions until the hydrostatic pressure arising from the weight of the fluid at a higher level externally be such as to counteract them.

If we call P the weight of fluid that would fill the space between the planes up to a level with the external fluid, that is the weight which would balance the external pressure, this will measure the two vertical tensions, and calling σ the thickness of the section along the length of which the tension $T - \Theta$ acts, we shall have

$$P = 2(T - \Theta)\sigma = 2T\sigma\cos\varpi \;\ldots\ldots\ldots \quad (1.)$$

7. The second case, in which the action of the planes on the liquid being greater than that of the fluid on itself, the fluid is compressed and rises along the planes, is easier to consider. The fluid stratum which covers the planes forms a continuation of the rest of the free surface of the fluid, which thus on either side becomes tangent to the planes. As that stratum has always a thickness greater than the inappreciable distance at which the molecular forces act, its external surface rapidly decreases in density and acquires a tension equal to that of the free surface. Thus on either side the free surface is acted on by a vertical tension which raises it up. As it rises the molecules beneath become rarefied, they acquire a force of tension owing to the rising of the free surface, and they follow its motion, which ceases when the weight of the raised column of fluid counteracts the two lateral tensions. If then we call P the weight of this column, we shall have

$$P = 2T \,.\, \sigma. \;\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

8. We can now, from the equations (1.) and (2.), deduce the experimental law which we enunciated at the beginning, that the elevations or depressions of the same fluid between two planes are in the inverse ratio of the distances between the planes; for let d be the distance between the planes, a the depression or elevation of the fluid between them below or above the external level, and since the distance d is supposed very small, and the weight of the fluid which would fill up the convexity or concavity of the upper extremity of the column may be neglected, if we call g the force of gravity and Δ the density of the fluid, the weight P will be expressed approximately by $g \,.\, \Delta \,.\, \sigma \,.\, d \,.\, a$, and equations (1.) and (2.) will thus give

$$g\Delta\sigma d a = 2(T - \Theta)\sigma = 2T\sigma\cos\varpi,$$

$$g\Delta\sigma d a = 2T\sigma,$$

whence we get

$$a = \frac{2(T-\Theta)}{g\Delta} \cdot \frac{1}{d}, \qquad a = \frac{2T}{g\Delta} \cdot \frac{1}{d}.$$

The coefficient of $\frac{1}{d}$ being constant in all cases for the same liquid, and for planes of the same substance, the depressions or elevations a will be approximately in the inverse ratio of the distances between the planes.

9. Equation (α.) is one of those which mathematicians call an equation at the limit, and holds for the circumference of the free surface. To obtain the equation corresponding to any point of that surface, let us consider the equilibrium of a small cylindrical fluid column, which from the external surface proceeds to a greater depth than the two planes, then bends and rises vertically between the two planes at a sensible distance from them (*vide* figs. 3 and 4). As soon as it is near the surface suppose

Fig. 3.

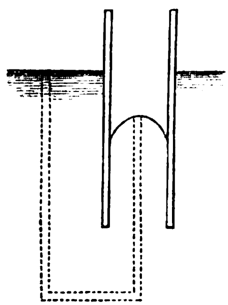
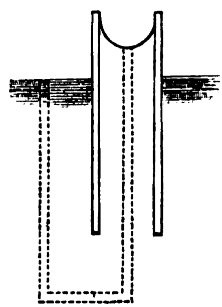


Fig. 4.



the column turns so as to terminate at the surface normally to it. The pressure on the external surface being supposed zero, the described column will not be subjected to any pressure at its extremity in this plane surface. The action of the molecules of the internal fluid which forms the channel in which the column is enclosed will also be zero till it comes within the neighbourhood of the internal surface, since if we suppose this channel divided into so many rings, each ring will exercise two equal opposite forces on the mass of the fluid column. Thus, if we omit the consideration of the action at the free surface within the planes, the fluid column suffers only the hydrostatic pressure arising from gravity; and if we call z the difference in level between the internal and external extremity of the fluid column, s the area of a section, Δ its density, the column will be urged by a force $g \Delta s z$ upwards or downwards, according as the height of the liquid outside is greater or less than that of the liquid between the planes. Now we have seen that the attraction of the molecules in the free surface within the planes, combined with its curvature, gives rise to a force which urges the portion of the fluid column normal to the said surface inwards or outwards, according as the surface is convex or concave. This force is measured at every point by $\frac{\mathrm{T}}{\varrho}$, ϱ being the radius of curvature. For the equilibrium of the column then we must have the equation

$$g \Delta s z + s \frac{\mathrm{T}}{\varrho} = 0.$$

The ordinate z being reckoned positive, when measured upwards from the external level, we must take ϱ positive or negative, according as the free surface is concave or convex.

The considerations which have led us to this equation do not depend on the supposition that the surface is cylindrical: if they extend therefore to the more general case of any surface whatever, keeping in mind that then the force perpendicular to the internal surface, which acts on the portion of the fluid column normal to it, is measured by the tension multiplied by the sum of the inverse radii of curvature of two sections perpendicular to each other, we shall have, calling ϱ' the second radius of curvature,

$$g \Delta s z = s \,.\, \mathrm{T} \left(\frac{1}{\varrho} + \frac{1}{\varrho'} \right).$$

If we put $\frac{\mathrm{T}}{g \Delta} = \frac{\tau^2}{2}$, τ being a constant quantity for every fluid, the above equation may be put under the simpler form,

$$z = \frac{\tau^2}{2} \left(\frac{1}{\varrho} + \frac{1}{\varrho'} \right). \quad \ldots\ldots\ldots\ldots \quad (\beta.)$$

The two formulæ (α.) and (β.), of which the former refers to the circumference, the latter to any point of the free surface, form the basis of the whole theory of capillary action. The application of these equations to the various cases requires only some processes of integral calculus, of no great difficulty to any one who has acquired practice in it. As we do not wish to do more than set forth the mechanical principles on which the theory is founded, and give a precise idea of the manner in which capillary phænomena are produced, we shall only add (note 2, p. 576) the formulæ which Poisson has deduced for some principal cases in his *Théorie de l'Action Capillaire*, in order that the reader may find them ready should he wish to apply them.

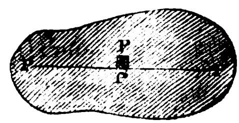
Note 1.

As the passage of the Lecture on Hydrostatics, which is here mentioned, contains the fundamental idea of the equilibrium of fluids, as conceived by Poisson, and is the key to the internal mechanism by which the molecular actions at a distance act so as to resist external pressures or tensions, I will transfer it here for the convenience of the reader.

"If we consider bodies as a collection of molecules that keep one another in stable equilibrium at given distances in consequence of forces that are repulsive at a small and attractive at a greater distance, and all of which produce a sensible action only within the limits of inappreciable distances, fluids differ from solids inasmuch as the forces which each molecule exerts on the others are, on account probably of the space between them being greater, independent of the position of the axes of its figure. These forces consequently act equally all round each molecule, and vary only with the distance; and in order that a fluid not acted on by external forces may be in equilibrium to a sensible depth within it by the action of the molecular forces alone, that is to say in order that any molecule whatever may be always in the midst of a number of forces acting symmetrically, and not be attracted or repulsed more in one direction than in another, the molecules must all be uniformly distributed about each other, and consequently the density of the fluid must be uniform.

"To conceive how in a mass of such a fluid a pressure or a tension can exist, let us suppose a plane (fig. 5) drawn through it, and upon an element of this plane and perpendicular to it a small prism of the fluid, of which the height be equal to the distance, to which extends the sensible action of the molecules on the other side of the plane. Equilibrium will not be destroyed if we suppose for an instant that this prism become solid. The sum of the actions of the molecules on the other side of the plane on the molecules of the small prism will vary according as the fluid is in a state of pressure or of tension. If the molecules are at such a distance that the repulsive action of the fluid on the other side of the plane on the molecules of the prism which are respectively nearest be equal to the attractive action of the molecules which are respectively furthest from one another, the prism will not be either repulsed from or attracted towards the plane; and in this case the fluid is in a natural state, not subject to any pressure or tension. If the fluid be compressed, its molecules are, however, imperceptibly condensed, and since, in consequence of this condensation, the repulsive forces between the molecules respectively nearest increase in a greater ratio than the attractive forces between the molecules which are respectively furthest from one another, the prism will be repulsed; this repulsion resists the pressure that tends to push it through the plane, and thus this pressure is counteracted by the action of the fluid itself*. If the fluid be acted on by a

Fig. 5.



* Those who are acquainted with the differential and integral calculus will here see the reason why, in estimating the resultants of the molecular forces, it is not allowable to substitute the integrals for the sums of the mutual actions of the molecules. For if we were to consider the fluid as a continuous mass, for every increase or diminution of its density the resultants of the attractive and repulsive forces on the small prism would all increase or diminish in the same proportion, viz. as the square of the density, and there would never be an excess of repulsion or attraction to counteract the pressure or tension to which the small prisms were subject. This will not be the case if we consider the mass as discontinuous and formed of separate molecules. The repulsions and attractions of the molecules being functions of their distances, any change in the distances between the molecules will have a much more perceptible effect on the sum of the actions of the molecules respectively nearest to each other, which are those that repel one another, than on the sum of the actions of the molecules respectively furthest from each other, which are those that attract one another. Consequently the repulsive force acting on the small prism will be either greater or less than the attractive force, according

tension its molecules are separated; as the attractive forces are removed to a greater distance, they are counteracted by the repulsive forces, but these decrease more rapidly than the former in proportion to the increased mutual distances between the molecules, and in consequence of this excess of attraction the fluid destroys the action that tends to separate the prism from the plane. This last excess (of the attractive over the repulsive force) is always very small in fluids, since they do not offer much resistance to division; still it exists, and various phænomena are known in which fluids show a sensible force of attraction before they are separated from the rest of the mass.

"From these considerations we must conclude, then, that in every fluid there exists between the molecules a certain space, in which the different parts of the fluid neither attract nor repel each other, and in which the fluid does not undergo any pressure or tension whatever, but is constituted in what we call a *natural state*. If this space be diminished, the parts of the fluid repel one another mutually, and sustain a pressure; *vice versâ* if it be increased, the parts of the fluid attract one another mutually, and counteract a tension."

Note 2.

Formulæ for calculating some Phænomena of Capillary Action.

1. The height a, to which a fluid rises between two vertical planes parallel and close to one another, and wetted by the fluid, is given by

$$a = \frac{\tau^2}{2r} - r\left(1 - \frac{\pi}{4}\right),$$

$2r$ being the distance between the planes, and π the ratio of the circumference to its diameter.

2. The depression $-a$ of a fluid between two planes, as above, but not wetted by the fluid, is given by

$$-a = \frac{\tau^2}{2r} - r\left(\frac{3}{4}\sin 2\omega + \frac{\omega}{2} - \frac{\pi}{4} - \cos\omega\right),$$

ω being the angle of contact between the fluid and the planes, measured by the angle formed by the normal to the external surface of the fluid, and the perpendicular to the nearest plane.

3. The height of a fluid in a small cylindrical and vertical tube is expressed by

$$a = \frac{\tau^2}{r} - \frac{r}{3},$$

r being the radius of a horizontal section of the tube.

4. If the tube be rather wide, so that $\frac{\tau^2}{r}$ be a fraction, then we have

$$a = \frac{4\sqrt{\pi\sqrt{2}}\,.\,\sqrt{\tau l}}{1+\sqrt{2}}\, e^{\frac{-l\sqrt{2}}{\tau}},$$

where

$$l = r + (\sqrt{2} - 1)\tau.$$

5. The depression in a small cylindrical vertical tube, not wetted by the fluid, is given by

$$-a = \frac{\tau^2 \cos\omega}{r} + \frac{r}{\cos^3\omega}\left(\cos^2\omega + \frac{2}{3}\sin^3\omega - \frac{2}{3}\right).$$

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as the molecules are condensed or separated from one another. That we must consider the molecules as separate, and employ the sums instead of the integrals, is clearly shown by the fact that the forces change their sign as the distances vary, as Poisson first observed.

6. If the tube be not very narrow, then the depression is given by the formula

$$-a = \frac{4\sqrt{\pi\sqrt{2}}\,.\,\sqrt{\tau l}\,.\sin\theta}{1+\cos\theta}\,e^{-l\frac{\sqrt{2}}{\tau}},$$

where

$$l = r + (1 - \cos\theta)\,\tau\sqrt{2},\quad \omega = \frac{\pi}{2} + 2\theta.$$

7. For a drop of large diameter, $2r$, of a fluid that does not wet the horizontal plane on which it is placed, the height is expressed by

$$a = \tau\sqrt{2}\cos\frac{\omega}{2} + \frac{\tau^2}{3\,l\cos\frac{\omega}{2}}\left(1 - \sin^3\frac{\omega}{2}\right),$$

where

$$l = r + (\sqrt{2} - 1)\,\tau.$$

8. The maximum height a to which we can raise a disc, applied to a fluid without the fluid detaching itself and falling back into the vessel that contains it, when the fluid is of such a nature as to wet the disc, is expressed by

$$a = \tau\sqrt{2} - \frac{\tau^2}{3r}.$$

The weight of the fluid raised in this case is given by

$$p = \pi m\left(\tau r^2\sqrt{2} - \frac{\tau^2 r}{3}\right),$$

r being the radius of the disc, and m the weight of a cube volume of the fluid, whose side is the unit of measure in which r and τ are expressed.

9. If the disc is not wetted by the fluid, then we have

$$a^2 = 2\tau^2\sin^2\frac{\omega}{2} - \frac{4}{3\sqrt{2}}\frac{\tau^3}{r}\left(1 - \cos^3\frac{\omega}{2}\right)\left(1 - \frac{\tau\sqrt{2}}{2r}\cos\frac{\omega}{2}\right)$$

$$p = \pi m a r^2 + \pi m \tau^2 r \sin\omega.$$

10. For water at the temperature 8°·5 *centigrade*, we have from Gay-Lussac's experiments, $\tau = 3{\cdot}8888$, the *millimetre* being the linear unit; and $\tau^2 = 2\frac{T}{g\Delta}$, Δ denoting the density of the fluid, g gravity, T the contractile force of the free surface of the water.

For mercury we have, from the experiments of the same philosopher, at a temperature of 8°·5 *centigrade*, $\tau = 2{\cdot}5547$; and for the angle of contact $\omega = 45°{\cdot}30'$.

Article XXV.

Note on a Capillary Phænomenon observed by Dr. Young. *By Prof.* Mossotti*.

1. I HAVE had the honour of presenting within the last few days, to the different members of this Section, a copy of a lecture† lately published in the 98th volume of the *Biblioteca Italiana,* on the Theory of Capillary Attraction. My object in so doing was to call your attention to the simple notions from which the said theory has been deduced, to apply it now to the explanation of a phænomenon which has been investigated, I think without success, by the illustrious Poisson in his excellent work *Nouvelle Théorie de l'Action Capillaire*, p. 141.

The phænomenon to which I allude relates to the equilibrium of two fluids placed one upon the other in a capillary tube, and is the memorable one that Dr. Young brought forward against the theory of Laplace. Dr. Young, as is well known, observed that if a drop of oil be poured into a tube immersed in water, and in which the fluid has risen to its due height, the level of the external surface of the oil descends perceptibly below the height at which the external surface of the column of water previously stood. Laplace's formulæ, as well as Poisson's, which are identical, do not agree with this descent, and only the latter of these authors contented himself with observing that the upper surface of the small fluid column will exhibit in the direction of its axis a slight depression of level, owing to the greater concavity that the said surface acquires. So slight a depression cannot, however, be the same as that which attended the fact mentioned by Dr. Young, since this philosopher, speaking of seeing the small fluid column descend, uses the word ***conspicuously***.

2. The chief cause that gives rise to capillary phænomena is that tension or contractile force that fluids acquire along their surface, which I have shown to depend on the rapid rarefaction to which liquids are subject near their surface. When the surface of the fluid is free this contractile force is greatest, since

* Read in part at the Section of Nat. Phil., Chemistry and Mathematics of the Second Meeting of the Italian Scientific Association at Turin, Sept. 1840.

† Vide Art. XXIII. and note, p. 558.

the fluid is rarefied to such a degree that the sum of repulsive and attractive actions of the internal molecules on a molecule situated in the surface is zero. If the surface of the fluid be not free, but there be another fluid contiguous to it, the contractile force diminishes, for the rarefaction of the fluid in which the molecular forces are more powerful is carried only to such a degree that the sum of the actions of the internal molecules on a molecule placed in the surface of contact of the two fluids shall be equal to the sum of the actions that are exerted on the same molecule by the fluid in which the molecular forces are less energetic. By means of this rapid decrease of density in the neighbourhood of the surface of contact between the two fluids the passage takes place from the more energetic state of the molecular forces of the first fluid, to the less energetic state of those of the second without altering the total equilibrium of the masses. These results are the necessary consequences of the theory that we have developed in the above-named lecture to explain the capillary phænomena.

3. In accordance with these principles, let us represent by T the constant force of tension that arises in the surface of contact between the two fluids in consequence of the above-mentioned decrease in density, and let us imagine a small fluid thread at a perceptible distance from the sides of a small cylindrical tube, terminating internally at the upper surface of the upper fluid and externally at the free surface of the lower fluid. Dividing the internal part of the fluid thread corresponding to the upper fluid into two portions, we shall easily sec (following the same reasoning that was employed in § 9 of the above-named lecture) that the equations for the particular equilibrium of these two portions will be

$$g\Delta(z-b)-\Theta = \mathrm{T}\left(\frac{1}{\varrho}+\frac{1}{\varrho'}\right)$$

$$g\Delta(b-z_{\prime})+g\Delta_{\prime}z_{\prime}+\Theta = \mathrm{T}_{\prime}\left(\frac{1}{\varrho_{\prime}}+\frac{1}{\varrho_{\prime}'}\right),$$

where b denotes the height of the point of division between the two portions above the level of the external fluid, and Θ the pressure or tension to which the fluid is subject at that point. The other letters have the same meaning as in the above-named lecture, and the accents serve to denote the analogous qualities in the lower fluid.

The horizontal section of the small fluid column at the height b being a surface of level, since the resultant of all the forces is perpendicular to it, the quantity Θ will be constant in that section for all the threads that cross it at a perceptible distance from the sides of the tube. Putting then

$$\Theta + g\,\Delta\, b = c,$$

the two preceding equations become

$$\left.\begin{aligned} g\,\Delta\, z - c &= \mathrm{T}\left(\frac{1}{\varrho} + \frac{1}{\varrho'}\right) \\ g\,(\Delta_{\prime} - \Delta)\, z_{\prime} + c &= \mathrm{T}_{\prime}\left(\frac{1}{\varrho_{\prime}} + \frac{1}{\varrho_{\prime}'}\right) \end{aligned}\right\} \quad \ldots\ldots (1.)$$

which are identical with those of Art. 69 *de la Nouvelle Théorie de l'Action Capillaire.*

To these equations must be added those that exist for the neighbouring points. If we denote by Γ and $\Gamma_{\prime}$ the tensions that exist respectively in the two fluids in those parts of their surface which lie along the sides of the tube or parallel to them, and by ϖ and $\varpi_{\prime}$ the angles under which these portions meet the concave or convex parts in which the two fluid columns terminate upwards, we shall have, in consequence of what was said in § 6 of the lecture, the other two equations,

$$\left.\begin{aligned} \Gamma &= \mathrm{T}\ \cos\varpi \\ \Gamma_{\prime} &= \mathrm{T}_{\prime} \cos\varpi_{\prime} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots (2.)$$

Treating these four equations with the same process as that employed by Poisson*, we shall obtain the following:—

* The following is the process employed by the above-named author, and we place it here for the reader's convenience.

Substitute in equations (1.) the expression for the sum of the values of the inverse radii of curvature, which for cylindrical surfaces, taking axis of figure as the axis of z, becomes

$$\frac{1}{\varrho} + \frac{1}{\varrho'} = \frac{\dfrac{d^2 z}{d t^2} - \dfrac{1}{t}\dfrac{d z}{d t}\left(1 + \dfrac{d z^2}{d t^2}\right)}{\left(1 + \dfrac{d z^2}{d t^2}\right)^{\frac{3}{2}}},$$

where t denotes the distance of the ordinate z from the axis to which it is parallel. Multiply both equations by $t\,d\,t$, and we shall have

$$2\,g\,\Delta\int z\,t\,d\,t - c\,t^2 = \frac{2\,\mathrm{T}\dfrac{d\,z}{d\,t}\,t}{\sqrt{1 + \dfrac{d\,z^2}{d\,t^2}}}$$

$$2\,g\,(\Delta_{\prime} - \Delta)\int z_{\prime}\,t\,d\,t + c\,t^2 = \frac{2\,\mathrm{T}_{\prime}\dfrac{d\,z_{\prime}}{d\,t}\,t}{\sqrt{1 + \dfrac{d\,z_{\prime}^{\,2}}{d\,t^2}}};$$

the integrals $\int z\,t\,d$ and $\int z_{\prime}\,t\,d\,t$ being $= 0$ when $t = 0$.

$$\left.\begin{aligned} h &= \varepsilon - \frac{\Delta}{\Delta_{\prime}}\varepsilon - \frac{2}{g\Delta_{\prime}\alpha}(\Gamma+\Gamma_{\prime}) + \frac{\alpha}{\Gamma^3}\left\{\mathrm{T}\Gamma^2 + \frac{2}{3}(\mathrm{T}^2-\Gamma^2)^{\frac{3}{2}} - \frac{2}{3}\Gamma^3\right\} \\ h_{\prime} &= -\frac{\Delta}{\Delta_{\prime}}\varepsilon - \frac{2}{g\Delta_{\prime}\alpha}(\Gamma+\Gamma_{\prime}) + \frac{\alpha}{\Gamma_{\prime}^3}\left\{\mathrm{T}_{\prime}\Gamma_{\prime}^2 + \frac{2}{3}(\mathrm{T}_{\prime}^2-\Gamma_{\prime}^2)^{\frac{3}{2}} - \frac{2}{3}\Gamma_{\prime}^3\right\} \end{aligned}\right\}(a.)$$

which give the heights h and $h_{\prime}$ (above the level of the external fluid) of the two centres or points on the axis of the upper surfaces in which the small column of each fluid terminates, in terms of the tensions $\mathrm{T}\,\mathrm{T}_{\prime}\,\Gamma\,\Gamma_{\prime}$, of the respective densities of the two fluids Δ and $\Delta_{\prime}$, of the radius α of the tube, and of the volume of the upper fluid expressed by $\pi\alpha^2\varepsilon$.

Now observing that we have

$$\cos\omega = -\frac{\frac{dz}{dt}}{\sqrt{1+\frac{dz^2}{dt^2}}}, \qquad \cos\omega_{\prime} = -\frac{\frac{dz_{\prime}}{dt}}{\sqrt{1+\frac{dz_{\prime}^2}{dt^2}}},$$

equations (2.) become

$$\mathrm{T}\frac{dz}{dt} + \Gamma\sqrt{1+\frac{dz^2}{dt^2}} = 0$$

$$\mathrm{T}_{\prime}\frac{dz_{\prime}}{dt} + \Gamma_{\prime}\sqrt{1+\frac{dz_{\prime}^2}{dt^2}} = 0,$$

and will hold for $t = \alpha$, where α is the radius of the tube.

The signs to be prefixed to the radicals in these equations are the same as those of the preceding equations. If then we make $t = \alpha$ and eliminate $\frac{dz}{dt}$ and $\frac{dz_{\prime}}{dt}$ between these four equations, we shall have

$$\left.\begin{aligned} 2g\Delta\int_0^\alpha z\,t\,dt - c\,\alpha^2 + 2\alpha\Gamma = 0 \\ 2g(\Delta_{\prime}-\Delta)\int_0^\alpha z_{\prime}\,t\,dt + c\,\alpha^2 + 2\alpha\Gamma_{\prime} = 0. \end{aligned}\right\} \ldots\ldots\ldots (3.)$$

Let h and $h_{\prime}$ be the vertical ordinates of the centres of the two capillary surfaces, that is to say the respective values of z and $z_{\prime}$ when $t = 0$. At these points the two radii of curvature of each surface are equal and of the same sign. If then we put $\varrho = \varrho' = \gamma$; $\varrho_{\prime} = \varrho_{\prime}' = \gamma_{\prime}$ when $t = 0$, we shall have from equations (1.)

$$\left.\begin{aligned} g\Delta h - c &= \frac{2\mathrm{T}}{\gamma} \\ g(\Delta_{\prime}-\Delta)h_{\prime} + c &= \frac{2\mathrm{T}_{\prime}}{\gamma_{\prime}}. \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots (4.)$$

Now for a first approximation, which will be sufficient, we may suppose that the capillary surfaces coincide with their osculating spheres at the points where they intersect the axis of their figure, the coordinates of which are respectively h and $h_{\prime}$. In this case we shall have

$$z = h + \gamma - \sqrt{\gamma^2 - t^2}, \quad z_{\prime} = h_{\prime} + \gamma_{\prime} - \sqrt{\gamma_{\prime}^2 - t^2}.$$

The radicals having respectively the same sign as the values of γ and $\gamma_{\prime}$, viz. positive or negative according as each surface turns its concavity upwards or

These two equations differ from the corresponding equations obtained by Poisson, as we have not made use of an equation which he denotes by $F - F' = K$ in Art. 69 of his *Théorie de l'Action*, &c., and which we consider inadmissible in these cases.

4. Let us apply the obtained equations to the experiments made by natural philosophers, that is when one of the two fluids wets the sides of the tube all round, in which cases alone can we, I think, easily obtain constant effects. I therefore observe, that if the upper fluid wets the sides, its internal surface being everywhere in contact with the lower fluid, the tension in the surface

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downwards. Substituting these values of z under the integrals in equations (3.), and effecting the integrations, we shall obtain the two equations

$$g\,\Delta\left\{(h+\gamma)\,\alpha^2+\frac{2}{3}(\gamma^2-\alpha^2)^{\frac{3}{2}}-\frac{2}{3}\gamma^3\right\}-c\,\alpha^2+2\,\alpha\,\Gamma=0$$

$$g(\Delta_,-\Delta)\left\{(h_,+\gamma_,)\,\alpha^2+\frac{2}{3}(\gamma_,^2-\alpha^2)^{\frac{3}{2}}-\frac{2}{3}\gamma_,^3\right\}+c\,\alpha^2+2\,\alpha\,\Gamma_,=0;$$

and in consequence of equations (4.) these will become

$$\frac{2\,T}{\gamma}\alpha^2+2\,\alpha\,\Gamma+g\,\Delta\left\{\gamma\,\alpha^2+\frac{2}{3}(\gamma^2-\alpha^2)^{\frac{3}{2}}-\frac{2}{3}\gamma^3\right\}=0$$

$$\frac{2T_,}{\gamma_,}\alpha^2+2\,\alpha\,\Gamma_,+g(\Delta_,-\Delta)\left\{\gamma_,\,\alpha^2+\frac{2}{3}(\gamma_,^2-\alpha^2)^{\frac{3}{2}}-\frac{2}{3}\gamma_,^3\right\}=0.$$

Assuming the radius α very small when Γ and $\Gamma_,$ are not supposed very small, we shall obtain approximately

$$\frac{1}{\gamma}=-\frac{\Gamma}{T}\,\frac{1}{\alpha}+\frac{g\,\Delta\,\alpha}{2\,T\,\Gamma^3}\left\{T\,\Gamma^2+\frac{2}{3}(T^2-\Gamma^2)^{\frac{3}{2}}-\frac{2}{3}\Gamma^3\right\}$$

$$\frac{1}{\gamma_,}=-\frac{\Gamma_,}{T_,}\,\frac{1}{\alpha}+\frac{g(\Delta_,-\Delta)\,\alpha}{2\,T_,\,\Gamma_,^3}\left\{T_,\Gamma_,^2+\frac{2}{3}(T_,^2-\Gamma_,^2)^{\frac{3}{2}}-\frac{2}{3}\Gamma_,^3\right\}.$$

If now we denote by $\pi\,\alpha^2\,\epsilon$ the volume of the upper fluid, which ought to be given, we shall have

$$\alpha^2\,\epsilon=2\int_0^{\alpha}z\,t\,d\,t-2\int_0^{\alpha}z_,\,t\,d\,t;$$

or by equations (3),

$$\alpha^2\,\epsilon=\frac{c\,\alpha^2}{g}\left(\frac{1}{\Delta}+\frac{1}{\Delta_,-\Delta}\right)+\frac{2\,\alpha}{g}\left(\frac{\Gamma_,}{\Delta_,-\Delta}-\frac{\Gamma}{\Delta}\right),$$

whence we get

$$c=g\,\epsilon\,\frac{\Delta(\Delta_,-\Delta)}{\Delta_,}+\frac{2}{\alpha\,\Delta_,}(\Gamma(\Delta_,-\Delta)-\Gamma_,\Delta).$$

Substituting this value of c as well as those of $\frac{1}{\gamma}$ and $\frac{1}{\gamma_,}$ in equations (4.), we shall finally obtain

$$h=\epsilon-\frac{\Delta}{\Delta_,}\epsilon-\frac{2}{g\,\alpha\,\Delta_,}(\Gamma+\Gamma_,)+\frac{\alpha}{\Gamma^3}\left\{T\,\Gamma^2+\frac{2}{3}(T^2-\Gamma^2)^{\frac{3}{2}}-\frac{2}{3}\Gamma^3\right\}$$

$$h_,=-\frac{\Delta}{\Delta_,}\epsilon-\frac{2}{g\,\alpha\,\Delta_,}(\Gamma+\Gamma_,)+\frac{\alpha}{\Gamma_,^3}\left\{T_,\Gamma_,^2+\frac{2}{3}(T_,^2-\Gamma_,^2)^{\frac{3}{2}}-\frac{2}{3}\Gamma_,^3\right\},$$

which are the same as the equations in the text.

of the latter must be the same as well in the parts parallel to the sides as in the upper part, in contact with the superimposed fluid, which part will consequently join with the former, bending downwards in a direction tangent to the sides, and we shall have $\varpi = 0$. If on the contrary it is the lower fluid that wets the sides, the tension in the surface of contact of the two liquids will also be constant, but this surface will bend itself in a direction tangent to the sides, turning its concavity upwards, and consequently we shall have $\varpi_{\prime} = \pi$. These results agree with those that Poisson (Art. 73) has deduced from other principles.

5. Having established the rule to be followed in assigning the value of $\varpi_{\prime\prime}$, we must, in order to possess all the numerical data to be introduced into equations (*a.*), know the value of $T_{\prime\prime}$, which represents the tension of the water in contact with the oil. To obtain this I shall avail myself of an experiment which our illustrious member Cavalier Avogadro has published in vol. xl. of the *Memorie dell'Accademia di Torino*, and in the *Annales de Chimie et Physique*, Avril 1837.

After coating with oil the inside of a small glass tube of rad. = 1 *millimetre*, he introduced it vertically into a trough of water. The water rose in the tube to the height of 5·34 *millimetres*, raising on its upper surface a slender stratum of oil which it detached from the sides as it rose. The specific gravity of the oil employed was $\Delta = \cdot 908$, that of water being taken as unity. The tension of the free surface in contact with the air of this oil, calculated according to the theory by an experiment of the same author, would be $T = 3{\cdot}81\, g\,\Delta$.

Now if we observe that in this experiment the upper surface of the oil was tangent to the sides with its concavity turned upwards, and that of the lower fluid or water must according to the exposed principles be at its circumference also tangent to the direction of the sides, but with its concavity turned downwards, we shall have $\varpi = \pi$, $\varpi_{\prime} = 0$, and therefore from equations (2.) we get

$$\Gamma = -T, \qquad \Gamma_{\prime} = T_{\prime}.$$

Substituting these values in the second equation (*a.*), we obtain

$$h_{\prime} = -\frac{\Delta}{\Delta_{\prime}}\,\epsilon + \frac{2}{g\,\Delta_{\prime}\,\alpha}(T - T_{\prime\prime}) + \frac{\alpha}{3};$$

whence we obtain

$$\frac{T_{\prime}}{g\,\Delta_{\prime}} = \frac{T}{g\,\Delta}\cdot\frac{\Delta}{\Delta_{\prime}} - \frac{\alpha}{2}\left(h_{\prime} + \epsilon\,\frac{\Delta}{\Delta_{\prime}}\right) + \frac{\alpha^2}{6} \;\ldots\ldots\; (b.)$$

Neglecting in this equation the quantity $\frac{a^2}{6} - \frac{1}{2}\frac{a \epsilon \Delta}{\Delta_{\prime}}$, the value of ϵ not being even given in this experiment, as being too small and inappreciable, and substituting $\frac{T}{g\Delta} = 3{\cdot}81$, $\Delta = {\cdot}908\ \Delta_{\prime}$, $h_{\prime} = 5{\cdot}34$ *millimetres*, and $a = 1$, we shall find

$$T_{\prime} = {\cdot}79\ g\,\Delta_{\prime},$$

which will be the expression for the force of tension of the surface of the water in apparent contact with the oil; $\Delta_{\prime}$ being the specific density of water taken as unity, and the linear unit being the *millimetre*.

6. Having obtained this, let us now come to Dr. Young's experiment. Into a small tube partly immersed in water, and in which this fluid, by reason of capillarity, was raised almost to the upper extremity of the tube, he let fall a drop of oil, and saw the small column of water in the tube descend perceptibly.

To calculate this experiment, I observe that the drop of oil on lying upon the water must also bend itself so that the concavity of its surfaces shall be turned upwards and terminate parallel to the sides. In this case therefore we shall have simultaneously $\omega = \pi$, $\omega_{\prime} = \pi$, and consequently

$$\Gamma = -\,T, \qquad \Gamma_{\prime} = -\,T_{\prime};$$

and the second equation (a.) will give

$$h_{\prime} = -\frac{\Delta}{\Delta_{\prime}}\,\epsilon + \frac{2}{g\,\Delta_{\prime}\,a}\,(T + T_{\prime}) + \frac{5}{3}\,a.$$

Let us neglect again the quantity $\frac{5}{3}\,a - \frac{\Delta}{\Delta_{\prime}}\,\epsilon$ as being small, and substitute for T and $T_{\prime}$ their values $3{\cdot}81\,g\,\Delta$ and ${\cdot}79\,g\,\Delta_{\prime}$, we shall have

$$h_{\prime} = \frac{6{\cdot}92}{a} + \frac{1{\cdot}58}{a} = 8{\cdot}50 \textit{ millimetres},$$

supposing the radius of tube to be one *millimetre*.

Before letting the drop of oil fall upon the water in the tube of rad. = 1 *millimetre*, according to Gay-Lussac, the water ought to be at a height = 15·58 *millimetres*. After the oil has been placed upon it, according to our calculation, it will not stand higher than 8·50. This explains the perceptible depression observed by Dr. Young, which I proposed here to discuss.

7. The difference between Poisson's and my formulæ arises

from the fact, that he considers the whole weight of the small column composed of the two fluids to be equivalent to the action that would be exerted by the tension of the surface of the lower fluid, combined with its curvature, as though the tube did not contain any other fluid above, whereas, in my opinion, the said weight is sustained by the united actions of the tension, both of the superior surface of the upper fluid and of the lower fluid, combined with their respective curvatures, but estimating the tension along the surface of the latter fluid according to the nature of the fluid that is placed upon it. Poisson has attempted another demonstration of his proposition in Art. 72; but we can easily perceive that his demonstration is defective, since in estimating the second part of the force which he denotes by R, he has not taken into account the state of tension of the upper fluid; for if the portion of the small fluid column that stands above the natural level were not slightly though imperceptibly rarefied, and did not acquire a state of tension, the upper fluid parts would be unable to sustain the lower parts, and the column would break and fall when placed *in vacuo**.

For these reasons we believe that the assertions of Gay-Lussac, quoted by Poisson, Art. 74, ought not to be so placed, but rather taken with the formula by means of which we calculated Cavalier Avogadro's experiment, viz. formula (*b*.), and from this we shall obtain the values of the tension of the surface of the mercury when perceptibly in contact with water and with alcohol, expressed respectively by

$$T_{\prime} = 2{\cdot}77\, g\, \Delta_{\prime\prime}, \qquad T_{\prime} = 2{\cdot}63\, g\, \Delta_{\prime\prime}$$

Δ being the density of the mercury.

8. I have thought fit to point out these examples, to show how much greater facility the new point of view under whic the theory of capillary action has been lately expounded affords in its applications where no misunderstanding occurs. On the other hand, these results by no means invalidate the principles laid down in the *Nouvelle Théorie de l'Action Capillaire*. It is to the celebrated author of that treatise that we are indebted for the calculation of the pressure or tension in bodies formed of discontinuous molecules, for the discovery of the necessity of a rapid rarefaction towards the surfaces of fluids in order that

* In the atmosphere the pressure of the air, which balances itself, condenses the fluid mass and produces in it a uniform increase of repulsion between the parts, which does not alter the conditions of equilibrium that obtain *in vacuo*.

capillary phænomena may be produced, and for having thus laid the foundation of the theory. I have great pleasure in availing myself of the present occasion to pay a just tribute to the memory of this great master, whose recent loss we all deplore. The classical works which Poisson brought forth at short intervals of time, were, for their depth of thought and their masterly display of analysis, eagerly received and zealously studied by mathematicians, and will remain impressed in our memory as everlasting titles to his glory, and in our hearts as an argument of grief for his precocious death. Not only mathematicians, but all society shares these sentiments of sorrow and admiration for so great a philosopher. Poisson, preceding the progress of natural science, like a beacon lighted the way along which human genius is now rapidly advancing.

Turin, September 26th, 1840.

Article XXVI.

Explanation of a Method for computing the Absolute Disturbances of the Heavenly Bodies, which move in Orbits of any Inclination and Elliptic Eccentricity whatever. By M. Hansen, *Director of the Observatory at Gotha*.*

[Read at the Meeting of the Royal Academy of Sciences at Berlin, January 12, 1843.]

It is well known that we employ for the calculation of the places, or, in other words, for the determination of the orbits of the older planets and satellites of our solar system, a method totally different from that used for the four new planets and for comets. For the former we calculate once for all the expressions for the heliocentric polar-coordinates simply as functions of the time, whose numerical value we need only substitute in each case to obtain the position of the body in space, a labour which is rendered easier by the previous computation of tables. For the four new planets and for comets, on the contrary, we calculate for a series of points of time following each other at short intervals the numerical values of the differentials of their elliptic elements, and thence deduce, by that kind of summation which is known under the name of mechanical quadratures, the changes which the elliptic elements undergo from the first to any one of the other points of time taken for computation. From hence we can, by means of the numerical values of the elliptic elements so derived, compute in space the place of the heavenly body in question.

This latter method, which is totally different from the former, has not been employed in the case of the above-mentioned bodies on account of its being preferable to the other, but only because no other method is known by which, on account of their great eccentricities and inclinations, their coordinates could be expressed in a function of the time.

The method of calculating by mechanical quadratures the effect which the attraction of the planets exerts on the places or the

* Translated and communicated by the Rev. Robert Main, M.A., Sec. R.A.S., First Assistant of the Royal Observatory, Greenwich.

orbits of a heavenly body, has in comparison with the other, real disadvantages, even while, through the labours of the greatest geometers of this century, it has been brought to a high degree of perfection. The author explains, in the Memoir read before the Academy, the most important of the disadvantages of this method; here it is sufficient, not to occupy too much space, to remark, that in the deduction of the influence of the planets, or the perturbations, by means of mechanical quadratures, the calculations never come to an end, since it is necessary to repeat the same afresh from one period to another, to be able to connect the observations with each other; while, by the other method, if the perturbations be computed once for all, we are by a short process enabled to connect with one another observations lying close together, as well as those separated from each other by a long interval of time.

All that we now have to assist us in the solution of the problem in question is contained in the celebrated treatise of Gauss, entitled *Determinatio Attractionis*, &c., in which is given an elegant method by which can be calculated the secular variations, however great be the elliptic eccentricity and the inclination of the orbit. Although much is gained by this, yet it cannot be denied that a considerable gap yet remains to be filled up; for, in this treatise, we are not furnished with means of calculating the periodical terms, which are, in number and frequently also in magnitude, far more considerable than the secular variations.

The author remarks upon this head, that, *theoretically* considered, the perturbations can be calculated by means of the method given in his Essay that received the prize of the Royal Academy in the year 1830, however great may be the elliptic eccentricity and the inclination, for it is proved that the resulting series are also in this case convergent. But the result arrived at in this way is practically useless, for, the convergence with large eccentricities being very small, it will consist of thousands of terms. On the contrary, the method which the author gives in the treatise read before the Academy, leads, at least in the cases in which he has already applied it, to rapidly converging series; and this can be relied on in it, that in all other cases it gives the convergence of the series as great, or very nearly as great, as the nature of the circumstances admits of. It is indeed evident in itself that the convergence cannot be the same in all cases. The method divides into two cases, accordingly as the radius-vector of the disturbed body is *less* or *greater* than that of the disturbing. If both cases should occur with two heavenly bodies, both methods must be connected with each other, and their results be employed alternately; for example, in the case of Encke's comet and the earth, and so on. In the present treatise the author only carries out at length the case in which the radius-vector of the disturbed body is less than that of the disturbing, and does but briefly point out the treatment of the other case. He reserves the full explanation of the latter till he shall have computed an example in illustration of it.

The case here treated of fully is that to which the most considerable disturbances of the bodies of our solar system belong, which move in very eccentric and considerably inclined orbits. He comprehends in it namely the disturbances which the four small planets, the comet of Encke, and the comet of Biela undergo from Jupiter, Saturn, and Uranus. Also, on grounds which will afterwards be discussed, he only enters substantially on the consideration of those disturbances which Halley's comet undergoes from those planets.

As a first example of his method, the author gives the calculation of the disturbances which Encke's comet experiences from Saturn, and moreover informs us that he has carried out the calculation of the disturbances produced by Jupiter so far as to arrive at a view of the nature of the result. According to this, the disturbances contain scarcely a greater number of terms than those produced by the sun in the motion of the moon; but, in the former disturbances, no coefficients result so great as in the latter. While the greatest coefficient in the lunar disturbances amounts to nearly 4470″, the greatest in the disturbances of Encke's comet by Jupiter amounts only to 2480″. The secular variation of the eccentricity is small, but the yearly motion of the line of apsides of this comet produced by Jupiter amounts to more than half a minute. The disturbances which this comet undergoes by Saturn are, when developed, as follow:—

Let g' be the mean anomaly of Saturn corrected by the great inequality;
u . . . the eccentric anomaly of Encke's comet;
t . . . the time, whose unit is a Julian year;
$n\delta z$ . . the disturbance of the mean longitude;
w . . . the corresponding disturbance of the hyperbolic logarithm of the radius vector;

$\frac{r}{a}\delta s$ the disturbance of the latitude multiplied into the ratio of the radius-vector to the semiaxis major;

i . . . the inclination of the comet's orbit to the assumed fundamental plane;

then is

$$
\begin{array}{llll}
n\,\delta z = & + \;\; 1''{\cdot}51 \sin u & & - \;\; 0''{\cdot}04 \cos u \\
 & + \;\; 0{\cdot}1121\, t \sin u & & - \;\; 2{\cdot}1745\, t \cos u \\
 & - \;\; 0{\cdot}79 \sin 2u & & - \;\; 0{\cdot}02 \cos 2u \\
 & - \;\; 0{\cdot}0370\, t \sin 2u & & + \;\; 0{\cdot}4593\, t \cos 2u \\
 & + \;\; 0{\cdot}06 \sin 3u & & \ldots\ldots\ldots\ldots \\
 & - \;\; 0{\cdot}14 \sin(-2u+g') & & + \;\; 2{\cdot}27 \cos(-2u+g') \\
 & + \;\; 0{\cdot}55 \sin(-u+g') & & - \;\; 8{\cdot}50 \cos(-u+g') \\
 & + \;\; 1{\cdot}09 \sin(g') & & - \;\; 6{\cdot}48 \cos(g') \\
 & + \;\; 0{\cdot}04 \sin(u+g') & & - \;\; 1{\cdot}40 \cos(u+g') \\
 & + \;\; 0{\cdot}02 \sin(2u+g') & & - \;\; 0{\cdot}12 \cos(2u+g') \\
 & + \;\; 0{\cdot}05 \sin(-4u+2g') & & + \;\; 0{\cdot}03 \cos(-4u+2g') \\
 & - \;\; 0{\cdot}01 \sin(-3u+2g') & & + \;\; 0{\cdot}02 \cos(-3u+2g') \\
 & - \;\; 7{\cdot}16 \sin(-2u+2g') & & - \;\; 6{\cdot}45 \cos(-2u+2g') \\
 & + 25{\cdot}28 \sin(-u+2g') & & + 22{\cdot}63 \cos(-u+2g') \\
 & + \;\; 4{\cdot}41 \sin(2g') & & + \;\; 3{\cdot}74 \cos(2g') \\
 & - \;\; 0{\cdot}64 \sin(u+2g') & & - \;\; 0{\cdot}70 \cos(u+2g') \\
 & + \;\; 1{\cdot}41 \sin(2u+2g') & & + \;\; 1{\cdot}29 \cos(2u+2g') \\
 & + \;\; 0{\cdot}07 \sin(3u+2g') & & + \;\; 0{\cdot}09 \cos(3u+2g') \\
 & - \;\; 0{\cdot}11 \sin(-4u+3g') & & + \;\; 0{\cdot}08 \cos(-4u+3g') \\
 & + \;\; 0{\cdot}29 \sin(-3u+3g') & & - \;\; 0{\cdot}15 \cos(-3u+3g') \\
 & + \;\; 1{\cdot}55 \sin(-2u+3g') & & - \;\; 2{\cdot}45 \cos(-2u+3g') \\
 & - \;\; 6{\cdot}83 \sin(-u+3g') & & + \;\; 9{\cdot}21 \cos(-u+3g') \\
 & - \;\; 1{\cdot}00 \sin(3g') & & + \;\; 1{\cdot}44 \cos(3g') \\
 & + \;\; 0{\cdot}07 \sin(u+3g') & & - \;\; 0{\cdot}09 \cos(u+3g') \\
 & - \;\; 0{\cdot}35 \sin(2u+3g') & & + \;\; 0{\cdot}45 \cos(2u+3g') \\
 & - \;\; 0{\cdot}01 \sin(3u+3g') & & + \;\; 0{\cdot}03 \cos(3u+3g') \\
 & - \;\; 0{\cdot}06 \sin(-4u+4g') & & - \;\; 0{\cdot}10 \cos(-4u+4g') \\
 & + \;\; 0{\cdot}11 \sin(-3u+4g') & & + \;\; 0{\cdot}28 \cos(-3u+4g') \\
 & + \;\; 0{\cdot}62 \sin(-2u+4g') & & + \;\; 0{\cdot}50 \cos(-2u+4g') \\
 & - \;\; 2{\cdot}78 \sin(-u+4g') & & - \;\; 2{\cdot}98 \cos(-u+4g') \\
 & - \;\; 0{\cdot}39 \sin(4g') & & - \;\; 0{\cdot}42 \cos(4g') \\
 & - \;\; 0{\cdot}02 \sin(u+4g') & & - \;\; 0{\cdot}03 \cos(u+4g') \\
 & - \;\; 0{\cdot}13 \sin(2u+4g') & & - \;\; 0{\cdot}14 \cos(2u+4g') \\
 & + \;\; 0{\cdot}07 \sin(-4u+5g') & & - \;\; 0{\cdot}02 \cos(-4u+5g') \\
 & - \;\; 0{\cdot}18 \sin(-3u+5g') & & + \;\; 0{\cdot}08 \cos(-3u+5g')
\end{array}
$$

$$
\begin{array}{ll}
-0''\!\cdot\!14 \sin(-2u+5g') & +0''\!\cdot\!20 \cos(-2u+5g') \\
+1\cdot 38 \sin(-u+5g') & -0\cdot 99 \cos(-u+5g') \\
+0\cdot 19 \sin(\quad 5g') & -0\cdot 16 \cos(\quad 5g') \\
+0\cdot 04 \sin(u+5g') & -0\cdot 03 \cos(u+5g') \\
+0\cdot 07 \sin(2u+5g') & -0\cdot 04 \cos(2u+5g') \\
+0\cdot 01 \sin(-4u+6g') & +0\cdot 05 \cos(-4u+6g') \\
+0\cdot 02 \sin(-3u+6g') & -0\cdot 12 \cos(-3u+6g') \\
-0\cdot 06 \sin(-2u+6g') & -0\cdot 01 \cos(-2u+6g') \\
+0\cdot 29 \sin(-u+6g') & +0\cdot 57 \cos(-u+6g') \\
+0\cdot 05 \sin(\quad 6g') & +0\cdot 03 \cos(\quad 6g') \\
+0\cdot 01 \sin(u+6g') & +0\cdot 03 \cos(u+6g')
\end{array}
$$

$$
\begin{array}{lll}
w = & -\ 0\cdot 67 & \\
& -\ 0\cdot 0739\,t & \\
& -\ 0\cdot 50 \cos u & +\ 0\cdot 03 \sin u \\
& -\ 0\cdot 0872\,t \cos u & -\ 1\cdot 0873\,t \sin u \\
& +\ 0\cdot 12 \cos 2u & -\ 0\cdot 18 \sin 2u \\
& \ldots\ldots\ldots\ldots\ldots\ldots & -\ 0\cdot 06 \sin(-2u+g') \\
& +\ 0\cdot 31 \cos(-u+g') & +\ 4\cdot 85 \sin(-u+g') \\
& +\ 0\cdot 27 \cos(\quad g') & +\ 3\cdot 90 \sin(\quad g') \\
& +\ 0\cdot 03 \cos(u+g') & +\ 0\cdot 17 \sin(u+g') \\
& -\ 0\cdot 10 \cos(-3u+2g') & +\ 0\cdot 09 \sin(-3u+2g') \\
& -\ 0\cdot 30 \cos(-2u+2g') & +\ 0\cdot 29 \sin(-2u+2g') \\
& +14\cdot 44 \cos(-u+2g') & -12\cdot 98 \sin(-u+2g') \\
& +14\cdot 76 \cos(\quad 2g') & -13\cdot 30 \sin(\quad 2g') \\
& +\ 4\cdot 07 \cos(u+2g') & -\ 3\cdot 74 \sin(u+2g') \\
& +\ 0\cdot 23 \cos(2u+2g') & -\ 0\cdot 21 \sin(2u+2g') \\
& -\ 0\cdot 02 \cos(-4u+3g') & -\ 0\cdot 02 \sin(-4u+3g') \\
& +\ 0\cdot 14 \cos(-3u+3g') & +\ 0\cdot 11 \sin(-3u+3g') \\
& -\ 0\cdot 09 \cos(-2u+3g') & +\ 0\cdot 05 \sin(-2u+3g') \\
& -\ 4\cdot 01 \cos(-u+3g') & -\ 5\cdot 08 \sin(-u+3g') \\
& -\ 3\cdot 86 \cos(\quad 3g') & -\ 4\cdot 99 \sin(\quad 3g') \\
& -\ 1\cdot 08 \cos(u+3g') & -\ 1\cdot 43 \sin(u+3g') \\
& -\ 0\cdot 09 \cos(2u+3g') & -\ 0\cdot 11 \sin(2u+3g') \\
& -\ 0\cdot 02 \cos(-4u+4g') & +\ 0\cdot 03 \sin(-4u+4g') \\
& +\ 0\cdot 07 \cos(-3u+4g') & -\ 0\cdot 12 \sin(-3u+4g') \\
& -\ 0\cdot 04 \cos(-2u+4g') & +\ 0\cdot 11 \sin(-2u+4g') \\
& -\ 1\cdot 52 \cos(-u+4g') & +\ 1\cdot 77 \sin(-u+4g') \\
& -\ 1\cdot 43 \cos(\quad 4g') & +\ 1\cdot 60 \sin(\quad 4g') \\
& -\ 0\cdot 42 \cos(u+4g') & +\ 0\cdot 46 \sin(u+4g') \\
& -\ 0\cdot 05 \cos(2u+4g') & +\ 0\cdot 05 \sin(2u+4g')
\end{array}
$$

$+0''.02\cos(-4u+5g')$	$+0''.01\sin(-4u+5g')$
$-0.08\cos(-3u+5g')$	$-0.04\sin(-3u+5g')$
$+0.08\cos(-2u+5g')$	$+0.02\sin(-2u+5g')$
$+0.79\cos(-u+5g')$	$+0.52\sin(-u+5g')$
$+0.70\cos(5g')$	$+0.45\sin(5g')$
$+0.20\cos(u+5g')$	$+0.13\sin(u+5g')$
$+0.03\cos(2u+5g')$	$+0.02\sin(2u+5g')$
. . . .	$-0.02\sin(-4u+6g')$
$-0.01\cos(-3u+6g')$	$+0.04\sin(-3u+6g')$
$+0.01\cos(-2u+6g')$	$-0.05\sin(-2u+6g')$
$+0.15\cos(-u+6g')$	$-0.39\sin(-u+6g')$
$+0.13\cos(6g')$	$-0.30\sin(6g')$
$+0.04\cos(u+6g')$	$-0.10\sin(u+6g')$

$$\frac{r\,\delta s}{a\cos i} =$$

	-0.081
	$+0.1395\,t$
$+0.089\sin u$	$+0.097\cos u$
$+0.2486\,t\sin u$	$-0.1651\,t\cos u$
$+0.034\sin 2u$	$-0.021\cos 2u$
$-0.015\sin 3u$	$-0.001\cos 3u$
$+0.006\sin(-3u+g')$	$+0.016\cos(-3u+g')$
$+0.012\sin(-2u+g')$	$-0.029\cos(-2u+g')$
$-0.685\sin(-u+g')$	$+0.035\cos(-u+g')$
$+0.083\sin(g')$	$+0.370\cos(g')$
$+0.598\sin(u+g')$	$-0.476\cos(u+g')$
$-0.014\sin(2u+g')$	$+0.022\cos(2u+g')$
$+0.003\sin(-3u+2g')$	$+0.014\cos(-3u+2g')$
$+0.034\sin(-2u+2g')$	$+0.021\cos(-2u+2g')$
$-1.119\sin(-u+2g')$	$-1.600\cos(-u+2g')$
$+0.302\sin(2g')$	$+0.505\cos(2g')$
$+0.755\sin(u+2g')$	$+1.043\cos(u+2g')$
$+0.026\sin(2u+2g')$	$-0.003\cos(2u+2g')$
$-0.023\sin(-3u+3g')$	$-0.002\cos(-3u+3g')$
$+0.033\sin(-2u+3g')$	$+0.005\cos(-2u+3g')$
$+0.509\sin(-u+3g')$	$-0.378\cos(-u+3g')$
$-0.224\sin(3g')$	$+0.138\cos(3g')$
$-0.277\sin(u+3g')$	$+0.229\cos(u+3g')$
$-0.010\sin(2u+3g')$	$+0.010\cos(+2u+3g')$
$-0.006\sin(-3u+4g')$	$-0.030\cos(-3u+4g')$
$+0.004\sin(-2u+4g')$	$+0.019\cos(-2u+4g')$
$-0.047\sin(-u+4g')$	$+0.199\cos(-u+4g')$

$-0''.016 \sin(\quad 4g')$	$-0''.095 \cos(\quad 4g')$
$-0.071 \sin(u+4g')$	$-0.093 \cos(u+4g')$
$+0.022 \sin(-3u+5g')$	$-0.005 \cos(-3u+5g')$
$-0.037 \sin(-2u+5g')$	$+0.013 \cos(-2u+5g')$
$-0.054 \sin(-u+5g')$	$+0.024 \cos(-u+5g')$
$+0.033 \sin(\quad 5g')$	$-0.013 \cos(\quad 5g')$
$+0.038 \sin(u+5g')$	$-0.019 \cos(u+5g')$

This is the result for the disturbances of Encke's comet by Saturn, and it is the first result of the kind.

On counting the arguments of the above disturbances in longitude, we find them to be forty-six in number, and in the disturbances of the logarithm of the radius-vector and of the latitude there are rather fewer. These disturbances consist of precisely the same number of terms as of arguments, when, by a known transformation, we unite into one each pair of the foregoing terms. Under the coefficients of the disturbances of longitude there are, if we do not include the secular variations, or the pair of terms multiplied by the time, only fourteen arguments whose coefficients are greater than $1''$, fifteen whose coefficients lie between $1''$ and $0''.1$, and fifteen whose coefficients are less than $0''.1$. In the disturbances of the logarithm of the radius-vector is found very nearly the same proportion, and, in the disturbances of the latitude, all the coefficients, including the two above mentioned, are less than $1''$.

The author here gives a comparison between the foregoing *absolute* disturbances and some *relative* disturbances computed by Encke by means of mechanical quadratures. This comparison, for want of room, it is thought proper to omit here, since it will very shortly be published.

In explanation of the method by which the preceding result has been arrived at, the author first treats of the expansion of the quantity: unity divided by the ratio of the mutual distance of the comet and planet, arranged according to powers of the ratio of the radii. The expansions are, as is known, as follow:—

$$\frac{1}{\Delta} = \frac{1}{r'} + \frac{r}{r'^2} U_1 + \frac{r^2}{r'^3} U_2 + \frac{r^3}{r'^4} U_3 + \&c.$$

$$\frac{1}{\Delta} = \frac{1}{r} + \frac{r'}{r^2} U_1 + \frac{r'^2}{r^3} U_2 + \frac{r'^3}{r^4} U_3 + \&c.$$

where Δ is the distance, r and r' the radii-vectores,

$$U_1 = H; \; U_2 = \frac{3}{2} H^2 - \frac{1}{2}; \; U_3 = \frac{5}{2} H^3 - \frac{3}{2} H; \; \&c.$$

and H the cosine of the angle which the radii-vectores include. These two series do not converge in all cases, for if r be $> r'$, the first series never converges, and if r be $< r'$, the second series never converges; for this reason, in the problem in question, the two cases corresponding to $r < r'$, and $r > r'$, must be taken separately. In the first case the first series is always convergent, and in the latter case the second is always convergent. If $r = r'$, both series converge, with the exception of the case where at the same time $H = +1$. But this case supposes the coming together of the comet and planet, in which generally the computation of the disturbances ceases to be possible.

The author names the degree of convergence which the above series present from one term to another, when they are expanded according to cosines of multiples of the angle whose cosine is H, the *natural* convergence of the disturbance-function, and it is his object that this shall be by no means increased, but on the contrary be diminished by means of further development. In this further development it is necessary to set out with the supposition that the natural convergence of the disturbance-function and of its differential coefficients is possible.

The integrals

$$\int \frac{1}{\Delta}\, dt; \quad \int \frac{d\,\frac{1}{\Delta}}{dr}\, dt; \quad \int \frac{d\,\frac{1}{\Delta}}{d\mathrm{H}}\, dt$$

converge more rapidly than the quantity $\frac{1}{\Delta}$ itself and its differential coefficients. This law sometimes suffers an exception, which, however, concerns only individual terms, which will be increased by integration. Precisely in those cases where the natural convergence of the differentials is least, it will commonly be most increased through integration.

Through the expansion of the disturbance-function in multiples of the sine and cosine of the mean anomalies of both the bodies in question, and the thereby necessary and inevitable expansion of the coefficients in infinite series proceeding according to powers of the eccentricity and inclination, whether we represent them explicitly or express their sums, that is, the coefficients themselves, by means of transcendents, the natural convergence of the disturbance-function, even when the eccentricity and inclination are small, is remarkably diminished; and when these quantities are of considerable magnitude, the natural convergence is so much diminished that it is necessary to give up the use of the infinite series thence arising. This takes place to a much greater extent when eccentricities and inclinations similar to those of the orbit of a comet are in question. It is therefore necessary, in the solution of the problem before us, both in the disturbance-function and in all the other functions whose expansions are required, to avoid infinite series proceeding according to the powers of the eccentricity and the inclination of the orbit of the comet.

The total disuse of such INFINITE *series is the basis of the method which is here represented.*

For this purpose let

$$H = A\cos f + B\sin f,$$

where f represents the true anomaly of the comet, and

$$A = \cos^2\frac{1}{2}\,I\cos(f' - 2k) - \sin^2\frac{1}{2}\,I\cos(f' + 2N)$$

$$B = \cos^2\frac{1}{2}\,I\sin(f' - 2k) - \sin^2\frac{1}{2}\,I\sin(f' + 2N),$$

in which f' represents the true anomaly of the planet, I the mutual inclination of the orbits of the comet and planet, and $N \pm K$ represents the distance of the perihelion from the ascending node of the orbit of the comet reckoned on the plane of the orbit of the comet. Call now the disturbance-function Ω, and the masses of the sun, of the comet, and of the planet respectively M, m, m', then have we, in the case in which $r < r'$,

$$\Omega = \frac{m'}{M+m}\left\{\frac{r^2}{r'^3}U_2 + \frac{r^3}{r'^4}U_3 + \&c.\right\}.$$

Substitute now the above expression for H in the preceding values of U_2, U_3, &c., and these again in the expression for Ω, and make

$$x = \frac{r}{a}\cos f;\quad y = \frac{r}{a}\sin f,$$

then

$$\Omega = \frac{m'}{M+m}\left\{\begin{array}{l} x^2\frac{a^2}{r'^3}\left(\frac{3}{2}A^2 - \frac{1}{2}\right) + x\,y^3\frac{a^2}{r'^3}AB \\ \qquad + y^2\cdot\frac{a^2}{r'^3}\left(\frac{3}{2}B^2 - \frac{1}{2}\right) \\ + x^3\frac{a^3}{r'^4}\left(\frac{5}{2}A^3 - \frac{3}{2}A\right) + x^2y\frac{a^3}{r'^4}\left(\frac{15}{2}A^2B - \frac{3}{2}B\right) \\ + xy^2\frac{a^3}{r'^4}\left(\frac{15}{2}AB^2 - \frac{3}{2}A\right) + y^3\frac{a^3}{r'^4}\left(\frac{5}{2}B^3 - \frac{3}{2}B\right) \\ + \&c. \end{array}\right\}$$

The coefficients of the powers and products of the coordinates x and y of this expression are whole and rational functions of A, B, and $\frac{1}{r'}$. By virtue of the foregoing expressions of A and B, and of the value $\frac{1 + e' \cos f'}{a' (1 - e'^2)}$ of $\frac{1}{r'}$, these coefficients are all whole and rational functions of $\sin f'$ and $\cos f'$, and may consequently be reduced to the form

$$\alpha_0 + \alpha_1 \cos f' + \alpha_2 \cos 2f' + \ldots + \alpha_\mu \cos \mu f'$$
$$+ \beta_1 \sin f' + \beta_2 \sin 2f' + \ldots + \beta_\mu \sin \mu f',$$

where the coefficients are whole and rational functions of the eccentricity e' of the planet and of the mutual inclination of the orbits of the comet and planet. Consequently there do not result any series that go on to infinity according to the powers of I and e'. If we denote the coefficients in general, when reduced to the above form, by $C_{k,l}$, we have then

$$\Omega = \Sigma\, x^k\, y^l\, C_{k,l}$$

neglecting the terms in which $k + l$ is < 2. We see readily that in the last term of the development of $C_{k,l}$ that has been pointed out, $\mu = 2\,(k + l) + 1$. Call now the eccentric anomaly of the comet u, then is

$$x = \cos u - e; \quad y = \sqrt{1 - e^2} \,.\, \sin u;$$

and consequently $x^k y^l$ is a whole and rational function of $\sin u$ and $\cos u$, in which the coefficients are also whole and rational functions of e and $\sqrt{1 - e^2}$. Consequently $x^k y^l$ is reduced to the following infinite series:—

$$x^k y^l = \gamma_0 + \gamma_1 \cos u + \gamma_2 \cos 2u + \ldots + \gamma_{k+l} \cos (k + l)\, u$$
$$\text{or} \quad = \quad \epsilon_1 \sin u + \epsilon_2 \sin 2u + \ldots + \epsilon_{k+l} \sin (k + l)\, u,$$

according as l is an even or an odd number. In this expression the coefficients are also whole and rational functions of e and $\sqrt{1 - e^2}$. By the multiplication of this expression for $x^k y^l$ into the above expression for $C_{k,l}$, there results finally,

$$\Omega = \Sigma\, K_{i,i'} \cos (i u + i' f') + \Sigma\, Z_{i,i'} \sin (i u + i' f'),$$

in which, infinite series proceeding according to the powers and products of the eccentricity and inclination, are altogether avoided, and consequently the natural convergence of the disturbance-function is not at all vitiated, or at all events in the smallest possible degree.

According to the foregoing division of the subject, it is now to be shown how we are to proceed in the second case, in which r is $> r'$.

On account of the small eccentricity of the disturbing planets it is not necessary, at least in the greater number of cases, to avoid infinite series proceeding according to the powers of the eccentricity of the planet. We certainly lose thereby somewhat of the natural convergence of the disturbance-function, but the diminution which it receives is not so great as to be hurtful; on the other hand, while something is lost in this respect, we gain an advantage with relation to the facility of the integration and the subsequent application of the disturbances. Instead of the form just given for the expansion of the disturbance-function, the author employs, for the case now treated of, the following:—

$$\Omega = \Sigma\, M_{i,i'} \cos\,(i\,u + i'\,g') + \Sigma\, N_{i,i'} \sin\,(i\,u + i'\,g'),$$

where g' is the mean anomaly of the disturbing planet.

It is now a matter of indifference, for the end here to be attained, which method is employed for carrying out the expansion of Ω and its differential coefficients, provided only that by expansion the preceding formula be adapted for use, and the values of the coefficients be fully obtained; for to this form correspond determinate values of the coefficients $M_{i,i'}$ and $N_{i,i'}$, and therefore the other method of development, if only it be based on correct principles, and be capable of complete development, must lead to the same values of these coefficients. It may be most advantageous in one particular case to employ the latter, and in others to employ the former method. In the calculation of the disturbances of Encke's comet by Saturn, before exhibited, the author has employed for the expansion of the differential coefficients of Ω the same analysis and the same quantities which were useful in the foregoing, for the purpose of finding the form which must be given to the expansion in the problem before us to produce the greatest possible amount of convergence. The latter method has in this instance led very rapidly to the desired result, for the expansion of the differential coefficients of Ω required only the labour of two days. The separation of the formulæ necessary for this method must, for the sake of brevity, be here omitted, and we proceed therefore with the explanation of the remaining part of the memoir.

The author employs for the calculation of the disturbances in question the three components of the disturbing force, of which one is parallel to the major axis and another to the minor axis of the comet's orbit, and the remaining one is perpendicular to the plane of the orbit. These are, as is known, the differential coefficients $\left(\frac{d\,\Omega}{d\,x}\right)$, $\left(\frac{d\,\Omega}{d\,y}\right)$, and $\left(\frac{d\,\Omega}{d\,z}\right)$, where x and y have the same signification as above. It is clear from the foregoing that their expansions have the same form as that of the quantity Ω. The differential through whose integration we must get the disturbances of the coordinates of the disturbing body, be those coordinates of what kind soever, we can always reduce to the following form:—

$$\mathrm{P}\left(\frac{d\,\Omega}{d\,x}\right) + \mathrm{Q}\left(\frac{d\,\Omega}{d\,y}\right) + \mathrm{R}\left(\frac{d\,\Omega}{d\,z}\right),$$

where P, Q, and R are functions of the elliptic elements and of the coordinates of the disturbed body. Since now with respect to these functions, which are also interminable, we must avoid series proceeding according to the powers and products of the eccentricity and the inclination of the orbit of the comet, these functions P, Q, and R, in the problem before us, must be whole and rational functions of sin u and cos u, as also those which arise in the disturbing function. But since the nature of P, Q, and R depends on the choice of coordinates, the choice is by no means a matter of indifference. The examination of the different known expressions for the differentials of the coordinates shows that we cannot choose the *true* longitude and the radius-vector as the coordinates in the problem before us, since for these the functions P, Q, and R are not whole and rational functions of sin u and cos u. If on the contrary we so arrange the disturbances that they must be added to the *mean* longitude, and to the computed elliptic value of the logarithm of the radius-vector, by help of the disturbed mean longitude, or, which is the same thing, by help of the disturbed true anomaly, then have the functions P, Q, and R the property required. The author has, in his treatise on perturbations, made these disturbances dependent on an expression which he has designated by T. If then we take this and substitute in it the above differential coefficients of Ω, the eccentric anomaly u, and the analogous quantity depending on τ (to be denoted by v), it is reduced to the following:—

$$\begin{aligned}\mathrm{T}\,dt = \sqrt{1-e^2}\,\{&3\sin u - \tfrac{1}{2}\,e\sin 2u - 3\sin v + e\sin(v-u) \\ &+ e\sin(v+u) + \sin(v-2u)\}\,\frac{a}{\sqrt{1-e^2}}\left(\frac{d\Omega}{dx}\right)du \\ + \Big\{\tfrac{3}{2}\,e - (3-e^2)&\cos u + \tfrac{1}{2}\,e\cos 2u + 3\cos v - 3e\cos(v-u) \\ &- e\cos(v+u) + \cos(v-2u)\Big\}\,\frac{a}{\sqrt{1-e^2}}\left(\frac{d\Omega}{dy}\right)du,\end{aligned}$$

where it is evident that this quantity has the requisite property. From the *Fundamenta nova investigationis* it follows, that if we take account only of the first power of the disturbing force, we get from T the disturbances of the mean longitude and the corresponding disturbances of the log. of the radius-vector in the following manner. Compute the value of

$$\mathrm{W} = \int \mathrm{T}\,dt,$$

in which integration v must be treated as constant. Hence we get

$$n\,\delta z = n\int \overline{\mathrm{W}}\,dt; \qquad w = -\frac{1}{2}\int \overline{\left(\frac{d\mathrm{W}}{dv}\right)}\,du,$$

where the stroke over the W and its differential coefficients shows that for this integration v must be changed into u. The constants to be added to these integrations are here for the sake of shortness omitted. In the same work it is shown how to proceed in the calculation of the disturbances depending on the squares, and so forth, of the disturbing force, which method can be employed without important change in the problem before us. To obtain the disturbances of latitude the author employs the elements p_1 and q_1, explained in the same work, whose differential expressions are the following:—

$$\frac{dp_1}{dt} = \frac{n\,a\cos i}{\sqrt{1-e^2}}(e - \cos u)\left(\frac{d\Omega}{dz}\right)$$

$$\frac{dq_1}{dt} = -\,n\,a\cos i\sin u\left(\frac{d\Omega}{dz}\right),$$

where i is the inclination of the orbit of the comet to any arbitrary fundamental plane. We see that here also the functions by which the differential coefficients of Ω are multiplied possess the required condition. After integrating these expressions we obtain the disturbance of the latitude δs by the following expression:—

$$\delta s = \delta q_1 \sin f - \delta p_1 \cos f;$$

from whence it follows that δs cannot be expressed by rapidly converging series, since $\sin f$ and $\cos f$ are not whole functions of $\sin u$ and $\cos u$; we must then let f be replaced by u, which however the application of the disturbances will render difficult. But if the preceding equation be multiplied by r, then

$$r\,\delta s = \delta q_1\, a \sqrt{1-e^2} \sin u - \delta p_1\, a (\cos u - e)$$

in which the required condition is fulfilled. This expression is never inconvenient, since it is easy, by the application of the disturbances according to their numerical values, to divide this by the numerical value of r. But it frequently happens that we employ for the computation of the heliocentric places formulæ which require $r\,\delta s$, and in this case the foregoing expression is the most convenient. We can, moreover, by tabulating the disturbances, employ the preceding expression for δs, which requires f in the place of u, and make also tables for δs.

For carrying out the expansions pointed out in the preceding, the differentials to be integrated consist of terms which are partly of the form

$$n\,a \left\{ \begin{matrix} \sin \\ \cos \end{matrix} \right\} (i u + i' g' + \mathrm{A})\, d t,$$

and partly of the form

$$a \left\{ \begin{matrix} \sin \\ \cos \end{matrix} \right\} (i u + i' g' + \mathrm{A})\, d u,$$

where a and A are independent of t and u. The former of these forms can in two ways be reduced to the latter. We have namely in the first place,

$$n\,d t = (1 - e \cos u)\, d u,$$

and by the substitution of this expression there results

$$n a \int {\textstyle\cos \atop \textstyle\sin} (i u + i' g' + \mathrm{A})\, d t = a \int {\textstyle\cos \atop \textstyle\sin} (i u + i' g' + \mathrm{A})\, d u$$
$$- \frac{1}{2} e\, a \int {\textstyle\cos \atop \textstyle\sin} (i + 1) u + i' g' + \mathrm{A})\, d u$$
$$- \frac{1}{2} e\, a \int {\textstyle\cos \atop \textstyle\sin} (i - 1) u + i' g' + \mathrm{A})\, d u.$$

In the second place we may effect the reduction by integration by parts. This gives

$$n a \int {\textstyle\cos \atop \textstyle\sin} (i u + i' g' + \mathrm{A})\, d t = \pm \frac{a}{i' \nu} {\textstyle\sin \atop \textstyle\cos} (i u + i' g' + \mathrm{A})$$
$$- \frac{i\,a}{i' \nu} \int {\textstyle\cos \atop \textstyle\sin} (i u + i' g' + \mathrm{A})\, d u,$$

where $\nu = \dfrac{n'}{n}$.

It is thus only necessary to consider the second form. Looking to the essential simplifying condition of this form $i' = 0$, its integral has the following form:—

$$\begin{aligned} a\int \cos(iu + i'g' + \mathrm{A})\, du = a\alpha_i \sin(iu + i'g' + \mathrm{A}) \\ + a\alpha_{i+1} \sin((i+1)u + i'g' + \mathrm{A}) \\ + a\alpha_{i+2} \sin((i+2)u + i'g' + \mathrm{A}) + \&c. \\ + a\alpha_{i-1} \sin((i-1)u + i'g' + \mathrm{A}) \\ + a\alpha_{i-2} \sin((i-2)u + i'g' + \mathrm{A}) + \&c.; \end{aligned}$$

and it is shown in the memoir in question that the determination of the factors of integration α_i, α_{i+1}, α_{i-1}, &c., depends on two rapidly converging continued fractions. These are,—

$$\frac{\alpha_{i+1}}{\alpha_i} = \cfrac{1}{\cfrac{i+1+i'\nu}{\lambda} - \cfrac{1}{\cfrac{i+2+i'\nu}{\lambda} - \cfrac{1}{\cfrac{i+3+i'\nu}{\lambda} - \&c.}}}$$

$$\frac{\alpha_{i-1}}{\alpha_i} = \cfrac{1}{\cfrac{i-1+i'\nu}{\lambda} - \cfrac{1}{\cfrac{i-2+i'\nu}{\lambda} - \cfrac{1}{\cfrac{i-3+i'\nu}{\lambda} - \&c.,}}}$$

wherein, for brevity, there has been put

$$\lambda = \frac{1}{2} e i' \nu.$$

When from these two continued fractions the numerical values of $\frac{\alpha_{i+1}}{\alpha_i}$ and $\frac{\alpha_{i-1}}{\alpha_i}$ have been computed, and there has been found

$$\frac{\alpha_{i+1}}{\alpha_i} = p, \quad \frac{\alpha_{i-1}}{\alpha_i} = q,$$

then

$$\alpha_i = \frac{1}{i + i'\nu - \lambda p - \lambda q}.$$

The same continued fractions serve to calculate the value of the ratio of any two consecutive factors of integration, and consequently these are all given. The integral

$$a\int \sin(iu + i'g' + \mathrm{A})\, du$$

leads to an expression in which the factors of integration are the same, but have all contrary signs, and in which are found the cosines instead of the sines. The process arising from the preceding expressions is very simple, and in the memoir is besides explained by means of a detailed example.

The expansion of the disturbances produced by the reaction of the planets on the sun, according to the principles laid down in what has preceded, occupies the remainder of the memoir.

Article XXVII.

Results of the Magnetic Observations in Munich during the period of three years, 1840, 1841, 1842. By Dr. J. Lamont*.

[From the Transactions of the Royal Bavarian Academy of Sciences.]

A PORTION of the period of three years during which, according to the original design, the two-hourly magnetic observations were to be continued, was consumed in Munich, as well as in most other places, in the construction and establishment of the observatory. Early in August 1840 all the preliminary arrangements were completed, and the regular observations were commenced; but subsequent experiments showed, that in order to obtain correct determinations, conditions and precautions, previously unrecognised by any one, were requisite. In consequence of this circumstance the declination results previous to July 1841, and the intensity results previous to November of the same year, must be regarded as undeserving of confidence, and have therefore been omitted in the following account, with the exception of the mean declination, in which the errors of the single results may probably have compensated each other.

Diurnal Movement of the Declination.

The first of the following tables shows the diurnal movement of the declination as given by the monthly means. On a closer inspection, however, we may perceive that this march is considerably disfigured by the influence of the days of disturbance; and Table II. has therefore been formed for the purpose of exhibiting the same monthly means, with the omission in the calculation of the days of disturbance. Even after this has been done the corresponding months of 1841 and 1842 present considerable differences, so that, even setting aside disturbances, thirty days appear insufficient to eliminate the irregularities, and to afford a correct representation of the regular diurnal movement; and it is probable that the combination of the results of several years will be required for this purpose. In the meantime I have attempted to reduce the irregularities by taking

* Communicated by Lieut.-Colonel Sabine.

sixty days (two months), of which the results are exhibited graphically in the accompanying Plate, in which the hours of the day are indicated by dotted, and those of the night by full lines.

We see that the diurnal movement differs considerably in summer and in winter. In summer the march towards the west begins between 7 and 8 A.M., and attains its maximum at 1 P.M., or very soon afterwards. Thence it retrogrades until about midnight, when a small westerly movement takes place until between 2 and 3 A.M., when it again retrogrades until between 7 and 8 A.M.

These leading characteristics may also be recognised in the winter months, but the movement during the day is less at that season, and the nightly movement is greater; the turning points are also earlier in the morning and later at midday and in the night.

Besides the principal turning points there are other smaller inflections, particularly during the night, which may be recognised in several and sometimes in all the months; a remarkable outward inflection of the curves usually occurs at sunset.

It does not however appear desirable to found further conclusions or calculations on the data at present possessed; we should rather await the results of subsequent observations, and avail ourselves, in respect to these, of the indications afforded by the numbers before us. It should be noticed, in particular, that it is not sufficient to note the declination at every even hour; it appears almost necessary to take every hour, except perhaps 5, 9, 11 A.M.

If observations are made at every even hour, and at part only of the intermediate hours, the former only can be employed in the calculation of the mean declination. The mean of the even hours departs a little from the true mean (given by the quadrature of the daily curve), but never materially, as may be seen by the following table of corrections of the means of the even hours to the true means.

Jan. 1.	+ 0′·03	May 1.	0′·00	Sept. 1.	0′·00
Feb. 1.	+ 0·01	June 1.	0·00	Oct. 1.	0·00
March 1.	− 0·02	July 1.	− 0·02	Nov. 1.	+ 0·02
April 1.	− 0·02	Aug. 1.	0·01	Dec. 1.	+ 0·01

We may hence infer that for obtaining mean values only two-hourly observations are quite sufficient.

Mean Declination, its Yearly Diminution and Monthly Fluctuation.

A high or a low state of the barometer and thermometer usually prevails for several days, and often for half a month together: but with the magnetic declination (which is otherwise subject to as many accidents as atmospheric pressure and temperature) this is not the case; the movement on one day may differ greatly from that on another, but the daily means do not on that account differ considerably from each other.

Table V. contains the mean declination for periods of ten days, and the yearly decrease of declination obtained by subtracting each ten-day mean from the corresponding determination of the previous year. We see that the values found for the yearly decrease present differences of some magnitude, but not apparently governed by any law. This is also seen in Table VI., which exhibits the monthly means.

Combining the observations in periods of four months, we obtain the following general view:—

		West Declination.	Diff.
		° ′	′
1840.	Aug.—Nov.	16 59·3	
			2·1
1840–41.	Dec.—March	... 57·2	
			2·8
	April—July	... 54·4	
			2·7
	Aug.—Nov.	... 51·7	
			1·6
1841–42.	Dec.—March	... 50·1	
			1·8
	April—July	... 48·3	
			2·7
	Aug.—Nov.	... 45·6	

These numbers give no reason to assume an annual period; they show, on the other hand, that the secular decrease is not uniform but has periods. Present observations do not yet determine whether these periods are subject to a law or are accidental.

Diurnal Movement of the Horizontal Intensity.

Table III. shows the march of the horizontal intensity as given by the monthly means. In order to eliminate irregularities, a second calculation has been made omitting the days of disturbance, and the results are exhibited in Table IV. The march represented in the accompanying Plate is obtained from the combination of periods of sixty days. The leading characteristics of the intensity curves are,—minimum between 9 and

11 A.M., then increase until between 12 and 4 P.M.; then follows a wave-like course, in which the summits of four waves may be distinguished; the first of these is quite short, and its highest point occurs in the summer months immediately after 3 P.M., and in the remaining months rather earlier; the summit of the second is on an average at 8 P.M.; the third shows a small rise between midnight and 2 A.M., and the fourth usually occurs about the time of sunrise.

The mere aspect of the curves of intensity, as well as those of declination, is sufficient to show that the diurnal march of the magnetic variations does not depend on the sun only; it appears highly probable that regular oscillations of the magnetic elements take place independently of the influence of the sun.

It should be remarked, that the magnets employed have been constantly losing force in a ratio not yet determined with sufficient precision to be introduced into the calculation; the uncertainty in the determination of the diurnal march due to this cause is however limited within very narrow bounds.

By combining the variations of the declination with those of the horizontal intensity, we obtain the configurations in the accompanying Plate, where the movement in the day is represented by dotted, and in the night by continuous lines.

It is not improbable that the total force of terrestrial magnetism may remain unchanged, and that the variations may relate to direction only, so that the fluctuations of the inclination would be given by those of the horizontal intensity. Under this supposition the drawing would represent the movement of the north end of a needle freely suspended in the direction of the earth's magnetic force.

We see that in summer the curve has some resemblance to two ellipses, the greater of which is passed over in the day, and the lesser in the night. The astronomical meridian is perpendicular to the major axes of both ellipses. In winter the curves become far more complicated, yet so that the transition in the form of the curve from one month to the next may be traced.

Absolute Horizontal Intensity, its Annual Period and Secular Change.

Table VII. gives the monthly means of the absolute horizontal intensity calculated from the two-hourly observations; the elements employed in the reduction (particularly the daily loss of magnetism in the deflecting magnets) still require some corrections, but these would not materially alter the final result.

In the month of May some particular disturbance appears to have taken place. It was probably instrumental, and, for the present at least, I think it better to make no use of the results of that month.

The comparison of November and December 1841 with the same months in 1842, gives a yearly increase of horizontal intensity of 0·0048, which, supposing the total intensity to remain unaltered, would correspond to a decrease of about 4′ in the inclination.

Applying this increase of intensity to the numbers in Table VII., and leaving out the month of May, we obtain the intensity reduced to the middle of 1842 as follows:—

1842.	Jan.—March	1·9326
	April—June.	1·9311
	July—September	1·9298
	October—December . . .	1·9317

An annual period appears clearly indicated, having its maximum in March and its minimum in September.

The movements of the intensity differ from those of the declination, in so far that the mean intensity (in analogy with the height of the barometer) continues for *several days* together greater, and then again less, while the mean declination on several successive days never seems to remain *constantly* above or below the true mean.

Term Observations.

It has been thought hitherto that term observations may be employed

a. For determining the diurnal movement.

b. For determining the mean declination.

c. For determining the mode and manner in which disturbances display themselves simultaneously at different places.

The last-named object has not been attained by this class of observations, because no great disturbance has yet occurred during a term, and indeed the probability of such a coincidence is but small *.

[* At Toronto one of the greatest disturbances which have been yet observed took place during the term-day of May 1840: simultaneous observations made by Captain Ross's expedition at Kerguelen Island show that it extended

There are no movements in the diurnal march which are not sufficiently determined by hourly observations; those taken intermediately show us rapidly passing irregularities, but contribute nothing to the determination of the regular movements.

The following table shows how far the terms can be successfully applied to the determination of the mean declination, and of its annual variation. In this table the mean of the two-hourly observations of the thirty preceding and thirty following days is taken as the *true declination.*

Term.	True Declination.	Declination deduced from			Correction of the results.		
		The whole term.	Every hour.	The even hours.	The whole term.	Every hour.	The even hours.
1840.							
Aug. ...	16° 60′·36	59′·54	59′·50	57′·90	+ 0′·82	+ 0′·86	+ 2′·46
Sept. ...	59·24	59·57	59·97	60·55	− 0·33	− 0·73	− 1·31
Oct.......	58·77	59·18	59·07	59·72	− 0·41	− 0·30	− 0·95
Nov. ...	58·07	57·97	57·71	57·69	+ 0·10	+ 0·36	+ 0·28
Dec.......	57·47	55·61	55·72	55·60	+ 1·86	+ 1·75	+ 1·87
1841.							
Feb.......	56·93	57·08	56·90	56·06	− 0·15	+ 0·03	+ 0·87
April ...	55·64	54·60	54·87	54·94	+ 1·04	+ 0·77	+ 0·70
May......	55·56	53·87	53·69	53·50	+ 1·69	+ 1·87	+ 2·06
June ...	53·37	54·74	54·71	54·63	− 1·37	− 1·34	− 1·26
July......	53·25	53·73	53·84	54·01	− 0·48	− 0·59	− 0·76
Aug. ...	52·47	52·85	52·74	53·11	− 0·38	− 0·27	− 0·64
Sept. ...	51·72	51·70	51·65	51·84	+ 0·02	+ 0·07	− 0·12
Oct.......	50·90	49·00	49·11	48·72	+ 1·90	+ 1·89	+ 2·18
Nov. ...	51·07	51·20	51·23	50·65	− 0·13	− 0·16	− 0·42
Dec. ...	50·60	50·65	50·40	50·94	− 0·05	+ 0·20	− 0·34

The probable errors of the means for the whole term, for every hour, and for the even hours, are to each other as

1·0 : 1·0 : 1·4.

We see that it is quite indifferent for the determination of the mean declination whether observations are taken every hour or every five minutes. The somewhat greater amount of the probable error of the mean of the even hours can only be regarded as accidental; for if we were to omit a few days on which the greatest deviations took place, the difference would nearly disappear.

What has been said of the declination is also true of the intensity, except that the deviation of the several days from the mean is much greater in the intensity than in the declination.

to the southern hemisphere: it appears also to have been a day of very great disturbance both in the Declination and Horizontal Intensity at Prague. The term-day of August 1840 was also a day of considerable disturbance, showing itself in both hemispheres.—E. S.]

TABLE I.

Diurnal March of the Declination according to the Monthly Means, in Scale Divisions; the value of one Scale Division is 1′·05.

	A.M.									P.M.								
	2h	4h	6h	7h	8h	9h	10h	11h	12h	1h	2h	3h	4h	5h	6h	8h	10h	12h
1841.																		
July	0·97	0·97	0·20	0·04	0·00	1·69	3·59	5·83	8·26	9·59	9·32	8·30	6·82	5·26	3·82	2·30	2·02	2·03
Aug.	0·56	1·43	0·15	0·00	0·66	1·92	3·91	6·71	8·96	10·05	9·40	7·97	5·44	4·20	2·49	1·90	0·84	0·13
Sept.	0·80	1·88	2·44	1·43	0·73	1·64	3·85	6·34	8·74	9·09	8·75	7·10	3·91	3·17	2·49	1·16	0·00	0·19
Oct.	1·99	1·88	2·32	2·09	1·39	1·70	3·52	6·03	7·43	7·89	6·92	4·72	4·59	1·99	1·40	0·79	0·00	0·34
Nov.	1·71	2·26	3·58	3·11	3·09	3·03	3·97	5·54	6·55	6·62	6·01	4·12	2·27	3·50	2·21	0·00	0·79	0·63
Dec.	2·76	2·88	2·88	2·87	2·97	2·59	3·72	4·84	5·30	5·72	4·91	4·39	2·60	2·72	1·42	0·83	0·00	1·24
1842.																		
Jan.	2·16	2·40	2·36	2·36	2·24	2·66	3·49	4·42	5·35	5·72	4·46	3·67	3·37	3·24	2·92	1·45	0·00	0·77
Feb.	0·31	0·96	1·07	1·46	1·54	1·79	2·63	4·00	5·49	6·05	5·74	4·91	3·80	2·18	2·84	1·04	0·39	0·00
March	1·24	1·22	1·29	0·49	0·08	0·88	2·92	5·28	7·31	8·02	7·08	5·99	4·27	3·29	2·92	1·39	0·00	0·40
April	1·23	1·28	1·80	0·49	0·00	0·75	3·11	5·88	8·54	9·84	9·19	7·50	5·48	4·05	3·00	1·98	1·41	1·54
May	2·28	1·72	0·47	0·00	0·29	1·67	3·65	6·61	8·96	9·16	8·71	7·40	6·05	4·92	3·51	3·29	2·97	2·55
June	2·36	1·69	0·63	0·05	0·00	1·07	3·02	5·66	7·89	9·31	9·47	8·27	7·59	6·18	4·91	2·72	2·80	1·73
July	1·90	1·75	1·46	0·00	0·10	1·13	3·13	5·22	7·19	8·08	8·38	7·92	6·63	5·49	4·57	3·13	1·04	1·17
Aug.	1·67	0·81	0·42	0·00	0·30	1·54	4·20	6·20	7·92	8·90	8·09	6·73	4·98	3·65	2·52	2·55	1·82	1·59
Sept.	0·74	0·74	0·55	0·00	0·25	1·76	3·82	5·86	7·56	7·60	6·61	5·02	3·18	2·28	2·05	0·24	0·68	0·20
Oct.	2·38	2·18	2·12	1·14	0·25	0·52	2·55	4·92	6·35	6·96	6·47	4·83	4·03	2·82	1·80	1·03	0·00	0·81
Nov.	1·72	2·57	2·33	1·99	1·91	1·93	2·83	4·37	5·36	5·68	4·38	3·40	2·55	2·39	2·24	1·17	0·00	0·83
Dec.	1·27	1·76	1·46	1·35	1·30	1·36	2·17	2·90	3·65	3·98	3 42	2·71	2·22	1·79	1·28	0·28	0·00	0·77

TABLE II.

Diurnal March of the Declination according to the Monthly Means, omitting days of disturbance. 1 Scale Division = 1′·05.

	A.M.									P.M.								
	2h	4h	6h	7h	8h	9h	10h	11h	12h	1h	2h	3h	4h	5h	6h	8h	10h	12h
1841.																		
July	0·9	0·9	0·2	0·0	0·0	1·6	3·5	5·8	8·2	9·5	9·3	8·3	6·8	5·2	3·8	2·3	2·0	2·0
Aug.	1·2	1·3	0·1	0·0	0·3	2·1	3·8	6·6	8·8	9·8	9·0	7·7	5·0	4·0	2·8	1·9	1·1	0·5
Sept.	1·3	0·9	1·5	0·8	0·0	0·8	2·9	5·6	7·7	8·0	7·3	5·9	3·5	2·3	1·5	0·7	0·1	0·4
Oct.	3·0	2·3	2·4	2·1	1·6	1·9	4·0	6·6	7·9	8·3	7·6	5·0	4·8	2·7	1·8	0·0	0·6	1·4
Nov.	1·8	2·1	2 8	2·4	2·2	2·2	3·2	4·8	5·9	6·0	5·4	3·5	2·6	2·8	1·6	0·0	0·4	0·9
Dec.	2·7	2·8	2·8	2·8	2·9	2·5	3·7	4·8	5·2	5·6	4·8	4·2	2·4	2·5	1·3	1·0	0·0	1·3
1842.																		
Jan.	2·2	2·5	2·5	2·5	2·4	2·8	3·6	4·6	5·5	5·8	4·5	3·8	3·5	3·4	3·0	1·5	0·0	1·3
Feb.	0·9	0·6	0·9	0·9	0·8	1·0	1·8	3·2	4·6	5·0	4·9	3·9	2·8	1·6	1·8	0·0	0·5	0·0
March	1·4	1·3	1·4	0·6	0·0	0·6	2·9	4·9	6·9	7·6	7·2	5·9	4·0	3·0	2·7	1·4	0·4	0·7
April	1·4	1·2	1·4	0·5	0·0	0·7	3·1	5·8	8·4	9·7	9·0	7·3	5·4	4·1	3·1	2·2	1·5	1·6
May	2·8	1·9	0·6	0·1	0·0	1·8	3·8	6·4	8·3	8·9	8·6	7·3	6·1	5·0	3·7	3·4	3·1	2·6
June	3 0	2·0	0·4	0·2	0·0	1·0	3·0	5·5	7·8	9·3	9·5	8·2	7·5	6·0	4·9	3·3	3·0	2·5
July	2·5	1·8	0·3	0·0	0·4	1·5	3·1	5·7	7·4	8·7	9·0	8·4	7·1	5·8	5·0	3·5	2·2	1·8
Aug.	1·7	0·8	0·4	0·0	0·3	1·6	4·2	6·2	7·9	8·9	8·1	6·8	5·0	3·7	2·5	2·6	1·8	1·6
Sept.	0·6	0·6	0·5	0·1	0·2	1·5	3·7	5·7	7·4	7·5	6·5	4·7	3·1	2·1	1·9	0·6	0·6	0·0
Oct.	2·4	2·2	2·2	1·1	0·3	0·6	2·6	5·0	6·4	7·0	6 5	4·9	4·1	2·9	1·8	1·1	0·0	0·8
Nov.	1·5	2·2	2·0	1·7	1·5	1·6	2·4	3·8	4·9	5·1	4·1	3·0	2·6	2·0	1·8	0·7	0·0	1·2
Dec.	1·3	1·8	1·5	1·3	1·3	1·4	2·2	2·9	3·6	4·0	3·4	2·7	2·2	1·8	1·3	0·3	0·0	0·8

TABLE III.

Diurnal Movement of the Horizontal Intensity according to the Monthly Means, expressed in Scale Divisions.
1 Scale Division = 0·00011967 of the Horizontal Intensity.

	A.M.									P.M.								
	2h	4h	6h	7h	8h	9h	10h	11h	12h	1h	2h	3h	4h	5h	6h	8h	10h	12h
1841.																		
Nov.	3·22	4·66	4·50	4·34	4·55	3·23	1·74	0·02	0·00	1·57	0·94	0·34	0·45	2·16	2·44	2·59	1·62	1·63
Dec.	2·63	4·14	5·58	5·80	5·77	5·01	3·65	2·20	4·07	3·98	1·67	0·68	0·80	1·00	0·00	0·30	2·24	1·92
1842.																		
Jan.	0·54	1·64	3·32	4·14	3·40	1·68	0·00	0·13	1·79	2·30	2·79	2·02	1·17	0·92	1·42	1·39	1·02	0·44
Feb.	2·83	3·42	4·27	4·33	3·53	2·39	0·69	0·00	0·39	1·17	2·18	1·38	0·94	0·33	1·87	2·99	1·53	2·10
March	5·44	5·80	5·61	4·39	3·01	0·60	0·00	0·47	2·41	3·97	5·26	5·99	5·00	4·64	5·61	5·71	5·70	4·50
April	11·58	10·21	9·16	7·14	4·36	1·92	0·61	0·00	3·46	7·48	8·79	11·64	11·38	11·06	11·71	15·08	12·90	13·52
May	7·36	6·44	4·99	3·19	1·09	0·00	0·52	3·61	5·20	5·71	6·84	8·55	7·74	17·23	9·11	9·56	9·62	8·39
June	8·53	9·91	3·82	1·12	0·30	0·25	0·00	2·25	4·81	6·49	9·15	11·40	11·56	10·93	11·85	11·28	11·45	9·62
July	12·02	10·70	7·50	5·48	2·36	0·00	1·58	3·24	5·08	6 86	10·36	11·34	13·13	13·68	14·34	15·56	14·02	12·45
Aug.	10·23	8·91	7·39	2·98	0·58	0·00	1·35	2·54	6·61	9·73	10·26	11·11	11·67	10·52	11·81	15·16	15·37	13·02
Sept.	10·14	9·58	7·28	4·73	1·97	0·80	0·00	2·04	6·36	9·19	10·93	10·89	9·57	9·96	11·29	13·76	12·18	11·64
Oct.	8·52	9·24	8·87	7·60	4·82	2·23	0·00	0·71	2·61	4·62	6·46	5·74	5·57	6·99	7·65	8·37	9·62	8·43
Nov.	5·35	6·32	5·88	5·61	4·66	2·54	0·00	0·12	0·66	1·20	1·05	1·09	1·17	2·35	3·64	5·53	3·71	4·33
Dec.	1·46	2·86	4·12	3·78	3·09	1·18	0·00	0·00	0·41	1·27	1·16	1·69	1·32	1·31	2·00	0·76	0·93	1·56

TABLE IV.

Diurnal March of the Horizontal Intensity according to the Monthly Means, omitting days of disturbance; expressed in Scale Divisions, each being 0·00011967 of the Horizontal Intensity.

	A.M.									P.M.								
	2h	4h	6h	7h	8h	9h	10h	11h	12h	1h	2h	3h	4h	5h	6h	8h	10h	12h
1841.																		
Nov.	1·3	2·2	3·9	3·3	3·8	2·4	1·6	1·1	0·2	1·3	0·6	0·0	0·5	1·0	1·3	0·3	1·3	0·6
Dec.	0·0	2·0	3·1	3·0	2·8	2·2	0·9	0·9	2·1	2·2	0·9	1·7	1·3	2·0	1·3	0·8	1·3	0·2
1842.																		
Jan.	0·9	1·8	3·4	4·2	3·5	1·8	0·0	0·2	1·4	2·1	2·4	1·7	1·0	0·9	1·4	1·3	0·8	0·2
Feb.	3·3	3·8	4·4	4·4	3·4	2·3	0·5	0·0	0·2	0·9	1·6	1·0	1·2	0·7	2·4	3·9	2·5	2·0
March	6·1	6·4	6·2	5·0	3·7	1·3	0·0	0·8	2·9	4·2	6·0	5·9	5·7	6·5	6·6	6·3	6·1	5·4
April	9·2	6·9	7·3	5·7	3·1	1·3	0·7	0·0	3·5	6·5	8·5	10·4	11·2	10·0	10·9	13·2	10·6	11·1
May	7·3	5·9	5·0	3·1	0·9	0·0	1·0	3·1	4·9	6·7	8·3	9·7	8·9	8·5	9·9	10·0	9·9	8·6
June	9·4	7·7	4·8	3·0	1·4	0·0	0·1	2·3	4·7	6·5	8·3	11·1	10·9	10·5	11·2	12·8	12·3	10·3
July	9·2	8·4	5·2	3·8	1·6	0·0	0·3	1·6	3·4	5·5	8·2	9·4	11·0	10·3	10·9	12·4	12·0	9·4
Aug.	9·1	7·1	5·8	3·2	0·0	0·0	0·1	1·8	5·4	8·5	9·4	9·6	10·1	10·0	10·8	14·3	13·8	11·2
Sept.	10·0	9·2	7·6	5·2	2·3	0·8	0·0	2·1	6·2	9·4	10·7	11·0	10·0	10·4	11·7	13·3	13·3	12·2
Oct.	7·5	8·0	7·6	6·5	4·3	4·4	0·0	0·1	2·0	4·2	5·7	5·7	6·2	7·7	8·4	8·7	8·3	8·3
Nov	3·4	3·5	4·7	4·8	3·5	1·6	0·0	0·3	0·6	1·3	1·3	1·2	1·8	2·2	3·3	4·3	3·4	3·6
Dec.	1·4	2·8	4·1	3·9	3·2	1·0	0·0	0·1	0·9	1·6	1·7	2·1	1·8	1·7	2·0	1·9	1·6	2·0

Table V.

Mean Declination from ten days to ten days deduced from two-hourly observations.

Time.		Mean Declination.	Difference from the preceding year.
1840.			
Aug.	1 to 10......	18° 60'·6	'
	11—20......	60·4	
	21—31......	60·3	
Sept.	1—10......	60·3	
	11—20......	59·4	
	21—30......	59·2	
Oct.	1—10......	58·3	
	11—20......	58·7	
	21—31......	59·1	
Nov.	1—10......	58·7	
	11—20......	58·5	
	21—30......	58·2	
Dec.	1—10......	58·3	
	11—20......	57·3	
	21—31......	57·0	
1841.			
Jan.	1—10......	56·8	
	11—20......	58·8	
	21—31......	57·4	
Feb.	1—10......	56·7	
	11—20......	58·0	
	20—28......	56·6	
March	1—10......	56·8	
	11—20......	56·5	
	21—31......	57·0	
April	1—10......	56·6	
	11—20......	55·8	
	21—30......	55·1	
May	1—10......	54·6	
	11—20......	55·1	
	21—31......	54·6	
June	1—10......	53·5	
	11—20......	53·2	
	21—30......	53·8	
July	1—10......	54·0	
	11—20......	52·9	
	21—31......	53·1	
Aug.	1—10......	53·0	7·6
	11—20......	52·6	7·8
	21—31......	53·3	7·0
Sept.	1—10......	52·6	7·7
	11—20......	51·6	7·8
	21—30......	51·0	7·6
Oct.	1—10......	51·1	7·2
	11—20......	50·6	8·1
1841.			
Oct.	21—31......	18° 50'·4	8'·7
Nov.	1—10......	51·4	7·3
	11—20......	51·5	7·0
	21—30......	51·2	7·0
Dec.	1—10......	50·5	7·8
	11—20......	50·9	6·4
	21—31......	50·5	6·5
1842.			
Jan.	1—10......	50·6	6·2
	11—20	50·1	8·7
	21—31......	50·9	7·5
Feb.	1—10......	50·5	6·2
	11—20......	49·8	8·2
	21—28......	49·1	7·5
March	1—10......	49·3	7·5
	11—20......	49·9	6·6
	21—31......	50·2	6·8
April	1—10......	48·9	7·7
	11—20......	48·8	7·0
	21—30......	49·1	6·0
May	1—10......	49·0	5·6
	11—20......	49·0	6·1
	21—31......	47·8	6·8
June	1—10......	47·9	5·6
	11—20......	48·5	4·7
	21—30......	48·4	5·4
July	1—10......	49·0	5·0
	11—20......	46·9	6·0
	21—31......	46·4	6·7
Aug.	1—10......	46·2	6·8
	11—20......	46·6	6·0
	21—31......	46·7	6·6
Sept.	1—10......	46·0	6·6
	11—20......	45·9	5·7
	21—30......	45·7	5·3
Oct.	1—10......	45·6	5·5
	11—20......	45·7	4·9
	21—31......	45·3	5·1
Nov.	1—10......	44·6	6·8
	11—20......	44·4	7·1
	21—30......	44·5	6·7
Dec.	1—10......	43·7	6·8
	11—20......	43·9	7·0
	21—31......	43·5	7·0

Table VI.

Monthly Means of the Declination deduced from two-hourly observations.

	1840.	1841.	1842.
	° ′	° ′	° ′
January	......	16 57·60	16 50·18
February............	......	56·98	49·84
March...............	......	56·86	49·81
April	......	55·82	48·95
May	......	54·77	48·61
June	......	53·51	48·24
July..................	......	53·35	47·42
August	16 60·39	53·00	46·49
September	59·61	51·73	45·38
October	58·76	50·69	45·48
November	58·46	51·39	44·53
December	57·52	50·61	43·68

Table VII.

Monthly Means of the Horizontal Intensity in absolute measure, deduced from the two-hourly observations.

1841. November...	1·9295	1842. June	1·9304
December...	1·9305	July	1·9307
1842. January ...	1·9304	August	1·9297
February ...	1·9309	September...	1·9316
March	1·9318	October ...	1·9318
April.........	1·9311	November...	1·9338
May	(1·934$_6$)	December...	1·9356

[The subjoined extracts are from Dr. Lamont's *Annalen für Meteorologie und Erdmagnetismus*, Jahrgang 1842, Heft. iv.—E. S.]

1. *Remarks on Dr. Lloyd's Induction-inclinometer.*

A paper by Professor Lloyd has been referred to, in which he proposes to determine the absolute inclination and its variations by means of the magnetism induced in soft iron bars. The idea is so simple, and the experiments given by Professor Lloyd speak so advantageously for the applicability of iron bars, that I immediately began to construct an instrument on this principle. In doing so I did not employ with Professor Lloyd a vertical iron-bar east or west of the freely-suspended magnet, but I made use of two vertical bars in the plane perpendicular to the middle of the free magnet, one deflecting on one side with the south pole, the other on the other side with the north pole. My first experiments were noways satisfactory; the magnetism of the bar increased so steadily and uniformly, that it could not but be expected that it would come to a constant condition. At the same time the influence of temperature was considerable, but not always equal. It was only by degrees that I recognised the conditions to be attended to, and the method by which the object would be most surely attained. If it is desired to use an iron bar for a differential instrument for the inclination, it must be plunged fifteen or twenty times alternately into hot and cold water, without ever being brought out of its vertical position; the bar thus acquires a constant amount of permanent magnetism. Its relation to temperature is then examined. I have found that some bars increase and others diminish their magnetism with increasing temperature; my experiments are not yet sufficiently numerous to show on what conditions this depends, but the circumstance is a very advantageous one, because the bars may be so chosen that the influences of temperature may compensate each other.

The hourly observations of the 1st of February 1843, were commenced with an instrument constructed in this manner*. From the first it was striking how exactly the march of the inclination accorded with that of the intensity; and this relation continued unaltered in considerable disturbances. I concluded from hence, as M. Kreil had already done from his observations

* The instrument consists of a small needle weighing hardly a half gramme, and two iron bars 8 inches long, 6·4 lines broad, and 1·7 thick; the narrower side is turned towards the magnet, the distance is 33 lines, and the deflection is 44°; the needle carries a mirror 8 lines square, and vibrates in $2''.7$. It is so narrowly enclosed on all sides that it has only room to move just so far as the measurements require, which circumstance naturally causes the vibrations to diminish much more rapidly than would be the case if the needle was merely suspended under a bell-glass; the decrease, however, is only $\frac{1}{50}$th of the arc of vibration for each oscillation. With the small magnets used in this observatory for measurements of absolute intensity, and which are suspended under bell-glasses when determining their time of vibration, I have never determined the decrease of the arc of vibration; the small amount of the resistance of the air may, however, be inferred from the circumstance, that in the measurements before-mentioned the arc of vibration was about 20′ at the commencement, and that after 400 vibrations it was still sufficiently large for the passage of the needle to be observed with facility. In the registers of 1842 cases occur in which 800 and even 1000 vibrations were observed. I mention this because a singular opinion has sometimes been expressed, to the effect that small magnets are quieted by the resistance of the air, and that it is owing to this kind of quieting that instruments of the construction in use in this observatory remain free from oscillation.

at Prague, that the total intensity continues unchanged during disturbances, and that the movements observed are to be attributed solely to the altered direction.

Later observation appeared, however, to point to the necessity of admitting some, although small, alterations in the total intensity also: this showed itself particularly in the great disturbance of April 5–7, 1843.

The ratio of the changes of inclination and intensity may best be taken from a comparative view.

Diurnal March in February 1843.

Sun Easterly. Midnight to Noon.				Sun Westerly. Noon to Midnight.			
h.	Inclination.	Hor. Int.	Total Int.	h.	Inclination.	Hor. Int.	Total Int.
12	− 2·42	+ 3·83	+ 0·87	12	− 0·17	+ 0·65	+ 0·46
1	− 3·22	+ 3·24	+ 0·41	1	− 1·32	+ 2·01	+ 0·94
2	− 2·55	+ 2·36	+ 0·09	2	− 1·09	+ 2·36	+ 1·55
4	− 2·31	+ 1·90	− 0·18	3	− 0·55	+ 1·62	+ 1·40
6	− 2·76	+ 2·39	+ 0·01	4	− 0·20	+ 1·45	+ 1·54
7	− 3·10	+ 2·80	+ 0·04	5	− 0·04	+ 1·34	+ 1·57
8	− 3·15	+ 2·84	+ 0·03	6	− 0·00	+ 0·87	+ 0·97
9	− 1·77	+ 1·40	− 0·20	7	− 1·41	+ 2·89	+ 1·83
01	− 1·31	+ 1·00	− 0·19	8	− 0·89	+ 2·47	+ 1·88
11	− 0·49	+ 0·00	− 0·49	9	− 2·11	+ 3·70	+ 2·03
12	− 0·17	+ 0·65	+ 0·46	10	− 1·92	+ 3·24	+ 1·71
				12	− 2·42	+ 3·83	+ 0·87

A scale-division is equal in the inclination to 13″·1, and in the horizontal and total intensity to 0·00012 and 0·00013 of the whole value. The march of the total intensity is, we see, quite peculiar; a rapid alteration occurs at the time of the upper and of the lower culmination of the sun; throughout the rest of the day the intensity is more constant, and is less when the sun is easterly, and greater when it is westerly. No magnetic element appears to be so exactly, or at least so simply, connected with the course of the sun as the total force. We may form to ourselves an idea of the alterations which may occur during magnetic disturbances from the following:—On the 5th of April 1843, the declination needle passed through an arc of 20′, and the horizontal force and inclination needles through twenty-seven and twenty-four scale-divisions; while the total force was altered only by two divisions, but at midnight it decreased rapidly ten divisions, and was stationary during great fluctuations of the other elements until noon on the 6th of April, when it again rapidly increased. It then continued almost entirely without alteration during a highly disturbed march of the direction (the change of declination amounted to 25′, one of the greatest which we have yet observed), until the return of the lower culmination of the sun.

The numerical values for the inclination and the total intensity are, however, to be regarded as only a provisional approximation; they rest on the assumption that a scale-division corresponds to 13″·1 of inclination, as would be the case if the intensity of the magnetism of the iron and its variations were in the same proportion as the intensity of the vertical portion of the earth's magnetism and its variations. But as we are not justified, *à priori*, in making this assumption,—and as, moreover, the hitherto generally received supposition, that with the increase or decrease of the magnetic moment of an iron bar each element increases or decreases in the same ratio, stands yet in great need of confirmation,—we must determine the value of the scale-divisions by means of deflection in a manner similar to that which can be made use of in the horizontal intensity. I have not performed this operation, but I have, by means of a magnet, induced in the iron about as much magnetism as is equivalent to five times the greatest change in the earth's magnetism which has yet fallen under my notice, and I have found that, after the removal of the inducting magnet, the needle returned very soon to precisely its previous condition, so that in *small* changes the resistance of the iron may be regarded as vanishing, just as imperfectly elastic bodies, in *small* alterations of the compressing forces, may be regarded as perfectly elastic.

There appears, therefore, to remain no doubt that the induction of soft iron may be applied with equal ease and certainty to the measurement of the variations of the inclination. Mr. Lloyd has not succeeded in obtaining by its means a determination of the absolute inclination; the endeavours made in this view in the Munich Observatory, with I hope some success, not being yet completed, the account of them must be reserved for a future occasion.

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2. *Remarks on Dr. Lloyd's method of Determining the Absolute Horizontal Intensity.*

Dr. Lloyd has sent the editor his memoir entitled "On the Determination of the Earth's Magnetic Force in Absolute Measure." Dr. Lloyd's method may be characterised as follows:—

1. When the distance of the deflecting from the deflected magnet is four times, or more than four times, greater than the

length of the deflected bar, we may neglect, in the known series which expresses the ratio of the magnetism of the bar to that of the earth, all but the two first members.

2. But if we develope the two first members of the series under the assumption made by Biot for short magnets, (*i. e.* that the force of magnetism increases uniformly towards the extremities of the bar,) we may express the coefficients of the second member by known quantities, and do not require to eliminate them by measurements at different distances.

The method of determining the intensity here given by Professor Lloyd, which not only renders the labour much less, but also, as he shows, increases the exactness more than *fivefold*, would be of great value were it not for some circumstances which may excite a degree of doubt as to its success.

1. In regard to the proposition, that at distances exceeding four times the length of the magnet, only the two first members need be taken into account, this proposition has been repeated from the *Intensitas vis*, &c. in almost all recent writings treating of the measurement of the absolute intensity, and the series of deflections given by M. Gauss have been regarded as a proof of its correctness. On a closer examination, however, we will easily be convinced of the incorrectness of the proposition. We will take the expression given in p. 21 of my memoir "On the determination of the Absolute Intensity," viz.

$$\frac{M}{X} = \tfrac{1}{2} e^3 \sin \phi \frac{1}{1 + \frac{p}{e^2} + \frac{q}{e^4}},$$

or

$$\sin \phi = 2 \frac{M}{X} \frac{1}{e^3}\left(1 + \frac{p}{e^2} + \frac{q}{e^4}\right),$$

where

$$p = 2\frac{M_3}{M} - 3\frac{M'_3}{M'}, \quad q = 3\frac{M_5}{M} - 15\frac{M_3\, M'_3}{M\, M'} + \frac{45}{8}\frac{M'_5}{M'},$$

and

$$M = \Sigma\, V\, x\, d\, m\ M_3\, \Sigma\, V^3\, d\, m, \text{ \&c.}$$

Let $V\, d\, m$ denote a free magnetic element at the distance x from the middle of the bar, we have then

$$p = \frac{1}{2} l^2 - \frac{3}{4} l'^2 \text{ and } q = \frac{3}{16} l^4 - \frac{15}{16} l^2 l'^2 + \frac{45}{128} l'^4,$$

l being the length of the deflecting and l' of the free magnet. Thence, if $e = 4 \cdot l$ and $l' = l$, the third number $\frac{q}{e^4} = -0{\cdot}00155$.

But if we assume, with Mr. Lloyd, that the magnetic force increases uniformly towards the two extremities of the bar, *i. e.* that $V\, d\, m = A\, x\, d\, x$, we obtain

$$p = \frac{3}{20}(2\, l^2 - 3\, l'^2), \text{ and } q = \frac{9}{112}\, l^4 - \frac{27}{80}\, l^2\, l'^2 + \frac{133}{896}\, l'^4.$$

Then if $e = 4\, l$ and $l' = l$,

$$\frac{q}{e^4} = -\,0{\cdot}000415.$$

In the first case the error in the determination of $\frac{M}{X}$, by neglecting $\frac{q}{e^4}$, will have been $\frac{1}{642}$, in the second case $\frac{1}{2410}$. According to the most exact experiments hitherto made, the first hypothesis gives too large and the second too small a ratio, but at any rate we see that the third member must not be neglected in the development.

To show this experimentally, the following observations were made;

Distances.		Deflections.		
		°	′	″
2·99998	. . . .	61	23	19
4·00002	. . . .	21	48	49
5·00002	. . . .	10	58	19
5·99984	. . . .	6	19	21
6·99979	. . . .	3	58	31
7·99973	. . . .	2	39	45

The deflections are the means of two series of experiments reduced to equal intensity and equal temperature. If we put

$$\log 2\,\frac{M}{X} = \log e^3 \sin \phi + \frac{p'}{e^2} + \frac{q'}{e^4},$$

we find

$$p' = -\,0{\cdot}0295$$
$$q' = +\,0{\cdot}3600,$$

and the several deflections give the following results:—

$$\log 2\,\frac{M}{X} = 1{\cdot}37582$$

1·37585
1·37580
1·37581
1·13582
1·37583.

From the values of p' and q' we obtain

$$p = +0{\cdot}0679$$
$$q = -0{\cdot}8286;$$

and hence if the distance is equal to four times the length of the magnet,

$$\frac{q}{e^4} = -\frac{1}{1400}.$$

The error becomes much greater still if the deflecting bar is placed south and north.

Thus we see that the third member must not be neglected in the development.

In regard to the series of deflections given by M. Gauss, the errors of observation are too considerable (most of them exceed $\frac{1}{300}$ of the whole amount) for it to have been possible to prove anything thereby; they merely show that the deviations from the assumed law are not greater than the probable errors of observation *.

2. The law which Mr. Lloyd has assumed, after Biot, for the distribution of magnetism in the bar appears by no means experimentally confirmed. M. Muncke has related, in Gehler's Physical Dictionary (Art. Magnetism), a number of experiments, which all show that the magnetism increases from the middle but very slowly, and rapidly towards the extremities; no single experiment indicates a uniform increase towards both ends. The latter hypothesis gives the magnetic centre too far from the ends, whereby the calculated values of p and q are too small. In the above-mentioned series of experiments

$$p \text{ was found} = +\ 0{\cdot}0679$$
$$q \text{ was found} = -\ 0{\cdot}8286.$$

* In the expressions given by M. Gauss,

$$\tan v = 0{\cdot}086870\ \mathrm{R}^{-3} - 0{\cdot}002185\ \mathrm{R}^{-5}$$
$$\tan v' = 0{\cdot}043435\ \mathrm{R}^{-3} + 0{\cdot}002449\ \mathrm{R}^{-5},$$

the coefficients of R^{-5} are already imperfect, as the members multiplied by $\sin^2 v$ and $\sin^2 v'$ are neglected, namely,

$$30\ \frac{\mathrm{M}'_3}{\mathrm{M}'}\ \frac{\mathrm{M}}{\mathrm{X}} \sin^2 v, \text{ and } -\frac{45}{2}\ \frac{\mathrm{M}'_3}{\mathrm{M}_1}\ \frac{\mathrm{M}}{\mathrm{X}} \sin v'.$$

Under the here very nearly correct assumption that $\mathrm{M} = \mathrm{M}'$ and $\mathrm{M}_3 = \mathrm{M}_3'$, we shall have to add

To the first expression $+\ 0{\cdot}032775 \sin^2 v\ \mathrm{R}^{-5}$

To the second expression $-\ 0{\cdot}012245 \sin^2 v'\ \mathrm{R}^{-5}$.

The corrections of the greatest values of v and v' given by M. Gauss would amount according to this to $+\ 2''{\cdot}7$ and $-\ 1''{\cdot}8$.

According to Mr. Lloyd's hypothesis it would have been

$$p = +\ 0{\cdot}04053$$
$$q = -\ 0{\cdot}3237,$$

values which are wholly irreconcilable with the series of experiments.

It is not however exactly necessary to the success of the method given by Mr. Lloyd that the calculated values of p and q should be the true ones, but only that they should be in certain ratios to the true values. In order to decide whether for magnets of equal dimensions equal coefficients apply, I have made the following experiments with two small bars of equal size, &c., and magnetised in the same manner. The determinations are the mean of two series of observations in each deflection; the four angles were read; the distances I. and II. are to each other as 3 : 8.

Value of log e^3 sin ϕ.

Distance.	Magnet A.	Magnet B.
I	6·60909	6·61063
II	6·44407	6·44413

We see that it is out of the question to apply the same values of p and q for both cases; it is needful therefore to determine them by observations for each single magnet, and they cannot be calculated from the dimensions.

It may perhaps be useful to subjoin one more remark on the experiments adduced by Mr. Lloyd to prove the correctness of the assumed law. His series II. gives

$$\log 2\,\frac{\mathrm{M}}{\mathrm{X}} = 9{\cdot}52247 + \frac{p'}{1{\cdot}5^2} + \frac{q'}{1{\cdot}5^4}$$
$$= 9{\cdot}52244 + \frac{p'}{2^2} + \frac{q'}{2^4}$$
$$= 9{\cdot}52266 + \frac{p'}{2{\cdot}5^2} + \frac{q'}{2{\cdot}5^4};$$

as p' and q' have different signs, we may make

$$p' = n + m\, q' = -m,$$

and we then have

$$\log 2\,\frac{\mathrm{M}}{\mathrm{X}} = 9{\cdot}52247 + n\,0{\cdot}444 + m\,0{\cdot}246$$
$$= 9{\cdot}52244 + n\,0{\cdot}250 + m\,0{\cdot}188$$
$$= 9{\cdot}52266 + n\,0{\cdot}160 + m\,0{\cdot}134.$$

If we make $n = +0{\cdot}0040$ and $m = 0$, we obtain

$$9{\cdot}52264$$
$$9{\cdot}52254$$
$$9{\cdot}52272;$$

but if we make $n = -0{\cdot}00280$ and $m = +0{\cdot}00880$, we obtain

$$9{\cdot}52339$$
$$9{\cdot}52339$$
$$9{\cdot}52339.$$

If with Professor Lloyd we regard the agreement of the numbers as they are as sufficient, then in the mean

$$\log 2\frac{M}{X} = 9{\cdot}52252;$$

if we amend these numbers by a second member, the agreement becomes better, but the value becomes somewhat greater, namely,

$$\log 2\frac{M}{X} = 9{\cdot}52258;$$

lastly, if we add a third member we may make the accordance complete, but the value mounts up to

$$\log 2\frac{M}{X} = 9{\cdot}52339;$$

in a word, if the distances are chosen large, the amendments depending on the higher members may still be considerable*; but it is impossible to determine them with certainty, because the coefficients of m form a slowly decreasing series, and the values of $e^3 \tan\phi$ must be considerably increased or diminished to equate small differences. It is therefore absolutely necessary to choose smaller distances and greater angles of deflection.

It would still remain to remark that Mr. Lloyd has neglected the member

$$15\frac{M'_3}{M'}\sin^2\phi,$$

while in the cases adduced by him it amounted to $\frac{1}{710}$. But no method which leaves out such considerable quantities can lay claim to exactness. There would however be no difficulty in remedying this defect.

Besides the objections which relate to the determination of the constants, we may also mention, as against the facility of

* Mr. Lloyd's hypothesis gives

$$p = 0 \quad q = -0{\cdot}000655,$$

thence

$$p' = 0 \quad q' = -0{\cdot}000284;$$

application of the method developed by Mr. Lloyd, that the deflections do not amount to more than $6\frac{2}{3}°$, or 400 minutes; so that to obtain the intensity to $\frac{1}{4000}$ would require an exactness of a tenth of a minute in the angle, a degree of precision which it would be difficult to obtain with certainty in a single deflection. We are thus again brought back to the necessity of taking greater deflections.

Although, for these reasons, it is not probable that the method proposed by Mr. Lloyd will find an immediate advantageous application*, yet the memoir is of great interest, inasmuch as it developes a new method of determining the constants, which being improved by future modifications, may, even where not absolutely necessary, offer a most highly desirable check.

It may not be superfluous to examine here the question, at what distances the three members of the series suffice, and where it becomes necessary to take higher members into the calculation.

If we say, as above,

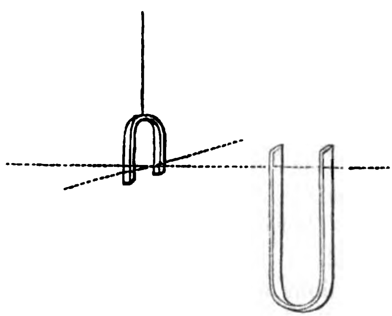
$$\frac{M}{X} = \frac{1}{2} e^3 \sin \phi \frac{1}{1 + \frac{q}{e^1} + \frac{q}{e^4} + \frac{r}{e^6}},$$

we find

$$r = 4\frac{M'_7}{M'} - 42\frac{M_3 M'_3}{M M'} + \frac{108}{2}\frac{M_3 M'}{M M'} - \frac{36}{4}\frac{M_7}{M'}.$$

If we assume the whole magnetism to be in the extremities of

* Mr. Lloyd's method would be strictly applicable if, instead of straight bar-magnets, horse-shoe magnets were used, thus:



The magnet would require to be thin and broad, the arms parallel, and the extremities of the arms, which (in analogy with the centre of gravity) may be termed magnetic centres, must be in the same horizontal plane. There would be no difficulty in the execution; but no material advantage would be gained thereby.

the magnets, calculation gives for the series of observations in page 218*,

$$p = +\ 0{\cdot}135$$
$$q = -\ 2{\cdot}360$$
$$r = -\ 1{\cdot}167$$

These coefficients are too large, and we come nearer the truth if we assume the magnetism to be concentrated in a point $\frac{1}{n}$ of the whole length from the end; we have then to multiply the values of p, q, and r by the second, fourth, and sixth power of $\left(1 - \frac{1}{n}\right)$. In order to find the factor $\left(1 - \frac{1}{n}\right)$, we have only to make the calculated values of p and q equal with those found by experiment, page 219; the result is that we may call $\left(1 - \frac{1}{n}\right) = 0{\cdot}74$ nearly. Hence we obtain

$$p = +\ 0{\cdot}073$$
$$q = -\ 0{\cdot}708$$
$$r = -\ 0{\cdot}191.$$

The value of p departs only $\frac{1}{13}$th, and the value of q $\frac{1}{8}$th from the true values, so that we may suppose the value of r also to be incorrect only to a small amount.

If we now compute the corrections for log $2\frac{M}{X}$, we find that 0·00007 would have to be put for the nearest, and 0·00001 for the second distance; for the remaining distances the correction falls beyond the fifth place of decimals. We conclude therefore that in cases which occur in practice the fourth member in the development may be neglected.

* The observations here referred to are contained in a notice printed in the same part of the *Annalen*, and entitled, "On the Determination of the Law according to which Magnetism is distributed in Steel bars."

Article XXVIII.

Observations of the Magnetic Inclination at Göttingen. By Professor C. F. Gauss *.

[From the *Resultate aus den Beobachtungen des Magnetischen Vereins* im Jahre 1841.]

1.

The Inclinatorium with which these observations were obtained was made by Robinson, and is the last instrument of the kind completed by that distinguished artist before his death.

The vertical circle has an interior diameter of 241·169 millimetres (about 9·5 English inches), and is graduated to ten minutes; so that every two divisions are 0·351 millimetre apart at their inner extremities. The divisions when viewed through a microscope, even with a considerable magnifying power, appear very perfect; by repeated measurement I have found their breadth = 0·024 millimetre, so that each covers nearly 41 seconds.

The diameter of the horizontal circle, measured between the points where the extremities of the index meet the graduation, is 148 millimetres. The graduation is to half degrees, and the vernier gives single minutes. There is only one reading.

On the vertical circle the graduation counts upwards and downwards to 90° from the two extremities of a horizontal diameter, an arrangement which may perhaps appear convenient in ordinary cases of observation, but which is liable to cause some confusion, or at least to render the registry more troublesome and less clear, when a loaded needle is employed, or when the plane of observation is varied from that of the magnetic meridian. I therefore prefer either a complete graduation of 360°, or 180° twice repeated; and I have accustomed myself, when observing either in the lower quadrant on the left side, or on the upper quadrant on the right side, to write down, instead of the division on the circle, the complement to 180°, and all the readings in this memoir are to be so understood. On the horizontal circle the numbers run twice in the same sense from 0 to

* Communicated by Lieut-Colonel Sabine.

180°; an uninterrupted reading from 0 to 360° would be more natural and convenient, and the readings are so expressed here. A space of one millimetre on the level corresponds to an inclination of 9 seconds.

2.

The instrument has four needles, which I distinguish as 1, 2, 3, 4, of which 3 and 4 have revolving axles, an arrangement to be spoken of in the sequel. A microscopic examination and measurement showed no sensible difference in the thickness of the eight terminations of the axles, which was found to be = 0·590 millimetre. Needles 1 and 2 weighed each 16·5 grammes, and the other two each 20·5 grammes.

Small differences were found in the lengths of the several needles. They measured respectively—

Needle 1	. . . . . 240·931	millimetres.
2	. . . . . 240·866	...
3	. . . . . 240·938	...
4	. . . . . 240·954	...

The shortest of the needles is therefore only 0·303, and the longest only 0·215 less than the diameter of the circle. This circumstance is favourable to exact reading, but at the same time it renders a very small excentricity capable of disturbing the free movement of the needle, making it difficult to adjust in a perfectly satisfactory manner those parts of the instrument on which the excentricity depends, the adjustment having also to satisfy four other conditions, making in all *six* conditions.

3.

These six conditions are the following:—

The two agate planes, on the upper surfaces of which the cylindrical terminations of the axle rest during observation, are to be adjusted by the supporting screws in such manner that—

1. Their upper surfaces may be in the same plane.

2. That this plane may be perpendicular to the plane of the vertical circle.

3. That it may be below the centre of the vertical circle, a distance equal to half the thickness of the cylindrical terminations of the axle.

4. That the line of intersection of the two planes abovementioned may form a right angle with the vertical axis.

Further, the Y's by means of which the needle is raised off and again lowered upon the agate planes, and which are so fastened to the raising frame as to admit of a small adjustment, must be so regulated, that after raising the needle,

5. Its axis may be normal to the line of intersection (4.), whereby, in combination with (2.), it will also be normal to the plane of the vertical circle, and at the same time

6. Coincide with the vertical diameter of the circle.

Conditions 1, 2 and 4 taken together are equivalent to a single one, viz. that the axis of rotation being accurately vertical, a horizontal plane shall be a tangent to both the agate planes in the direction of their length, it being assumed that the plane of the vertical circle is parallel with the axis of rotation, so that they both become vertical together. This may be regarded as the seventh condition, which is usually tacitly assumed in dependance on the skill of the artist, and for which the instrument as supplied offers no means of trial and eventual correction.

4.

For the observations in this Memoir, in the absence of other means of testing the above-mentioned conditions, I proceeded as follows:—

For the *first* condition I used the plane glass of an artificial horizon, which (after the frame, &c. were taken away) was placed on the agates with the ground surface uppermost. If the condition is not fulfilled, the glass touches only *one* agate throughout its length, and the other only at one end; this will be detected by the eye if the error is not very small; but greater precision and certainty will be obtained by placing on the glass a level, showing whether it has two different planes of contact or only one. It will be easily seen, that by the aid of this level, after the first condition is fulfilled, the *second* and *fourth* may also be tested.

In order to examine the *fifth* condition, the needle must be brought into two different positions of equilibrium, such that the ends of the axle being similarly placed on the supports, (or the same face of the needle being to the front,) the needle, having only a moderate inclination from the horizontal line, may have that end which was to the left in one position, to the right in the other position. These two positions are most conveniently obtained by loading the needle suitably, and the fulfilment or otherwise of the conditions is recognised by seeing whether in the two positions the sharp extremities of the two ends of the needle project an equal quantity in front of the margin of the circle. In countries where the inclination of the magnetic needle is of moderate amount, the two positions may be obtained by a half revolution of the instrument, so that the plane of the circle shall on both occasions be near the magnetic meridian.

For examining the *second* condition a similar method may be employed, only with the difference that it is two opposite nearly *vertical* positions which are required, one of which is given at once by bringing the plane of the circle nearly at right angles to the magnetic meridian, and the other must be obtained either by loading the needle suitably, or by reversing the poles. But it is easy to see that this method is only equivalent to that with the level before described, when the seventh condition is fulfilled; therefore, by combining the two methods a kind of test of the last-named condition is obtained; a very imperfect one it is true, because the equal projection of the points of the needle in front of the limb of the circle can only be judged of by estimation.

These combined positions of the needle serve also for examining the two remaining conditions: the *sixth* condition is fulfilled, if each end of the needle in the first nearly horizontal position is at the same distance from the inner periphery of the circle, as in the second position. The same thing applies to the *third* condition in the two nearly vertical positions. It is evident that it would be sufficient for this examination, to compare with each other the distances of the two ends of the needle from the inner periphery of the circle in one nearly horizontal and one nearly vertical position, if the two halves of the needle were of exactly equal length; but in our instrument, where the intervals are so very small, this is not sufficient; even a very small inequality in the two halves of the needle would be sensible.

5.

The difficulty of satisfying in this manner all the conditions at the same time, is evident from the circumstance that the two screws on which each agate plane rests are only eight millimetres apart, so that, as the width of a turn of the screw amounts to 0·283, a half revolution of a screw turns the agate one degree.

The task is greatly facilitated by an apparatus which I did not have made until afterwards, and which serves to bring the surfaces of the agates into one plane, and to make this plane horizontal: I will not however stop to describe it in this place, as it could not be employed for the present observations, with the exception of those of the 23rd of September. A second contrivance, which also was not completed until after the conclusion of the observations, serves for the accurate determination of the deviation of the principal circle from the vertical position. A deviation of *ten minutes* has thus been found: this could not be removed without altering the putting together of the instrument: its influence on the inclination however cannot amount even to a second.

Further, I must not omit to notice, that small errors in the different adjustments can only produce a nearly imperceptible effect on the determinations of inclination. The influence which some of the errors have on the position of the needle, is, in respect to that position, only a magnitude of the second order; and the effects of the others, *i. e.* of an excentricity, and of an inclination of the tangential plane to the agates in the sense parallel with the plane of the circle (faults against conditions 3, 6 and 4), are perfectly eliminated by the combination of the partial observations. I cannot therefore agree in the opinion expressed by Horner (*Physik. Wörterb.* 5 Band. S. 759), that the removal of this latter error ought to be looked to *before all*; on the contrary, I regard its perfect removal as that which is of the least importance.

6.

The observations of inclination here given were all made on the spot described in vol. v. of the *Resultate*, protected by a shed from the rays of the sun. The instrument was placed on the stone in such manner that the line joining two of the feet should be nearly perpendicular to the magnetic meridian; and for this position the places of the three feet were marked. The exact magnetic orientation of the instrument was obtained by means of an auxiliary needle supplied with it, suspended by a small inverted agate cup which rested on a point; the frame-work carrying the point has two short cylindrical side arms which are laid in the two Y's, so that the point takes of itself a vertical position. I have several times combined with this method the other usual one, of corresponding inclinations in two positions of the vertical circle nearly perpendicular to the magnetic meridian: the differences found were always quite unimportant, showing that the auxiliary needle is sufficiently sensitive, and that it produces no constant deviation. A small deviation of the vertical plane in which the observations are made from the magnetic meridian, which, be it remembered, is not altogether invariable during the observations, has, on the dipping or inclination needle, only an influence of the second order, which may be regarded as insensible.

7.

The coincidence of the centre of gravity of a needle with the axis of rotation can be effected only approximately even by the most skilful artists: there almost always remains a deviation, the influence of which, on the position of the needle, is sought to be determined or eliminated, by combining observations made under circumstances varied in several respects. Among these artificially varied circumstances, a principal one is the reversal of the poles of the needle. Other circumstances being equal, the influence of the above-mentioned deviation is the stronger the weaker the magnetism of the needle; and, as we are not justified in assuming this magnetism to be of equal strength before and after the reversal of the poles, an exact reduction of the observations becomes dependent on our knowledge of the relative amount of force. This is obtained by observing the time of vibration of the needle; but I have, for several reasons, employed, by preference, *horizontal* vibrations, and have used in this observation a particular apparatus made by M. Meyerstein. The needle oscillates in a wooden case with glazed lids, and is supported by a light stirrup suspended to a silk thread 270 millimetres in length, and protected from the air by a glass tube: scales at either extremity of the needle comprise each forty degrees, and are divided to half degrees: five minutes can be estimated with certainty. The time of vibration of the needle is always determined before and after the reversal of the poles by 150 vibrations divided into three sets, which, after a proper reduction to infinitely small arcs, always give extremely accordant results. The commencing arc was usually about thirty-six degrees; and it is deserving of notice, that, contrary to our experience with heavier bars (*Resultate*, vol. ii. p. 70), the rate of decrease of the arc is almost the same on all days and with all needles, so that the time within which the arc was reduced to a fourth part of its amount was, with only slight variations—fourteen minutes. The observations of vibration were always taken in the astronomical observatory upon a stone pedestal, the object being not so much the determination of the absolute time of vibration as of its relative amount, which cannot be sensibly affected by the slight local influences which may possibly exist there.

8.

In the observations of inclination in the summer of 1842, I had in view, besides the establishment of the magnetic inclination corresponding to that epoch, the determination of the degree of precision attainable with the instrument employed. It did not appear to me that the trustworthiness of a final result, which is influenced by so many circumstances, could be sufficiently estimated from the differences in the position of the needle in repeated raisings of the Y's. The mere comparison of results, obtained on different days, is equally unsuitable for this object, as accidental errors are mingled with the actual fluctuations of the magnetic inclination itself. I was further desirous of learning whether my four needles were accordant, or whether, as has happened to some observers, they would give results having decided and important differences*.

These considerations induced me to deviate from the arrangement usually followed, and to adopt another differing from it principally in the following particulars.

It is usual to observe the position of the needle, *i.e.* the readings of the circle opposite the two extremities of the needle in four different combinations of the position of the circle, and of the mode of putting in the needle; the graduated face of the circle and the marked face of the needle being turned either towards the east or towards the west, and either to the same or to opposite quarters. These combinations are repeated after reversing the poles, producing in all sixteen numbers, of which the arithmetical mean is taken as the inclination, unless they show great differences arising from the centre of gravity of the

* The most striking instance of this kind occurs in the Fifth Report of the British Association for the Advancement of Science, p. 142. Eight needles, employed by Captain Ross, in determining the inclination in London, gave differences of forty-one minutes, although the observations with each needle separately were numerous, and agreed well together. The reason of this remarkable fact, fuller details of which are not given, was supposed in England to have been the want of a perfectly cylindrical form in the axles, and moveable or revolving axles were tried on this account.

needle deviating considerably from its axis of rotation. Each of the sixteen numbers may of course be itself the mean of a greater or less number of readings, obtained by raising and lowering the needle repeatedly in each combination.

The method which I followed differed from the above, inasmuch as I observed each day with two needles without inverting the poles as usual during the observations: the inversion of the poles took place between two successive observations, and alternately always with one neeedle only. It is obvious that in this manner the observations of four days comprise all the combinations of the different changes of poles with both needles. Needles 1 and 3 were observed in this way, July 6 to 9, and needles 2 and 4, July 17 to 20. A continuation of similarly combined alternations during eight days of observation, with needles 1 and 2 from May 20 to June 5, and with needle 3 and 4 from June 5 to June 8, gave therefore each combination with the direct and inverted poles twice. The observations on each day were arranged so that the results of the two needles should be as nearly as possible simultaneous. With this view the four combinations above mentioned were first observed throughout with one needle, which was raised four times in each; the second needle was then observed in the same four combinations, raising it eight times in each; and, lastly, the first needle was again observed in the same combinations, but repeated in the reverse order, and the needle raised four times in each.

In this arrangement the observations of a single day taken alone do not give any determination of inclination; but when the observations of the following day are combined with them, it is clear that the needle, which has not had its magnetism interfered with, will show how much the inclination differed on the two days, and the one-sided observations with the other needle can be thus reduced to one epoch, and so be completed. With a view to a more rigorous treatment, comprehending the totality of the observations for all the twenty-four days, it will be necessary first to investigate more closely the mutual relations of the partial results.

9.

This development, which is important in more respects than one, may be most conveniently connected with an example taken from an observation made in the usual manner, of which I have made some on several days.

I select for the purpose the observation with needle 1 on the 23rd of September 1842, from 8½ to 11 A.M. The instrument was placed in the magnetic meridian by the aid of an auxiliary needle in the manner indicated in article 6, and the index of the azimuth circle (the degrees of which are reckoned uninterruptedly from left to right, and by me, as before noticed, from 0° to 360°) pointed to 90° 5′ when the vertical circle was in the meridian, and the graduated face was turned towards the east.

Besides the eight usual combinations in the magnetic meridian, I made on this day an equal number in the vertical plane normal to the meridian; these latter observations are here included, as they give occasion to several investigations. The needle employed (as well as the other three) is marked on one side with the letters A and B at either end, forming a convenient distinction both in reversals of the poles and of the faces of the needles. In each of the sixteen combinations the needle was raised and lowered on the agate planes five times by means of the Y's; the following table gives only the mean numbers in each position.

End B a North Pole.

Azim. circle.	Marked face of Needle.			
	To the front.		To the back.	
	Lower.	Upper.	Lower.	Upper.
° ′	° ′ ″	° ′ ″	° ′ ″	° ′ ″
90 5	67 27 54	67 29 36	67 45 39	67 44 51
180 5	89 52 39	89 52 51	90 12 30	90 10 30
270 5	112 18 39	112 16 45	112 38 51	112 33 54
0 5	89 58 33	89 57 48	90 13 27	90 10 54
End A a North Pole.				
90 5	68 2 51	68 2 33	67 35 15	67 37 0
180 5	90 14 48	90 12 21	89 51 12	89 51 36
270 5	112 27 21	112 22 33	112 7 6	112 5 33
0 5	90 16 15	90 14 0	89 53 54	89 54 18

The duration of one horizontal vibration was found—

before the observations ..... $5^{s}\cdot83555$.

after the observations ...... $5\ \cdot87416$.

10.

I will consider first the differences between the readings of the upper and lower ends, which arise from the axis of rotation not passing either through the centre of the graduation, or through the straight line which unites the two extremities of the needle. If we make $x\ y$ signify the coordinates of the intersection of the axis of rotation with the plane of the circle relatively to the centre of the graduation expressed in parts of arc of the inner periphery of the circle, x being parallel with the diameter, passing through the two zero points, and positive towards the right, y being parallel with the diameter, passing through the two points of 90°, and positive towards the upper part; further, 180° —z being the angle between the two planes passing through the axis of rotation and the points A and B, (it being understood that the needle being supposed horizontal and its marked side being uppermost, the reckoning is from left to right from A to B,) and lastly, the mean between the two readings being designated by l; the difference between them (understanding that the lower reading is subtracted from the upper reading) will be

$$= 2x \sin l + 2y \cos l \pm z,$$

where the upper sign applies when the marked face is to the front and A is at the same time uppermost (or in this part of the world a South Pole), or when the unmarked face is to the front and B is uppermost; in the two other cases the lower sign applies.

The above observations thus give sixteen equations, from which, by the method of least squares,

$$x = -\ 38''\!\cdot\!3$$
$$y = +\ 153\cdot 2$$
$$z = +\ 75\cdot 4.$$

The comparison then gives (the arrangement being according to the amount of l)—

l.	Observation.	Calculation.	Error.
67° 28′ 45″	+ 102″	+ 122″	− 20″
67 36 7	+ 105	+ 121	− 16
67 45 15	− 48	− 30	− 18
68 2 42	− 18	− 32	+ 14
89 51 24	+ 24	0	+ 24
89 52 45	+ 12	− 1	+ 13
89 54 6	+ 24	− 1	+ 25
89 58 10	− 45	− 1	− 44
90 11 30	− 120	− 153	+ 33
90 12 10	− 153	− 153	0
90 13 34	− 147	− 153	+ 6
90 15 7	− 135	− 153	+ 18
112 6 19	− 93	− 111	+ 18
112 17 42	− 114	− 112	− 2
112 24 57	− 288	− 264	− 24
112 36 22	− 297	− 264	− 33

The sum of the squares of the remaining errors is 7924, whence the mean error of the difference between two means from five readings may be assumed

$$= \sqrt{\frac{7924}{13}} = 24''\cdot 7,$$

and the mean error of a simple reading of one end of the needle

$$= \sqrt{\frac{5}{2} \cdot \frac{7924}{13}} = 39''\cdot 0.$$

This is certainly a very satisfactory degree of precision, and is not only confirmed, but sometimes even surpassed by a similar discussion of the observations on other days. It may be noticed, however, that the attainment of such an accord depends materially on the circumstance of the needle never being raised until it is in repose, or very nearly in its position of rest. Without this precaution the needle, the vibration of which consists in a rolling of the axles upon the supporting planes, would come to rest on a different part of the plane from that on which it was let down, and thus the element of excentricity x would be a variable one. In the manner here given, the values of the elements of excentricity x and y are obtained, it is true, with much precision; but these values cannot serve alone to instruct us whether and how much the supports and the Y's must be moved, in order to satisfy conditions 3 and 6 in Art. 3, inasmuch as the latter refer to the centre of the inner circle, and the former to the centre of the graduation, which may not be, and in fact are not, the same in the instrument under consideration. The proper corrections of the adjustment were made with all possible care previous to the observations here given.

The value of z is evidently constant for each needle, and accordingly a similar treatment of the observations on other days has given nearly the same value. For the three other needles I found—

		′	″
for needle 2	....	+ 3	18
... 3	....	− 1	4
... 4	....	+ 1	2

Although the knowledge of these values has no particular practical interest, yet their small amount is an honourable testimony to the care which the distinguished artist bestowed on the execution of the needles*.

* [The dipping needles which have been made by Mr. Robinson's successor, Mr. Barrow, have proved in no respect inferior to those made by the excellent artist from whom M. Gauss's instrument was procured.—E. S.]

11.

The mean of the readings of the two ends of the needle gives us the angle between the straight line joining those points, or a line parallel thereto, and the zero diameter of the vertical circle. I subjoin these 16 means in pairs:—

End B a North Pole.

Azimuth circle.		Marked face of needle to the front.			Azimuth circle.		Marked face of needle to the back.		
°	′	°	′	″	°	′	°	′	″
90	5	67	28	45	270	5	112	36	23
180	5	89	52	45	0	5	90	12	11
270	5	112	17	43	90	5	67	45	15
0	5	89	58	10	180	5	90	11	30
End A a North Pole.									
90	5	68	2	42	270	5	112	6	20
180	5	90	13	34	0	5	89	54	6
270	5	112	24	57	90	5	67	36	7
0	5	90	15	8	180	5	89	51	24

Those positions are here placed next to each other, in which the vertical circle faced in opposite directions, while the position of the axle of the needle relatively to the different quarters of the heavens was the same. The relation of two such numbers l and l' is a very simple one, when the supports are so adjusted that a plane normal to the vertical axis of rotation is tangential to them. Assuming this, the axle of the needle is in both positions in a horizontal plane, and the position of rest of the needle is evidently the same; that is to say, if we understand by L the angle formed by the straight line drawn from the upper to the lower point of the needle with that horizontal radius of the circle which is each time on the right side of the marked face of the needle, then L will have equal values in both positions. But this angle in the first position will be

$$= l - \alpha,$$

and in the second

$$= 180° - (l' - \alpha);$$

α denoting the error of the zero point (i. e. the reading at that radius which is at right angles to the vertical axis). Thus on the above-mentioned supposition we have

$$\alpha = \tfrac{1}{2}(l + l') - 90°$$
$$L = \tfrac{1}{2}(l + 180° - l').$$

From the observations of the 23rd September, when this adjustment had been rectified with the greatest care by the aid of the apparatus mentioned in Art. 5, we obtain eight different determinations of α, namely,

$$\begin{array}{rr} +\,2' & 34'' \\ 2 & 28 \\ 1 & 29 \\ 4 & 50 \\ 4 & 31 \\ 3 & 50 \\ 0 & 32 \\ 3 & 16 \end{array}$$

The sum of the squares of the differences from the mean value of $2'\,56''$ expressed in seconds, is $=57214$; if then we regard these differences as wholly accidental, we obtain the mean error of the result of a pair of coordinate positions $=\sqrt{\frac{57214}{7}}=90''\cdot4$. We see that with this instrument the anomalies of position are much more considerable than the mere errors of reading.

12.

It is however otherwise when the supposed exact adjustment of the supports has not taken place. If we assume the surfaces of the agates to be indeed in one plane, but this plane not to be normal to the vertical axis, then it will be inclined to the horizon in opposite ways in the two positions of the instrument: it is however only the inclination of the planes in the direction of their surfaces, or parallel with the plane of the circle, which comes into the consideration, for a small inclination in the cross direction, or in that of the axle of the needle, has no sensible influence on the position of repose of the needle. Now let L denote that inclination of the needle (understood as above) which would take place with a perfectly horizontal support; let δ be the corresponding directive force, *i. e.* the coefficient by which the sine of a deflection from the position of rest must be multiplied, to express the rotative moment of the force tending to bring the needle back to this position; lastly, let $L+\beta$ be the actual inclination in the first position with the inclined plane. It may then be easily shown that

$$\delta \sin\beta = p\,\rho \sin\gamma,$$

where p denotes the weight of the needle, ρ the semi-diameter of the axles, and γ the inclination of the planes to the horizontal line; the latter quantity being positive when the agate support on the right side of the marked face of the needle is the lowest. But it is now evident that in the second position $-\gamma$ must be put instead of γ, whereby β passes into $-\beta$; hence in this second position the inclination of the needle will be $L-\beta$. We have thus

$$l-\alpha = L+\beta$$
$$180^\circ-(l'-\alpha) = L-\beta;$$

and consequently, as in the preceding article,

$$\tfrac{1}{2}(l+180^\circ-l') = L;$$

on the other hand, in lieu of the other equation in that article, we have now

$$\tfrac{1}{2}(l+l')-90^\circ = \alpha+\beta.$$

But if the surfaces of the agates are not in one plane, these two formulæ will still be sufficiently exact, if we only take for γ the mean of the inclinations of the two sides, assuming the centre of gravity of the needle to be nearly equidistant from the two points of the axles which rest upon the agates. Strictly speaking, there is still a small modification, arising from the circumstance, that in the case supposed, the straight line joining the two points of contact of the axles and the supporting planes has not quite equal azimuths in the two positions; but the influence of this circumstance on the direction of the needle may be regarded as quite insensible even where it is greatest, namely, in observations made in the plane perpendicular to the magnetic meridian.

13.

It may not be uninteresting to show, when the adjustment of the planes is not perfect, how far their inclination affects the position of the needle; I therefore subjoin what is still wanting for this purpose for the needle employed on the 23rd of September. With a view to determine the moment of inertia of the needle, I had previously observed its horizontal vibrations, both with and without the super-imposition of a ring, the moment of inertia of which could be calculated with sufficient exactness from its weight and dimensions. On the 21st of September the time of vibration was

	$''$
Without the ring. . .	5·88431
With the ring . . .	7·32835
Weight of the ring . .	19·2385 grammes.

Inner diameter of ditto. 75·525 millimetres.
Outer diameter . . . 79·767 ...

Hence, taking grammes and millimetres as units

The moment of inertia of the ring . 29019
... ... of the needle* 52662

From these numbers, combined with the times of vibration of the 23rd of September, given in Art. 9, and the length of the simple pendulum vibrating seconds in Göttingen, taken at 994·126 millimetres, we obtain by following the known method—

Horizontal magnetic directive force,
before the reversal 1·5556
after the reversal 1·5352

Strictly speaking, these numbers apply directly only to the place where the vibrations were observed, and include therefore whatever local influences may have existed there; for the present object this influence, which in any case can only be very small, does not come into consideration.

With an inclination of 67° 40′ 54″, it further follows that the whole magnetic directive force
before the reversal of the poles is 4·0965
after ... ... 4·0429

The vertical magnetic directive force
before the reversal of the poles is 3·7897
after ... ... 3·7401

Neglecting the small modification which the directive magnetic force of the needle suffers by reason of the excentricity of the centre of gravity, these four numbers may be regarded as the values of δ, according as the observations are made in the magnetic meridian, or in the plane perpendicular to it. As β and γ are always so small that we may take these quantities themselves instead of their sines,

$$\beta = \frac{p\rho}{\delta}.\gamma,$$

and we obtain, according to whether we take for our base the force of magnetism in the needle as it was before, or as it was after, the change of poles:

for observations in the magnetic meridian —

$$\beta = 1{\cdot}1882\,\gamma, \text{ or } \beta = 1{\cdot}2039\,\gamma:$$

* Strictly speaking, it is the sum of the moments of inertia of the needle and of the stirrup; it would be both impracticable and superfluous to attempt to separate the two, as none but horizontal vibrations are taken with this stirrup.

for observations in the plane normal to the magnetic direction,—

$$\beta = 1{\cdot}2844\,\gamma,\ \text{or}\ \beta = 1{\cdot}3014\,\gamma.$$

The relations between the separate parts of the observation which have been under consideration hitherto, are not, it is true, very material, in as far as concerns only the deduction of the magnetic inclination from the whole; but they are not unimportant for the purpose of proving and confirming the result, inasmuch as a just confidence in the entire observation requires first a clear view of the satisfactory accordance of the several parts.

14.

The result of the observations is now brought back to the eight values of L, which may be stated as the inclinations of the straight line joining the north and south poles of the needle, in reference to the horizontal radius which is on the right side of the marked face of the needle in the position of equilibrium, the points of the axles being assumed to rest on a horizontal surface; or, what is obviously the same in statical respects, the needle being assumed to be capable of revolving only around the axis corresponding to the axle points. In other words, the values of L are the values given in Article 11, under the heading "*Marked face of Needle,*" corrected, *i. e.* freed from the influence of the error of the zero point, and of the non-horizontality of the supporting planes.

Values of L.

Azimuth circle.	B. North pole.	A. North pole.
90° 5′	67° 26′ 11″	67° 58′ 11″
180 5	89 50 17	90 9 44
270 5	112 16 14	112 24 25
0 5	89 53 20	90 11 52

For the purpose of expressing the connection of the values of L with the elements on which it depends in an equation, I employ the following designations:—

V signifies the position of the azimuthal circle for the observation.

V^0 the position of the azimuthal circle, in which the vertical circle is in the magnetic meridian and the graduated side is turned towards the east.

i the magnetic inclination.

m the product of the magnetic moment of the needle into the total intensity of the magnetic force of the earth, gravity being taken as the unit of the accelerating forces.

q the weight of the needle multiplied into the distance of the centre of gravity from the axis of rotation.

c the small angle between the straight line joining the extremities of the needle and its magnetic axis, being positive when the latter is to the right, the needle being imagined to be lying flat with its marked face uppermost.

Q the angle between the line joining the extremities of the needle, and a line from the axis of rotation to the centre of gravity, commencing from the first-named line, and counted from left to right in the same position of the needle as in c.

δ the directive force.

If we resolve the magnetic force of the earth into a vertical and a horizontal portion, we obtain from the vertical portion,

$$m \sin i \cos (\mathrm{L} + c)$$

as the moment of rotation, taken as positive in the sense of L increasing; and from the horizontal portion,

$$- m \cos i \, (\mathrm{V} - \mathrm{V}^0) \sin (\mathrm{L} + c).$$

The effect of gravity is the moment

$$q \cos (\mathrm{L} + \mathrm{Q}).$$

As L expresses the position of equilibrium, the sum of these three moments $= 0$, whence we obtain the leading equation,

$$- \sin i \cos (\mathrm{L} + c) + \cos i \cos (\mathrm{V} - \mathrm{V}^0) \sin (\mathrm{L} + c)$$
$$= \frac{q}{m} \cdot (\mathrm{L} + \mathrm{Q}).$$

If we write in the sum of the three moments $\mathrm{L} + z$ instead of L, we obtain the moment of rotation which exists with a deflection z from the position of equilibrium; if we develope this expression into two parts with the factors $\cos z$ and $\sin z$, the first disappears by virtue of the leading equation, and the second becomes $= - \delta \sin z$. Thus we have for δ the general formula

$$\delta = m \sin i \sin (\mathrm{L} + c) + m \cos i \cos (\mathrm{V} - \mathrm{V}^0) (\cos (\mathrm{L} + \mathrm{C})$$
$$+ q \sin (\mathrm{L} + \mathrm{Q}).$$

We find from hence, for the three chief cases,—

I. For $\mathrm{V} = \mathrm{V}^0$,

$$\sin (\mathrm{L} + c - i) = \frac{q}{m} \cos (\mathrm{L} + \mathrm{Q})$$

$$\delta = m \cos (L + c - i) + q \sin (L - Q)$$
$$= \frac{m \cos (Q + i - c)}{\cos (L + Q)}$$
$$= \frac{q \cos (Q + i - c)}{\sin (L + c - i)}.$$

II. For $V = V^0 + 180^\circ$,

$$\sin (L + c + i) = -\frac{q}{m} \cos (L + Q)$$
$$\delta = -m \cos (L + c + i) + q \sin (L + Q)$$
$$= -\frac{m \cos (Q - c - i)}{\cos (L + Q)}$$
$$= \frac{q \cos (Q - c - i)}{\sin (L + c + i)}.$$

III. In accordance therewith, for $V = V^0 + 90^\circ$, and $V = V^0 + 270^\circ$,

$$\sin i \cos (L + c) = -\frac{q}{m} \cos (L + Q)$$
$$\delta = m \sin i \sin (L + c) + q \sin (L + Q)$$
$$= -\frac{m \sin i \sin (Q - c)}{\cos (L + Q)}$$
$$= \frac{q \sin (Q - c)}{\cos (L + c)}.$$

Our example gives for the two latter cases, instead of equal values of L, inequalities of 3′ 3″ and 2′ 8″ respectively, attributable partly to accidental errors of observation, and partly to the concurrence of several circumstances; namely, a small uncertainty as to the magnetic meridian at the commencement, and the possible change of the magnetic declination, and therefore of the value of V^0, in the course of the observations,—in a small excentricity of the horizontal circle, for which, in the absence of two readings, there is no check; and finally, because the axle can only be made imperfectly rectangular to the plane of the circle in raising it and lowering it by means of the Y's. All these circumstances are rendered as harmless as possible by taking the means of the two positions thus:—

For B a north pole . . $L = 89^\circ\ 51'\ 49''$.
For A a north pole . . $L = 90\ \ 10\ \ 48$.

However, by reason of these circumstances the results, in a position normal to the magnetic meridian, will always deserve rather less confidence than those in the meridian itself, in which the influence of the above-named causes may be regarded as insensible.

15.

The remaining six of the thirty-two original numbers may henceforth be designated as follows :—

Values of L.	For $V - V^0 =$
f, f	0
$180° - g, 180° - g'$	180°
h, h'.	90° and 270°.

Where the non-accented signs apply to B a north pole, and the accented to A a north pole, f, f', g, g' are the dips of the straight line joining the two extremities of the needle below the north horizontal line, for the positions in the magnetic meridian, f and f' being for the position in which the marked face of the needle is turned towards the east, and g and g' for the contrary position; h, h' are the dips of the same straight line in reference to the eastern or the western horizontal line, according as the marked face of the needle is turned towards the south or towards the north, the instrument being in the plane normal to the magnetic meridian.

In regard to the elements on which these six quantities depend, q is quite constant, and i must be assumed equal for all, as we cannot take into account the small fluctuations which may take place during the course of the observations; but Q, m, c alter their values after the poles have been changed; Q exactly 180°; m and c, so that they have no further definite relation to their previous state save that, if in changing the poles the manipulation has been the same, and powerful magnets have been employed, we may be sure that the differences cannot be great, nor in the case of c can the absolute values be very considerable. As henceforth I let the non-accented signs Q, m, c signify the definite values applicable to the observations with B a north pole, and replace them for A a north pole by Q + 180°, m' and c', the general equations of the last article transform themselves into the six following :—

$$\sin(f + c - i) = \frac{q}{m}\cos(f + Q) \quad \ldots\ldots\ldots \quad (1.)$$

$$\sin(g - c - i) = \frac{q}{m}\cos(g - Q) \quad \ldots\ldots\ldots \quad (2.)$$

$$\sin i \cos (h + c) = -\frac{q}{m} \cos (h + Q) \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

$$\sin (f' + c' - i) = -\frac{q}{m'} \cos (f' + Q) \quad . \quad . \quad . \quad . \quad (4.)$$

$$\sin (g' - c' - i) = -\frac{q}{m'} \cos (g' + Q) \quad . \quad . \quad . \quad . \quad (5.)$$

$$\sin i \cos (h' + c') = \frac{q}{m'} \cos (h' + Q) \quad . \quad . \quad . \quad . \quad . \quad (6.)$$

16.

Theoretically considered these six equations are sufficient for determining the six unknown quantities c, c', $\frac{q}{m}$, $\frac{q}{m'}$, Q, i; and the solution of this problem may be allowed a place here, although it has no practical value, because the enormous influence of the unavoidable errors of observation on the final result renders this proceeding quite unsuitable.

By multiplying the equations 1, 2, 3 respectively by $\sin (g + h)$, $\sin (f - h)$, $\sin (f + g)$, and adding, we obtain, after some easy reductions,

$$\sin (f + c) . \sin (g + h) = \sin (g - c) . \sin (h - f),$$

whence c can be easily determined, and most conveniently, by means of the formula,

$$\tan (c + \tfrac{1}{2} f - \tfrac{1}{2} g) = - \tan \tfrac{1}{2} (f + g)^2 . \cot (h - \tfrac{1}{2} f + \tfrac{1}{2} g).$$

In a similar manner we obtain from the equations (4.), (5.), (6.),

$$\tan (c' + \tfrac{1}{2} f' - \tfrac{1}{2} g') = - \tan \tfrac{1}{2} (f' + g')^2 . \cot (h' - \tfrac{1}{2} f' + \tfrac{1}{2} g').$$

The numbers in our example are—

$$\begin{array}{ll} f = 67^\circ\ 26'\ 11'' & f' = 67^\circ\ 58'\ 11'' \\ g = 67\ 43\ 46 & g' = 67\ 35\ 35 \\ h = 89\ 51\ 49 & h' = 90\ 10\ 48 \end{array}$$

whence, according to the above formula,

$$c = +\ 12'\ 21'' \qquad c' = -\ 14'\ 18''.$$

The magnitude of these values appears already almost beyond probability, and the little confidence due to them becomes visible on developing the influence upon them of small errors in the fundamental numbers. The differential formula serving thereto may be given in several forms, of which the following is one:—

$$d\,c = -\frac{\sin (g - c) . \sin (h + c)}{\sin (h - f) . \sin (f + g)} d f +$$

$$\frac{\sin(f+c) \cdot \sin(h+c)}{\sin(g+h) \cdot \sin(f+g)} \cdot dg + \frac{\sin(f+c) \cdot \sin(g-c)}{\sin(h-f) \cdot \sin(h+g)} \cdot dh.$$

The same formula applies for dc' if we only substitute f, g, h, for f', g', h'. Applied to our calculation they give

$$dc = -3{\cdot}435\, df + 3{\cdot}441\, dg + 5{\cdot}876\, dh$$

$$dc' = -3{\cdot}499\, df' + 3{\cdot}494\, dg' + 5{\cdot}993\, dh'.$$

Remembering that the values of h and h' themselves are still less trustworthy, and may therefore contain errors of one or two minutes, it is evident that the values found for c and c' merit no confidence.

For the sake of completeness, I subjoin the mode of finding the remaining unknown quantities.

From the combination of equations (1.) and (2.), it follows that

$$\cos i = -\frac{q}{m} \cdot \frac{\sin(f+g) \sin(Q-c)}{\sin(2c+f-g)}; \ldots . \quad (7.)$$

and thus, under the application of equation (3.),

$$\tan i = \frac{\sin(2c+f-g)}{\sin(f+g) \cdot \cos(h+c)} \cdot \frac{\cos(Q+h)}{\sin(Q-c)}.$$

In an entirely similar manner equations (4—6.) give

$$\tan i = \frac{\sin(2c'+f'-g')}{\sin(f'+g') \cdot \cos(h'+c')} \cdot \frac{\cos(Q+h')}{\sin(Q-c')}.$$

Consequently if, for the sake of brevity, we write

$$\frac{\sin(2c'+f'-g') \cdot \sin(f+g) \cdot \cos(h+c)}{\sin(2c+f-g) \cdot \sin(f'+g') \cdot \cos(h'+c')} = k,$$

then

$$\cos(Q+h) \cdot \sin(Q-c') = k \cos(Q+h') \cdot \sin(Q-c).$$

If we make

$$\cos(h-c') - k\cos(h'-c) = A \sin B$$

$$\sin(h-c') - k\sin(h'-c) = A \cos B$$

$$\frac{\sin(h+c') - k\sin(h'+c)}{A} = C,$$

this equation takes the simple form

$$\cos(2Q - B) = C,$$

whereby Q is determined; i is then found from one of the two equations for $\tan i$; and lastly, $\frac{q}{m}$, $\frac{q}{m'}$ are found from (1,) or (2.), and from (4.) or (5.). Respecting these calculations we may conclude with the following remarks:—

I. In order to be able to conduct with precision the numerical calculation according to the above formulæ, c and c' must be computed with much more exactness than the absolute confidence due to them would in itself merit, otherwise the double determination for i, $\frac{q}{m}$, $\frac{q}{m'}$ would give but little accord*. However other formulæ not subject to this inconvenience but somewhat less simple may be substituted; these are subjoined, omitting the deduction, which is not a difficult one:—

$$\tan i = -\frac{2\sin(f+c)\,.\,\sin(g-c)}{\sin(f+g)\,.\,\sin(h+c)}\cdot\frac{\cos(Q+h)}{\sin(Q-c)}$$
$$= -\frac{2\sin(f'+c')\,.\,\sin(g'-c')}{\sin(f'+g')\,.\,\sin(h'-c')}\cdot\frac{\cos(Q+h')}{\sin(Q-c')}$$
$$k = \frac{\sin(f+g)\,.\,\sin(f'+c')\,.\,\sin(g'-c')\,.\,\sin(h+c)}{\sin(f'+g')\,.\,\sin(f+c)\,.\,\sin(g-c)\,.\,\sin(h'+c')}.$$

II. The equation $\cos(2Q - B) = C$ (with the exception of the special case in which $C = \pm 1$), has always four different solutions or values of Q between 0° and 360°, in pairs differing 180°. Such a pair of values of Q belong to one and the same value of i, but to opposite though equal values of $\frac{q}{m}$, $\frac{q}{m'}$: now, as these latter quantities must from their nature be positive, one value of Q in each pair drops out of itself. But if one value of Q gives the signs of $\frac{q}{m}$, $\frac{q}{m'}$ opposite to each other, it is clear that the whole pair is to be rejected; and if the same thing should take place with both pairs, it can only be concluded that the errors of observation render the combination of equations (1—6.) quite unfit for determining the unknown quantities. In our example the calculation gives the two following systems of values:—

	First System.	Second System.
	° ′ ″	° ′ ″
$Q =$	12 44 41 / 192 44 41	179 57 42 / 359 57 42
$i =$	67 41 33	60 2 11
$\frac{q}{m} =$	$\mp$ 0·0051395	$\mp$ 0·3443905
$\frac{q}{m'} =$	$\mp$ 0·0042073	$\pm$ 0·3563855

* All the calculations in this memoir have been made with the greatest precision; but fractions of seconds have been omitted in printing. If, therefore, any person should pursue the calculation with the intermediate values thus curtailed, he may sometimes find results which deviate a little.

It is here evident that we must reject the second system entirely, and the upper value of Q in the first system, leaving only as admissible the value of $Q = 192° 44' 41''$. It can only be ascribed to an accidental mutual compensation of the errors of observation, that a really good value of i is combined therewith, and that the already considerable deviation of the ratio of the values of $\frac{q}{m}$ and $\frac{q}{m'}$ from that of the squares of the times of vibration (Art. 9.) to which they ought to be proportional, is not much greater still. In fact the mere increase of one minute in the value of h' (the values of the five other quantities, f, g, h, f', g', remaining the same) produces worthless results, inasmuch as calculation conducted according to the above method gives two systems of solutions, in which the inclination is respectively $68° 17' 40''$ and $66° 23' 12''$, while in both systems the values of $\frac{q}{m}$, $\frac{q}{m'}$ receive opposite signs; a striking proof that calculation cannot be founded on such combinations.

17.

If we now give up the observations in the plane perpendicular to the magnetic meridian, we must either supply their place by other data, or we must lay down certain arbitrary suppositions which are not rigorously correct, and must be satisfied with the degree of exactness in the results which is attainable in this manner. Throughout my observations a new datum can be derived from the times of vibration observed before and after the reversal of the poles, the squares of which times may be regarded as proportional to the quantities $\frac{q}{m}$, $\frac{q}{m'}$. It is true that the same apparatus with which the times of vibration are observed may also be employed for an immediate determination of the quantities c and c', by placing the needle in the stirrup in two ways, one with the marked face uppermost, and one with the same face beneath, and observing the place of the ends of the needle relatively to the graduated arc, and taking account of any changes of declination by means of simultaneous observations with the unifilar magnetometer. But the apparatus in question is not susceptible of such accurate readings as would be requisite for this application, for which it was not designed. If an apparatus of the kind were divided with much greater exactness, and carefully and immoveably fixed, and if the readings were taken with microscopes, it would no doubt be possible to determine c and c' directly, with all the accuracy that could be desired; we should then have even a datum more than necessary, so that by suitable arrangements the accuracy of the results could be further increased.

At present I supply the place of the datum which is wanting by supposing the magnetic axis of the needle not to be changed by the reversal of the poles, or that $c' = c$. This assumption has been made by all observers who have attempted to determine the inclination by a more rigorous calculation than by the formula otherwise in general employment, $i = \frac{1}{4}(f + g + f' + g')$; and we certainly have reason to expect that the assumption will not be likely to be *much* in error if the change of poles is always performed with great care, with the same bar-magnets, and with the needle in the same position in a suitably constructed groove. However, my own experience shows that, in spite of these precautions, differences which are not inconsiderable may arise in the position of the magnetic axis, and clear indications of the same may often be traced in the figures of other observers. (For example, Erman's observations of the 13th of October 1829, treated according to his own propositions, give the deviation of the magnetic axis of one needle 36′ 24″, whilst at other times it appears to have been very small.) Fortunately, under the circumstances which take place here, even a considerable degree of incorrectness in the assumption in question can have only a very small influence on the result.

18.

According to these positions the solution of the problem is given in the following manner. I combine with the equation (7.) already employed, viz.—

$$\frac{\cos i \, . \sin (2\,c + f - g)}{\sin (f + g)} = -\frac{q}{m} \, . \sin (\mathrm{Q} - c);$$

those which follow in a similar manner from (4.) and (5.), putting therein c in lieu of c', and $\frac{\lambda q}{m}$ instead of $\frac{q}{m'}$,

$$\frac{\cos i \, . \sin (2\,c + f' - g')}{\sin (f' + g')} = -\frac{\lambda' q}{m} \, . \sin (\mathrm{Q} - c); \quad . \quad (8.)$$

thus

$$\lambda \sin(f' + g') \sin(2c + f - g) = \sin(f + g) \, . \sin(2c + f' - g'),$$

whereby c is best determined by means of formula (9.),

$$\tan\left(2c - \tfrac{1}{2}(g + g' - f - f')\right)$$
$$= \frac{\lambda \sin(f + g) - \sin(f' + g')}{\lambda \sin(f + g) + \sin(f' + g')} \, . \tan(f - g - f' + g').$$

It further follows from (1.) and (2.), that

$$2 \cos i \, . \sin(f + c) \sin(g - c) - \sin i \, . \sin(f + g)$$
$$= \frac{q}{m} \, . \cos(Q - c);$$

thus by combination with (7.),

$$\operatorname{cotan}(Q - c) = \frac{\sin(f + g)}{\sin(2c + f - g)} \, . \tan i$$
$$- \frac{2 \sin(f + c) \, . \sin(g - c)}{\sin(2c + f - g)}.$$

We deduce in a similar manner from (4.), (5.) and (8.),

$$\operatorname{cotan}(Q - c) = \frac{\sin(f' + g')}{\sin(2c + f' - g')} \, . \tan i$$
$$- \frac{2 \sin(f' + c) \, . \sin(g' - c)}{\sin(2c + f' - g')}.$$

If we write for the sake of brevity,

$$\operatorname{cotan}(f + c) = F \qquad \operatorname{cotan}(f' + c) = F'$$
$$\operatorname{cotan}(g - c) = G \qquad \operatorname{cotan}(g' - c) = G',$$

these two equations receive the form

$$\operatorname{cotan}(Q - c) = \frac{G + F}{G - F} \, . \tan i - \frac{2}{G - F}$$

$$\operatorname{cotan}(Q - c) = \frac{G' + F'}{G' - F'} \, . \tan i - \frac{2}{G' - F'};$$

whence, lastly, we obtain

$$\tan i = \frac{G' - F' - G + F}{G' F - G F'}$$

$$\operatorname{cotan}(Q - c) = \frac{G' + F' - G - F}{G' F - G F'}.$$

After i and Q have been found, $\frac{m}{q}$ may be determined from any one of the equations (1.), (2.), (4.), (5.), (7.), (8.).

In our example we have

$$\lambda = \left(\frac{5{\cdot}87416}{5{\cdot}83555}\right)^2;$$

and further calculation gives

$$\begin{array}{lrrr} c = - & 0^\circ & 1' & 13'' \\ i = & 67 & 40 & 54 \\ Q - c = & 145 & 17 & 10 \\ Q \quad = & 145 & 15 & 57 \end{array}$$

$$\frac{q}{m} = 0{\cdot}0055111$$

$$\frac{q}{m'} = 0{\cdot}0055843.$$

The *calculated* values of h, h', computed according to these elements, are

$$\begin{aligned} h &= 89^\circ\ 49'\ 30'' \\ h' &= 90\ \ 12\ \ 59, \end{aligned}$$

from which the observed values differ $+\ 2'\ 19''$ and $-\ 2'\ 11''$.

19.

In the absence of a direct determination of the relation of $\frac{q}{m}$, $\frac{q}{m'}$, we are constrained to make two arbitrary suppositions instead of one. The two following modes have come into use by observers:—

I. It is assumed that $c = 0$ and $c' = 0$, whence we have for i the formula

$$\tan i = \frac{\cot g' - \cot f' - \cot g + \cot f}{\cot g' \,.\, \cot f - \cot f' \,.\, \cot g}.$$

This is the usual mode of proceeding when the needle is purposely loaded with a small weight, according to Mayer's method. As in this mode readings are obtained at parts of the limb quite different from the place of the needle without weights, if the results do not show any considerable discordance, some satisfactory indication is afforded that the limb does not contain any magnetic particles. It is advisable to keep the loading within moderate limits, as otherwise the influence of errors of observation on the result would be unduly increased, and even the neglected c, c', would have a sensibly prejudicial effect.

II. It is assumed that $m = m'$ and $c = c'$. This it will be seen is only a special case of the one treated of in the last article, and therefore the formula there given applies, λ being made $= 1$. The formula (9.) for c then assumes a form even somewhat more simple, namely,—

$$\tan\left(2c - \tfrac{1}{2}(g + g' - f - f')\right) =$$
$$\tfrac{1}{2}\,\frac{\tan\frac{1}{2}(f + g - f' - g')\,.\,\tan\frac{1}{2}(f - g - f' + g')}{\tan\frac{1}{2}(f + g + f' + g')}.$$

For the case when c is not required, but only i is to be determined, an elegant rule for the calculation is given in Erman's *Reise*, part 2. vol. ii. p. 22.

20.

The relations of the observations to the inclination and to the other elements which have been hitherto developed apply generally, whether the deviation of the centre of gravity from the axis of rotation be great or small. When the latter is the case, which it always will be in needles executed by a really good artist, as long as they remain unaltered by any external causes (for example, by rust, wear, taking out the axles, or by weights intentionally added) the formulæ admits of a very material simplification. As long as $\frac{q}{m}$ or $\frac{q}{m'}$ do not exceed the value 0·03, the difference between the sines of $f + c - i$, $g - c - i$, $f' + c' - i$, $g' - c' - i$, and the arcs themselves, cannot amount to a second; and considering the moderate degree of exactness of which observations with an inclinatorium are susceptible, the arc and the sine may be interchanged without scruple, even with considerably higher values of $\frac{q}{m}$, $\frac{q}{m'}$. With the four needles of Robinson's Inclinatorium the values are within even much smaller limits, and I shall therefore treat the observations to be here given according to such an abbreviated method, first subjecting to it the example which has been hitherto under our consideration.

21.

If, for the sake of brevity, we write

$$\frac{206265''\,q\cos Q}{m} = t$$

$$\frac{206265''\,q\sin Q}{m} = u,$$

then under the supposition that $f + c - i$, $g - c - i$, $f' + c' - i$, $g' - c' - i$ are so small that they may be used for their sines, the equations (1.), (2.), (4.), (5.) of Article 15 assume the following form:—

$$\begin{aligned}
i &= f + c - t\cos f + u\sin f \\
i &= g - c - t\cos g - u\sin g \\
i &= f' + c' + \lambda t\cos f' - \lambda u\sin f' \\
i &= g' - c' + \lambda t\cos g' + \lambda u\sin g'.
\end{aligned}$$

The five unknown quantities i, c, c', t, u cannot, it is true, be determined by four equations, but they may be expressed by one quantity which remains indeterminate; and if $c' - c$ be selected for the purpose, we learn, in the clearest manner, in what degree we are justified in neglecting the quantity so represented. The elimination itself is in each separate case most conveniently conducted, after substituting the numerical values in the observation data.

In our example the four equations become

$$\begin{aligned}
i &= 67^\circ\,26'\,11'' + c - 0{\cdot}3837\,t + 0{\cdot}9234\,u \\
i &= 67\;\;43\;\;46 - c - 0{\cdot}3790\,t - 0{\cdot}9254\,u \\
i &= 67\;\;58\;\;11 + c' + 0{\cdot}3801\,t - 0{\cdot}9393\,u \\
i &= 67\;\;35\;\;35 - c' + 0{\cdot}3862\,t - 0{\cdot}9368\,u,
\end{aligned}$$

whence we find by elimination,

$$\begin{aligned}
i &= 67^\circ\,41'\,54'' - 0{\cdot}0006\,(c' - c) \\
t &= -\,934 + 0{\cdot}0002\,(c' - c) \\
u &= +\,648 + 0{\cdot}5369\,(c' - c) \\
\tfrac{1}{2}(c' + c) &= -\,73 + 0{\cdot}0037\,(c' - c).
\end{aligned}$$

Hence we see that the arbitrary supposition of the equality of c and c' does indeed make a secure determination of u impracticable, but that it has no sensible influence on the values of i and t, and only a small influence even on the determination of the mean value of c and c'.

The mean of these four equations is

$$i = 67^\circ\,40'\,56'' + 0{\cdot}0009\,t - 0{\cdot}0011\,u,$$

where the absolute part is the simple mean of f, g, f', g', and might fairly, without anything further, have been taken as the inclination. This is in fact the ordinary mode of proceeding, and may be adopted without scruple where the values of f, g, f', g' do not present any great differences.

22.

Before leaving the example which has been under treatment hitherto, I will remark further, that the equations (3.) and (6.) are susceptible of an abbreviation quite similar to that of the others. We may put

$$c = 90^\circ - h + \frac{\cos h}{\sin i} \cdot t - \frac{\sin h}{\sin i} \cdot u$$

$$c' = 90^\circ - h' - \frac{\lambda \cos h'}{\sin i} \cdot t + \frac{\lambda \sin h'}{\sin i} \cdot u.$$

In the numerical calculation we may safely substitute for i the value $\frac{1}{4}(f + g + f' + g')$, whereby in our example the equations will arrange themselves thus :—

$$c = + 491'' + 0{\cdot}0026\, t - 1{\cdot}0810\, u$$
$$c' = - 648 \;\; + 0{\cdot}0034\, t + 1{\cdot}0953\, u.$$

As the values of h and h' rest on twice as many positions as the values of f, g, f', g', we should have, if we consider only the number of positions, to append to each of these equations the weight $2 \sin i^2$, the weight of each of the four equations in the preceding article being made $= 1$: but from the reasons previously given (Art. 14) the determinations of h and h' possess considerably less trustworthiness, and the weight of all six equations may therefore be taken as equal, by which the calculation will be rendered more simple. If we deduce from them in this manner the five unknown quantities, calculating according to the method of least squares, we find

$$\begin{aligned} i &= 67^\circ\, 40'\;\; 55'' \\ t &= \;\; - \;\; 934 \\ u &= \;\; - \;\; 211 \\ c &= \;\; + \;\; 719 \\ c' &= \;\; - \;\; 880 \end{aligned}$$

by which values all the equations are satisfied to one or two seconds, a degree of agreement which must indeed be looked upon as merely accidental, as the data include a much greater amount of uncertainty. The values of u, c, c' also deserve no confidence; with the high dips of these regions the data are not suitable for a separation of those quantities with any degree of certainty.

23.

After this review of the different methods of calculation, I pass to the principal object; and shall first subjoin in a tabular form the observations made in the manner described in Art. 8. I place here only the quantities expressed by f, g, f', g', leaving out the partial results from which they have been deduced in the manner pointed out in Articles 11 to 13; these are omitted, partly on account of space, partly because the elements on which they depend had not the same value on different days by reason of frequent alterations made in the Y's and planes. The observations were made for the most part in the forenoon, between the hours of eight and eleven; but on the 16th, 22nd, and 25th of June, and 17th and 20th of July, they were made in the afternoon, between four and six o'clock.

The several columns contain the mark on the north pole of the needle, the values of f and g or of f' and g', according as B or as A was a north pole; and lastly, the time of horizontal vibration.

Observations with Needle 1.

May 20.	B	67° 11′ 0″	67° 58′ 46″	5″·87152
21.	A	57 1	35 14	5·81508
22.	A	56 29	36 45	5·82044
24.	B	16 45	45 48	5·81557
31.	B	18 1	49 41	5·82075
June 2.	A	53 55	33 9	5·85778
4.	A	56 38	32 10	5·86442
5.	B	24 13	46 44	5·83615
July 6.	A	59 41	35 21	5·83716
7.	A	58 7	37 51	5·83818
8.	B	20 8	44 47	5 89602
9.	B	20 43	44 25	5 90035

Observations with Needle 2.

May 20.	A	67° 40′ 57″	67° 20′ 37″	5″·72416
21.	A	41 8	21 5	5·72453
22.	B	43 28	50 45	5 65355
24.	B	41 43	54 32	5·66875
31.	A	43 34	18 29	5·67439
June 2.	A	41 46	18 12	5·67665
4.	B	42 42	46 57	5·68010
5.	B	44 53	50 24	5·68890
July 17.	B	45 20	50 17	5·70183
18.	A	40 26	22 50	5·68692
19.	A	40 21	22 10	5·69677
20.	B	40 40	54 19	5·66585

Observations with Needle 3.

June 8.	B	67° 47′ 58″	67° 48′ 52″	6″·17149
9.	B	40 55	42 28	6·18077
11.	A	30 58	32 35	6·18080
16.	B	40 0	42 40	6·17046
18.	B	43 13	47 40	6·18005
22.	A	27 33	39 19	6·16591
23.	A	29 46	41 8	6·16948
25.	A	29 3	41 7	6·17663
July 6.	A	32 38	40 37	6·18305
7.	B	45 56	42 12	6·17982
8.	B	46 59	43 37	6·18339
9.	A	30 42	39 42	6·23905

Observations with Needle 4.

June 8.	A	67° 45′ 9″	67° 27′ 3″	5″·96200
9.	B	22 56	68 8 28	5·91653
11.	B	23 16	7 48	5·94665
16.	A	49 54	67 12 8	6·01785
18.	B	27 48	68 8 45	5·93204
22.	B	26 46	3 56	5·94065
23.	A	50 19	67 15 37	5·93939
25.	A	50 4	15 22	5·94731
July 17.	A	50 13	15 43	5·96850
18.	A	49 57	14 48	5·96931
19.	B	22 43	68 9 18	5·92673
20.	B	22 41	10 19	5·92783

24.

In the calculation of these observations, instead of t, u employed above (Art. 21, 22), I shall introduce somewhat modified auxiliary quantities. If for one of the needles we denote by n the time of a horizontal vibration, by k the sum of the moments of inertia of the needle and of the stirrup in relation to the axis of rotation, which in these observations is vertical, and by l the length of the second's pendulum, then it is known that

$$l\, m\, n^2 \cos i = k.$$

Let us select a normal time of vibration N and a normal inclination I, being about the mean of the values of n and i, and let M denote the corresponding value of m, so that

$$l\, \mathrm{M}\, \mathrm{N}^2 \cos \mathrm{I} = k.$$

Lastly, let

$$x = \frac{q \cos \mathrm{Q} \,.\, \cos \mathrm{I} \,.\, 206265''}{\mathrm{M}}$$

$$y = \frac{q \sin \mathrm{Q} \,.\, \sin \mathrm{I} \,.\, 206265''}{\mathrm{M}},$$

which quantities are constant for all the observations with this needle. The equations then become

$$i = f + c - \frac{n^2 \cos f}{\mathrm{N}^2 \cos \mathrm{I}} \cdot \frac{\cos i}{\cos \mathrm{I}} \cdot x + \frac{n^2 \sin f}{\mathrm{N}^2 \sin \mathrm{I}} \cdot \frac{\cos i}{\cos \mathrm{I}} \cdot y$$

$$i = g - c - \frac{n^2 \cos g}{\mathrm{N}^2 \cos \mathrm{I}} \cdot \frac{\cos i}{\cos \mathrm{I}} \cdot x - \frac{n^2 \sin g}{\mathrm{N}^2 \sin \mathrm{I}} \cdot \frac{\cos i}{\cos \mathrm{I}} \cdot y$$

if B is a north pole; for the case of A a north pole, it is only necessary to give the contrary signs to the members which contain x and y.

This form has the advantage, that the coefficients of x and y always differ little from unity, and in fact with so little excentricity of the centre of gravity as in the four needles in question, and with such moderate fluctuations of n, we are justified in taking unity in lieu of those coefficients, which I term the abbreviated calculation. I have however been at the pains of calculating the 192 coefficients more exactly, leaving out only the factor $\frac{\cos i}{\cos \mathrm{I}}$, although the principal use of doing so is only to show more clearly the admissibility of the abbreviated calculation. In the sequel the non-accented letters N, x, y will refer to needle I, and the values for the remaining three needles will be distinguished respectively by one, two and three accents. The values selected for the present calculation are,—

$$\mathrm{I} = 67^\circ\ 40'\ 0''$$
$$\mathrm{N} = 5''{\cdot}847785$$
$$\mathrm{N}' = 5{\cdot}686867$$
$$\mathrm{N}'' = 6{\cdot}181742$$
$$\mathrm{N}''' = 5{\cdot}949567$$

I do not subjoin the calculations themselves in full, on account of the space they would occupy, but only so much of them as is necessary to give a general view of the proceedings. The values of the coefficients which differ most widely from unity, are 0·96895 and 1·04324 on the 9th and 16th of June with needle 4.

25.

From the two equations afforded by the observations of one needle on each day, two other equations, which we will call I. and II., may be formed, by halving both their sum and their difference. There arise thus 48 equations I. and as many II., the first of each of which are subjoined as examples. The original equations from the observations of the 20th May with needle 1 are,—

$$i = \overset{\circ}{67}\ \overset{\prime}{11}\ \ \overset{\prime\prime}{0} + c - 1{\cdot}02880\,x + 1{\cdot}00460\,y$$
$$i = 67\ \ 58\ \ 46 - c - 0{\cdot}99473\,x - 1{\cdot}01038\,y,$$

whence there arise the deduced equations—

$$i = 67^\circ\ 34'\ 53'' - 1{\cdot}01176\,x - 0{\cdot}00289\,y \ldots \text{(I.)}$$
$$c = +\ 1433'' + 0{\cdot}01703\,x - 1{\cdot}00749\,y \ \ldots \text{(II.)}$$

In order to be able to institute the test pointed out in Article 8, I have added another member to the equations I. by writing

$i+e$ instead of i, so that e expresses any supposed constant* error of needle 1, the presumptive constant error of needles 2, 3, 4 being expressed by e' e'' e'''.

In this manner the 48 equations I. contain in all 36 unknown quantities, namely, the inclinations on the 24 days of observation, and the 12 quantities x, y, e, x', y', e', x'', &c. But first we must remark, that the members which contain y, y', y'', y''' have all only very small coefficients, and that in the abbreviated calculation they are altogether wanting: the greatest of these 48 coefficients is 0·00289 in the instance above quoted. If however it is desired to take account of so small an influence, amounting only to a few seconds, the values of these y, y', y'', y''' must first have been otherwise deduced, in which however roughly approximate values will suffice for the object.

26.

For this deduction we have at our command the equations II. But if we consider that in the 12 equations of this division which relate to one needle, the letter c represents unequal values, its value being liable to alteration every time the poles are reversed by means of the bar-magnets, we shall easily perceive that it is impossible to eliminate this c out of the equations, and that we are therefore *necessitated* to call to our aid a somewhat precarious hypothesis. Mine is the following. As with all the fluctuations of c, supposing the same manner of touch in changing the poles to be always adhered to, a mean value of c will result, I assume that the mean value is the same for the two positions of the poles. It is true that if the number of changes in the poles has been but small, only a very imperfect compensation can be expected, and the value of y deduced in this manner will have but little certainty; but this uncertainty cannot be avoided in any way, unless we obtain the values of c by means of a special apparatus (Art. 17). In order to use this principle, those equations II. with each needle which relate to B a north pole, must first be separated from those in which A was a north pole; then both sets must be resolved into as many groups as the number of times when the magnetism of the needle was inter-

* The reader need hardly be reminded that such an error, which, supposing it to be real, must be ascribed to a deviation of the axle from the cylindrical form, is only constant so long as it is the same part of the axle that rests upon the planes, and therefore that with quite a different inclination, the error in question might have quite a different value.

fered with; then the mean must be taken of the equations belonging to the same group (if several come into one group), and then again the mean of these partial means must be taken; then by making the means so resulting equal to each other, the equation by which y is determined is obtained. I subjoin in illustration the abbreviated calculation for needle 1, in which I have employed *for this object*, in addition to the above 12 observations, three others *, made on the 1st and 7th of August and the 23rd of September. During the whole interval the needle was magnetised nine times, so that it was in ten different conditions, five belonging to each position of the poles.

Needle 1. B a North Pole.			Needle 1. A a North Pole.		
	$c + y =$			$c - y =$	
	$''$	$''$		$''$	$''$
May 20.	...	$+ 1433$	May 21.	$- 653$	$- 623$
24.	$+ 871$	$+ 910$	22.	$- 592$	
31.	$+ 950$		June 2.	$- 623$	$- 678$
June 5.	...	$+ 675$	4.	$- 734$	
July 8.	$+ 739$	$+ 723$	July 6.	$- 730$	$- 669$
9.	$+ 711$		7.	$- 608$	
Aug. 1.	$+ 720$		Aug. 1.	$- 720$	$- 752$
7.	$+ 584$	$+ 556$	7.	$- 785$	
Sept. 23.	$+ 528$		Sept. 23.	...	$- 680$
	Mean $c + y =$	$+ 859$		Mean $c - y =$	$- 680$

whence $y = + 769''$. The non-abbreviated calculation gave

for B a North Pole, $c = + 859 + 0{\cdot}00102\, x - 1{\cdot}00290\, y$

for A a North Pole, $c = - 680 + 0{\cdot}00082\, x + 0{\cdot}99915\, y$,

whence

$$y = + 769'' + 0{\cdot}00097\, x.$$

In like manner we find for the other three needles,

$$y' = + \ 456'' - 0{\cdot}00192\, x'$$
$$y'' = - \ 101 + 0{\cdot}00134\, x''$$
$$y''' = + 1107 + 0{\cdot}00224\, x'''.$$

The fluctuations in the values of c extend with needle 1 to $14\frac{1}{2}$ minutes, with needles 2 and 3 to $4\frac{1}{2}$ minutes, and with needle 4 to 10 minutes. Lest any particular importance should be attached to the circumstance that the widest result with needle 1 appears on the first day of observation, it should be noticed that

* The observation of the 23rd of September is that which was employed above as an example, Art. 9—22. The observations of the 1st and 7th of August are given in Art. 30.

the poles, both of this and of the other needles, had been often changed, and always with the same care and the same magnet-bars before any of the observations here given were made.

27.

After substituting the values of y, y', y'', y''' in the equations (1.), there still remain in those equations thirty-two unknown quantities. We then subtract throughout from each other the two equations belonging to observations of one and the same day, and twenty-four new equations are thus formed, which contain only the eight unknown quantities $x, x', x'', x''', e, e', e'', e'''$. The four latter however enter only into the differences between each two, so that if we say

$$\begin{aligned} e' - e &= d' \\ e'' - e &= d'' \\ e''' - e &= d''', \end{aligned}$$

only seven unknown quantities remain. The coefficients of d', d'', d''' all differ very little from $+1$ or -1. For determining the values of the seven unknown quantities by means of the method of least squares, we may omit, on account of the formation of the normal equations relating to x, x', x'', x''', the multiplication by the respective coefficients, so that for the formation of the whole seven normal equations simple addition only is required. The following normal equations have been found in this manner:—

$$\begin{aligned}
0 &= +4804'' + 12{\cdot}00266\,x - 0{\cdot}00708\,x' + 0{\cdot}01900\,x'' \\
0 &= -5806 + 0{\cdot}01559\,x + 12{\cdot}01005\,x' - 0{\cdot}00072\,x''' \\
0 &= -3228 + 0{\cdot}00145\,x + 12{\cdot}00544\,x'' + 0{\cdot}04561\,x''' \\
0 &= -5267 + 0{\cdot}01786\,x' - 0{\cdot}00489\,x'' + 12{\cdot}00343\,x''' \\
0 &= -297 + 0{\cdot}02717\,x + 0{\cdot}11088\,x' - 0{\cdot}04723\,x''' \\
&\qquad - 12\,d' + 4\,d''' \\
0 &= -241 + 0{\cdot}06326\,x + 0{\cdot}05839\,x'' - 0{\cdot}08085\,x''' \\
&\qquad - 12\,d'' + 8\,d''' \\
0 &= +254 - 0{\cdot}02682\,x' - 0{\cdot}02676\,x'' + 0{\cdot}12808\,x''' \\
&\qquad + 4\,d' + 8\,d'' - 12\,d'''
\end{aligned}$$

and hence the values

$$\begin{aligned}
x &= -400'' \\
x' &= +484 \\
x'' &= +267 \\
x''' &= +438 \\
d' &= -22
\end{aligned}$$

$$d'' = - \quad 23$$
$$d''' = + \quad 1$$

Instead of the three latter, by making

$$\tfrac{1}{4}(e + e' + e'' + e''') = \epsilon,$$

we may write

$$e \;= + 11'' + \epsilon$$
$$e' \;= - 11 \;+ \epsilon$$
$$e'' = - 12 \;+ \epsilon$$
$$e''' = + 12 \;+ \epsilon,$$

where the part in common (ϵ) is evidently not determinable from the data at our disposal. The substitution of the values found for x, e, x', e', &c. in the equations I. (already freed from y, y', &c.) now gives us, omitting ϵ, the following inclinations:—

	Needle.		Needle.	
May 20.	1	67° 41′ 25″	2	67° 39′ 12″
21.	...	39 21	...	39 31
22.	...	39 51	...	39 22
24.	...	37 43	...	40 21
31.	...	40 17	...	39 17
June 2.	...	36 39	...	38 16
4.	...	37 31	...	37 0
5.	...	41 56	...	39 48
8.	3	44 12	4	43 14
9.	...	37 27	...	38 15
11.	...	36 27	...	38 1
16.	...	37 6	...	38 17
18.	...	41 12	...	40 48
22.	...	38 5	...	37 51
23.	...	40 6	...	40 2
25.	...	39 45	...	39 49
July 6.	1	40 42	3	41 17
7.	...	41 11	...	39 49
8.	...	39 5	...	41 3
9.	...	39 12	...	39 57
17.	2	39 55	4	40 7
18.	...	39 56	...	39 31
19.	...	39 35	...	38 32
20.	...	39 43	...	39 1

28.

The differences between the two determinations of the inclination on each day must now give us the measure of the uncertainty of the observations themselves. The greatest difference (on the 24th of May) amounts to 2′ 38″, and the sum of the squares of all the twenty-four differences, taking the second as unity, is 124389. From the principles of the calculus of probabilities, it is easy to deduce, that if we give equal weight to the observations with each of the four needles (and we have no reason to depart from this supposition), the mean uncertainty of a value of $\frac{1}{2}(f+g)$ or $\frac{1}{2}(f'+g')$, as found from the observations and subjected to our calculations, is, as far as we can form a judgement from our numbers,

$$= \sqrt{\frac{124389}{34}} = 60''\cdot 5,$$

if we speak only of accidental or irregular errors of observation. The mean of two such numbers, founded on observations independent of each other, is therefore charged with a mean uncertainty

$$= \sqrt{\frac{124389}{68}} = 42''\cdot 8;$$

and this may also be regarded as the mean error of an inclination determined in the usual manner (*i. e.* with one needle in both positions of the poles), in as far as the small correction required for $\frac{1}{4}(f+g+f'+g')$ may either be regarded as quite insensible, or may be based on an independent determination of u or y (compare Art. 21.). Of course this valuation of the error applies only to the instrument under consideration, and to observations of which the fundamental circumstances are similar to these. With a less number of positions than eight in each combination, the degree of confidence due to the result would be less, although I would not choose to affirm the mean error of the final result to be in an exact inverse proportion to the square root of the number of readings after successive raising and lowering by means of the Y's.

On the other hand, I must not omit to notice that during the whole of the above observations the agate planes could not be adjusted as perfectly as I wished, and afterwards accomplished by means of the instrument alluded to in Art. 5. Any possible increase of the error of observation from an imperfect adjustment of the planes (wherein a constant amount of influence is the less supposable, because frequent alterations were made in the planes) is already included in the above number, and I have therefore reason to expect that future observations with the same instrument would rather show still smaller errors. It results from a separate investigation, of which I omit the details, that the mean uncertainty of the forty-eight inclinations in the preceding article does not differ much from the mean uncertainty of $\frac{1}{2}(f+g)$, and that nearly twice the weight, and consequently a mean error of 42″·8, must be assigned to the means of each corresponding pair, as shown in Art. 30.

29.

A particularly remarkable and welcome result is afforded by the smallness of the values found for e, e', e'', e''', or rather immediately for their differences from their mean ϵ. A separate investigation has given the weight of these determinations $\frac{96}{11}$ times greater than the weight of $\frac{1}{4}(f+g)$, consequently the mean uncertainty with which they are charged $= 60''\cdot5 \sqrt{\frac{11}{96}} = 20''\cdot5$; whence it is evident that even the reality of any inequalities between e, e', e'', e''' remains wholly doubtful. Now as it is highly improbable that in four needles there should be constant errors of almost precisely equal amount, we are justified in assuming that they have either no constant errors, or, at least only such as are wholly insensible; and hence it might appear almost superfluous to make use of the capability of revolution of the axles of two of the needles for the purpose of further trial.

For one of the needles, No. 4, this conclusion is actually confirmed by some earlier observations. During four days, from the 15th to the 19th of May, similarly combined observations to the subsequent ones (8th to the 25th of June) were made only with the difference, that each partial result rested on only four positions instead of eight: in needle 3 the axle was in the same position as in the later observations, but in needle 4 it was differently placed, having on the 19th of May been made to revolve through nearly a quadrant. The observations arranged as in October 23 are as follows:—

Observations with Needle 3.

		° ′ ″	° ′ ″	s.
May 15.	B	67 41 26	67 44 53	6·16166
17.	B	43 52	45 52	6·20333
18.	A	33 56	39 15	6·17781
19.	A	36 8	37 8	6·19566

Observations with Needle 4.

		° ′ ″	° ′ ″	s.
May 15.	A	67 14 28	67 47 49	5·94332
17.	B	68 5 39	36 36	5·92034
18.	B	68 3 30	36 13	5·94235
19.	A	67 3 4	59 47	5·94663

All the observations were made in the forenoon.

In the calculation the values of x'', y'', e'', found above, were employed for needle 3, but for needle 4 the values of x''', y''', e''' had to be deduced as well as they could from the observations themselves, whereby it was found that

$$y''' = -1103''$$
$$x''' = +556''$$
$$e''' - \epsilon = +24''.$$

The determination of y''', resting on so few observations, is no doubt very uncertain; but the influence of the uncertainty on the reduction of $\frac{1}{2}(f+g)$ is quite insignificant, as the greatest coefficient of y''', in the equations I., is only 0·00341. The results for i then stand thus:—

	Needle 3.	Needle 4.
	° ′ ″	° ′ ″
May 15.	67 38 57	67 40 0
17.	40 36	41 36
18.	41 15	40 15
19.	41 19	40 16

The weight of the determination of $e''' - \epsilon$ is here only twice as great as the weight of $\frac{1}{2}(f+g)$, and as the observations themselves have considerably less exactness than the later ones, it is obvious that the value now found says as little for the reality of a constant error as that which was deduced from the later observations.

The great departure of the values of x''' and y''' from those found in Art. 26, 27, only proves that the revolving part of the needle, considered in itself, has not its centre of gravity in the axis of rotation, which, however, is of little consequence.

30.

I now bring together the final results for the inclination, deduced from all the observations which have been treated of, and include among them the observations of the 1st and 7th of August, which have been already mentioned, and which were made with needle 1 in a similar manner in all respects to those on the 23rd of September. These observations were the following:—

	August 1.	August 7.
	° ′ ″	° ′ ″
f	67 20 12	67 22 41
g	44 11	67 42 8
f'	59 53	68 1 56
g'	35 53	67 35 46

Determinations of Inclination.

	° ′ ″		° ′ ″
1842. May 15.	67 39 28	June 18.	67 41 0
17.	41 06	22.	37 58
18.	40 45	23.	40 4
19.	40 47	25.	39 47
20.	40 18	July 6.	41 0
21.	39 26	7.	40 30
22.	39 36	8.	40 4
24.	39 2	9.	39 34
31.	39 47	17.	40 1
June 2.	37 27	18.	39 44
4.	37 15	19.	39 4
5.	40 52	20.	39 22
8.	43 43	Aug. 1.	39 57
9.	37 51	7.	40 26
11.	37 14	Sept. 23.	40 54
16.	37 42		

The mean of these thirty-one determinations, without considering any difference of weight, is

$$67^\circ\ 39'\ 44'',$$

and may be regarded as corresponding to the 21st of June. The mean of the twenty-four determinations only which are comprised between the 20th of May and the 20th of July, corresponds to the middle period of the 19th of June, and is

$$67^\circ\ 39'\ 31''.$$

31.

The differences of the inclinations on the thirty-one several days of observation from their mean, are compounded of the still remaining influence of the errors of observation, and of the real inequalities of the inclination itself. It is true that these component parts cannot be separated for single days, but with so numerous a series the estimation of a mean value of the actual fluctuations may well be attempted. With this view I have first reduced the inclinations to the 21st of June, assuming a regular annual diminution of 3 minutes, and have have then added together the squares of the differences from the mean value; the sum is 220184″, which divided by 30 gives 7339″·5 as the square of the mean error to which we should be liable, in taking at hazard any one of those thirty-one inclinations and regarding it as valid for the mean epoch of observation. If we wish to take into account the unequal weight of the three groups of observations, and call the mean error of the four first observations m', that of the three last m'', and that of the other twenty-four m, and the mean fluctuation of the inclination itself M, we may derive from the principles of the calculus of probabilities the following equation :—

$$7339''\cdot 5 = \frac{24\, m^2 + 4\, m'^2 + 3\, m''^2}{31} + M^2.$$

For m^2 we found above the value 1829·25, or at least this number may be regarded as a sufficient approximation; for the seven other observations, in the absence of any more certain standard, we may use as a basis the number of positions from whence the results are derived, thus

$$m'^2 = 2\, m^2, \quad m''^2 = \tfrac{8}{3}\, m^2,$$

whence we have

$$M^2 = 7339\cdot 5 - \frac{184}{155}\,.\,1829\cdot 25 = 5168$$

and $M = 71''\cdot 9$.

32.

I had made in the preceding year, with the same instrument and in the same place, a series of inclination observations, of which I subjoin only the final results.

1841. Sept.	22	. . . . .	67°	40′	20″
	24	. . . . .		40	53
	27	. . . . .		46	41
Oct.	2	. . . . .		42	57
	7	. . . . .		42	14
	10	. . . . .		42	40
	12	. . . . .		43	15
	20	. . . . .		44	2
	20	. . . . .		42	5
	22	. . . . .		42	52
Mean, Oct.	8	. . . . .	67°	42′	48″

The first eight observations were made in the same manner as those of the present year, by observing on each day two needles (Nos. 1 and 2) without changing their poles between the observations; the two last observations were made in the usual manner. The second observation made on October 20, was with needle 4, that of the 22nd of October was with needle 3. The time was on the 27th of September and 10th of October, in the afternoon, between 3 and 5 o'clock; on all other occasions it was in the forenoon. Each of these ten inclinations was deduced from sixteen positions, and even on this account alone they would have proportionably less weight than the inclinations of 1842, which rest respectively on thirty-two, sixty-four, and forty positions.

33.

All the inclinations hitherto given still require a small common correction, on account of the influence exerted at the place of observation by the magnets in the Astronomical and Magnetical Observatories: 5″·15 must be deducted from the results throughout to free them from this effect. (Compare *Resultate*, vol. v. page 33.)

The *absolute* degree of confidence due to the determinations of inclination still remains dependent on the correctness of the assumption, that the instrument itself contains no particles which can act magnetically on the needle. There are no grounds for such an apprehension in the case of the instrument which I have employed; some observations which I made with a loaded needle in the manner mentioned in Article 18, always gave discrepancies of a few minutes only, which are quite naturally explained, partly by the unavoidable errors of observation, and partly from the actual anomalies of the inclination itself. The sufficiently satisfactory agreement of the values found in Article 11 for the quantity there designated by a is also opposed to the existence of such disturbances. It is true that such tests are not suited to the detection of very small influences, and I must therefore reserve a further test by more searching means.

34.

In conclusion I subjoin my own results, together with some earlier determinations by other observers.

1805. Dec.	. . .	69° 29′		Humboldt.
1826. Sept.	. . .	68 29 26		Humboldt.
1837. July 1	. . .	67 47 0		Forbes.
... ...	. . .	67 53 30		Forbes.
1841. Oct. 8	. .	67 42 43		
1842. June 21	. .	67 39 39		

The two first of these results are taken from the additions to the thirteenth volume of the *Voyage aux Régions Equinoxiales*, p. 152; the first was made with an Inclinatorium of Lenoir's; the second with an instrument of Gambey's: the latter rests on two needles, of which the separate results are given as 68° 30′ 7″ and 68° 28′ 15″, with which the above mean does not quite agree; probably the result with the second needle has been made 30″ too small by an error of the press. I do not know the particular locality of the observation of 1805; that of 1826 was taken in the open field some hundred paces east of the Astronomical Observatory.

Forbes's observations are printed in th. Transactions of the Royal Society of Edinburgh, vol. xv. part 1. pp. 31 and 32; they were made in the garden of the Astronomical Observatory with an instrument of Robinson's of smaller dimensions than mine, having two needles six English inches in length: the observer himself regards the second needle as the best of the two.

I have not included in this series the observations made by Mayer in March 1814, and recorded in the *Commentationes recent. Soc. Gotting.*, vol. iii. pp. 36, 37, because they are undeserving of confidence. The example which Mayer himself gives in p. 35 shows how very imperfect was the instrument used by him; its place being unchanged, it gave in ten partial results differences of more than a degree. Even the final results on two different days differ half a degree.

My own observations of June 23, 1832, recorded in p. 44 of the *Intens. vis Magneticæ Terrestris*, is equally undeserving of a place here, as well on account of the imperfection of the instrument, as on account of the place of observation which was in the Astronomical Observatory, where iron at no great distance materially affected the result and appears to have produced an increase of inclination.

These inclinations admit of being brought into very good accord by means of the assumption of an annual uniform decrease of 3 minutes (or more exactly of 3′ 2″·3), if in Forbes's observations we keep to the result with the second needle; and there then remains only such differences as we may well ascribe to the combined influence of errors of observation and of fluctuations of the inclination. As however it appears from Hansteen's investigations on observations at other European stations, that the yearly decrease has gradually become less, we should regard the number above given rather in the light of a mean value corresponding to about the epoch of 1829, awaiting from future observations the confirmation and more exact determination of its non-uniformity.

Article XXIX.

√

Sketch of the Analytical Engine invented by Charles Babbage *Esq. By* L. F. Menabrea, *of Turin, Officer of the Military Engineers.*

[From the *Bibliothèque Universelle de Génève*, No. 82. October 1842.]

[Before submitting to our readers the translation of M. Menabrea's memoir 'On the Mathematical Principles of the Analytical Engine' invented by Mr. Babbage, we shall present to them a list of the printed papers connected with the subject, and also of those relating to the Difference Engine by which it was preceded.

For information on Mr. Babbage's "*Difference* Engine," which is but slightly alluded to by M. Menabrea, we refer the reader to the following sources:—

1. Letter to Sir Humphry Davy, Bart., P.R.S., on the Application of Machinery to Calculate and Print Mathematical Tables. By Charles Babbage, Esq., F.R.S. London, July 1822. Reprinted, with a Report of the Council of the Royal Society, by order of the House of Commons, May 1823.

2. On the Application of Machinery to the Calculation of Astronomical and Mathematical Tables. By Charles Babbage, Esq.—Memoirs of the Astronomical Society, vol. i. part 2. London, 1822.

3. Address to the Astronomical Society by Henry Thomas Colebrooke, Esq., F.R.S., President, on presenting the first Gold Medal of the Society to Charles Babbage, Esq., for the invention of the Calculating Engine.—Memoirs of the Astronomical Society. London, 1822.

4. On the Determination of the General Term of a New Class of Infinite Series. By Charles Babbage, Esq.—Transactions of the Cambridge Philosophical Society.

5. On Mr. Babbage's New Machine for Calculating and Printing Mathematical Tables.—Letter from Francis Baily, Esq., F.R.S., to M. Schumacher. No. 46, Astronomische Nachrichten. Reprinted in the Philosophical Magazine, May 1824.

6. On a Method of expressing by Signs the Action of Ma-

chinery. By Charles Babbage, Esq.—Philosophical Transactions. London, 1826.

7. On Errors common to many Tables of Logarithms. By Charles Babbage, Esq.—Memoirs of the Astronomical Society, London, 1827.

8. Report of the Committee appointed by the Council of the Royal Society to consider the subject referred to in a communication received by them from the Treasury respecting Mr. Babbage's Calculating Engine, and to report thereon. London, 1829.

9. Economy of Manufactures, chap. xx. 8vo. London, 1832.

10. Article on Babbage's Calculating Engine.—Edinburgh Review, July 1834. No. 120. vol. lix.

The present state of the Difference Engine, which has always been the property of Government, is as follows:—The drawings are nearly finished, and the mechanical notation of the whole, recording every motion of which it is susceptible, is completed. A part of that Engine, comprising sixteen figures, arranged in three orders of differences, has been put together, and has frequently been used during the last eight years. It performs its work with absolute precision. This portion of the Difference Engine, together with all the drawings, are at present deposited in the Museum of King's College, London.

Of the ANALYTICAL ENGINE, which forms the principal object of the present memoir, we are not aware that any notice has hitherto appeared, except a Letter from the Inventor to M. Quetelet, Secretary to the Royal Academy of Sciences at Brussels, by whom it was communicated to that body. We subjoin a translation of this Letter, which was itself a translation of the original, and was not intended for publication by its author.

Royal Academy of Sciences at Brussels. General Meeting of the 7th and 8th of May, 1835.

"A Letter from Mr. Babbage announces that he has for six months been engaged in making the drawings of a new calculating machine of far greater power than the first.

"'I am myself astonished,' says Mr. Babbage, 'at the power I have been enabled to give to this machine; a year ago I should not have believed this result possible. This machine is intended to contain a hundred variables (or numbers susceptible of changing); each of these numbers may consist of twenty-five figures, $v_1, v_2, \ldots\ldots v_n$ being any numbers whatever, n being less than a hundred; if $f(v_1, v_2, v_3, \ldots v_n)$ be any given function which can be formed by addition, subtraction, multiplication, division, extraction of roots, or elevation to powers, the machine will calculate its numerical value; it will afterwards substitute this value in the place of v, or of any other variable, and will calculate this second function with respect to v. It will reduce to tables almost all equations of finite differences. Let us suppose that we have observed a thousand values of a, b, c, d, and that we wish to calculate them by the formula $p = \sqrt{\frac{a+b}{c\,d}}$, the machine must be set to calculate the formula; the first series of the values of a, b, c, d must be adjusted to it; it will then calculate them, print them, and reduce them to zero; lastly, it will ring a bell to give notice that a new set of constants must be inserted. When there exists a relation between any number of successive coefficients of a series, provided it can be expressed as has already been said, the machine will calculate them and make their terms known in succession; and it may afterwards be disposed so as to find the value of the series for all the values of the variable.'

"Mr. Babbage announces, in conclusion, 'that the greatest difficulties of the invention have already been surmounted, and that the plans will be finished in a few months.'"

In the Ninth Bridgewater Treatise, Mr. Babbage has employed several arguments deduced from the Analytical Engine, which afford some idea of its powers. See Ninth Bridgewater Treatise, 8vo, second edition. London, 1834.

Some of the numerous drawings of the Analytical Engine have been engraved on wooden blocks, and from these (by a mode contrived by Mr. Babbage) various stereotype plates have been taken. They comprise—

1. Plan of the figure wheels for one method of adding numbers.
2. Elevation of the wheels and axis of ditto.
3. Elevation of framing only of ditto.
4. Section of adding wheels and framing together.
5. Section of the adding wheels, sign wheels and framing complete.
6. Impression from the original wood block.

7. Impressions from a stereotype cast of No. 6, with the letters and signs inserted. Nos. 2, 3, 4 and 5 were stereotypes taken from this.

8. Plan of adding wheels and of long and short pinions, by means of which *stepping* is accomplished.

N.B. This process performs the operation of multiplying or dividing a number by any power of ten.

9. Elevation of long pinions in the position for addition.

10. Elevation of long pinions in the position for stepping.

11. Plan of mechanism for carrying the tens (by anticipation), connected with long pinions.

12. Section of the chain of wires for anticipating carriage.

13. Sections of the elevation of parts of the preceding carriage.

All these were executed about five years ago. At a later period (August 1840) Mr. Babbage caused one of his general plans (No. 25) of the whole Analytical Engine to be lithographed at Paris.

Although these illustrations have not been published, on account of the time which would be required to describe them, and the rapid succession of improvements made subsequently, yet copies have been freely given to many of Mr. Babbage's friends, and were in August 1838 presented at Newcastle to the British Association for the Advancement of Science, and in August 1840 to the Institute of France through M. Arago, as well as to the Royal Academy of Turin through M. Plana.—Editor.]

Those labours which belong to the various branches of the mathematical sciences, although on first consideration they seem to be the exclusive province of intellect, may, nevertheless, be divided into two distinct sections; one of which may be called the mechanical, because it is subjected to precise and invariable laws, that are capable of being expressed by means of the operations of matter; while the other, demanding the intervention of reasoning, belongs more specially to the domain of the understanding. This admitted, we may propose to execute, by means of machinery, the mechanical branch of these labours, reserving for pure intellect that which depends on the reasoning faculties. Thus the rigid exactness of those laws which regulate numerical calculations must frequently have suggested the employment of material instruments, either for executing the whole of such calculations or for abridging them; and thence have arisen several inventions having this object in view, but which have in general but partially attained it. For instance, the much-admired machine of Pascal is now simply an object of curiosity, which, whilst it displays the powerful intellect of its inventor, is yet of little utility in itself. Its powers extended no further than the execution of the four* first operations of arithmetic, and indeed were in reality confined to that of the two first, since multiplication and division were the result of a series of additions and subtractions. The chief drawback hitherto on most of such machines is, that they require the continual intervention of a human agent to regulate their movements, and thence arises a source of errors; so that, if their use has not become general for large numerical calculations, it is because they have not in fact resolved the double problem which the question presents, that of *correctness* in the results, united with *œconomy* of time.

Struck with similar reflections, Mr. Babbage has devoted some years to the realization of a gigantic idea. He proposed to himself nothing less than the construction of a machine capable of executing not merely arithmetical calculations, but even all those of analysis, if their laws are known. The imagination is at first astounded at the idea of such an undertaking; but the more calm reflection we bestow on it, the less impossible does success appear, and it is felt that it may depend on the discovery of some principle so general, that if applied to machinery, the latter may be capable of mechanically translating the operations which may be indicated to it by algebraical notation. The illustrious inventor having been kind enough to communicate to me

* This remark seems to require further comment, since it is in some degree calculated to strike the mind as being at variance with the subsequent passage (page 675), where it is explained that *an engine which can effect these four operations* can in fact effect *every species of calculation*. The apparent discrepancy is stronger too in the translation than in the original, owing to its being impossible to render precisely into the English tongue all the niceties of distinction which the French idiom happens to admit of in the phrases used for the two passages we refer to. The explanation lies in this: that in the one case the execution of these four operations is the *fundamental starting-point*, and the object proposed for attainment by the machine is the *subsequent combination of these* in every possible variety; whereas in the other case the execution of some *one* of these four operations, selected at pleasure, is the *ultimatum*, the sole and utmost result that can be proposed for attainment by the machine referred to, and which result it cannot any further combine or work upon. The one *begins* where the other *ends*. Should this distinction not now appear perfectly clear, it will become so on perusing the rest of the Memoir, and the Notes that are appended to it.—NOTE BY TRANSLATOR.

some of his views on this subject during a visit he made at Turin, I have, with his approbation, thrown together the impressions they have left on my mind. But the reader must not expect to find a description of Mr. Babbage's engine; the comprehension of this would entail studies of much length; and I shall endeavour merely to give an insight into the end proposed, and to develope the principles on which its attainment depends.

I must first premise that this engine is entirely different from that of which there is a notice in the 'Treatise on the Œconomy of Machinery,' by the same author. But as the latter gave rise* to the idea of the engine in question, I consider it will be a useful preliminary briefly to recall what were Mr. Babbage's first essays, and also the circumstances in which they originated.

It is well known that the French government, wishing to promote the extension of the decimal system, had ordered the construction of logarithmical and trigonometrical tables of enormous extent. M. de Prony, who had been entrusted with the direction of this undertaking, divided it into three sections, to each of which were appointed a special class of persons. In the first section the formulæ were so combined as to render them subservient to the purposes of numerical calculation; in the second, these same formulæ were calculated for values of the variable, selected at certain successive distances; and under the third section, comprising about eighty individuals, who were most of them only acquainted with the two first rules of arithmetic, the values which were intermediate to those calculated by the second section were interpolated by means of simple additions and subtractions.

An undertaking similar to that just mentioned having been entered upon in England, Mr. Babbage conceived that the operations performed under the third section might be executed by a machine; and this idea he realized by means of mechanism,

* The idea that the one engine is the offspring and has grown out of the other, is an exceedingly natural and plausible supposition, until reflection reminds us that no *necessary* sequence and connexion need exist between two such inventions, and that they *may* be wholly independent. M. Menabrea has shared this idea in common with persons who have not his profound and accurate insight into the nature of either engine. In Note A. (see the Notes at the end of the Memoir) it will be found sufficiently explained, however, that this supposition is unfounded. M. Menabrea's opportunities were by no means such as could be adequate to afford him information on a point like this, which would be naturally and almost unconsciously *assumed,* and would scarcely suggest any inquiry with reference to it.—NOTE BY TRANSLATOR.

which has been in part put together, and to which the name Difference Engine is applicable, on account of the principle upon which its construction is founded. To give some notion of this, it will suffice to consider the series of whole square numbers, 1, 4, 9, 16, 25, 36, 49, 64, &c. By subtracting each of these from the succeeding one, we obtain a new series, which we will name the Series of First Differences, consisting of the numbers 3, 5, 7, 9, 11, 13, 15, &c. On subtracting from each of these the preceding one, we obtain the Second Differences, which are all constant and equal to 2. We may represent this succession of operations, and their results, in the following table:—

	A. Column of Square Numbers.	B. First Differences.	C. Second Differences.	
	1			
		3		
	4		2	*b*
		5		
a	9		2	*d*
		7		
c	16		2	
		9		
	25		2	
		11		
	36			

From the mode in which the two last columns B and C have been formed, it is easy to see that if, for instance, we desire to pass from the number 5 to the succeeding one 7, we must add to the former the constant difference 2; similarly, if from the square number 9 we would pass to the following one 16, we must add to the former the difference 7, which difference is in other words the preceding difference 5, plus the constant difference 2; or again, which comes to the same thing, to obtain 16 we have only to add together the three numbers 2, 5, 9, placed obliquely in the direction *a b*. Similarly, we obtain the number 25 by summing up the three numbers placed in the oblique direction *d c*: commencing by the addition 2 + 7, we have the first difference 9 consecutively to 7; adding 16 to the 9 we have the square 25. We see then that the three numbers 2, 5, 9 being given, the whole series of successive square numbers, and that of their first differences likewise, may be obtained by means of simple additions.

Now, to conceive how these operations may be reproduced by a machine, suppose the latter to have three dials, designated as A, B, C, on each of which are traced, say a thousand divisions, by way of example, over which a needle shall pass. The two dials, C, B, shall have in addition a registering hammer, which is to give a number of strokes equal to that of the divisions indicated by the needle. For each stroke of the registering hammer of the dial C, the needle B shall advance one division;

similarly, the needle A shall advance one division for every stroke of the registering hammer of the dial B. Such is the general disposition of the mechanism.

This being understood, let us at the beginning of the series of operations we wish to execute, place the needle C on the division 2, the needle B on the division 5, and the needle A on the division 9. Let us allow the hammer of the dial C to strike; it will strike twice, and at the same time the needle B will pass over two divisions. The latter will then indicate the number 7, which succeeds the number 5 in the column of first differences. If we now permit the hammer of the dial B to strike in its turn, it will strike seven times, during which the needle A will advance seven divisions; these added to the nine already marked by it, will give the number 16, which is the square number consecutive to 9. If we now recommence these operations, beginning with the needle C, which is always to be left on the division 2, we shall perceive that by repeating them indefinitely, we may successively reproduce the series of whole square numbers by means of a very simple mechanism.

The theorem on which is based the construction of the machine we have just been describing, is a particular case of the following more general theorem: that if in any polynomial whatever, the highest power of whose variable is m, this same variable be increased by equal degrees; the corresponding values of the polynomial then calculated, and the first, second, third, &c. differences of these be taken (as for the preceding series ot squares); the mth differences will all be equal to each other. So that, in order to reproduce the series of values of the polynomial by means of a machine analogous to the one above described, it is sufficient that there be $(m + 1)$ dials, having the mutual relations we have indicated. As the differences may be either positive or negative, the machine will have a contrivance for either advancing or retrograding each needle, according as the number to be algebraically added may have the sign *plus* or *minus*.

If from a polynomial we pass to a series having an infinite number of terms, arranged according to the ascending powers of the variable, it would at first appear, that in order to apply the machine to the calculation of the function represented by such a series, the mechanism must include an infinite number of dials, which would in fact render the thing impossible. But in many cases the difficulty will disappear, if we observe that for a great number of functions the series which represent them may be rendered convergent; so that, according to the degree of approximation desired, we may limit ourselves to the calculation of a certain number of terms of the series, neglecting the rest. By this method the question is reduced to the primitive case of a finite polynomial. It is thus that we can calculate the succession of the logarithms of numbers. But since, in this particular instance, the terms which had been originally neglected receive increments in a ratio so continually increasing for equal increments of the variable, that the degree of approximation required would ultimately be affected, it is necessary, at certain intervals, to calculate the value of the function by different methods, and then respectively to use the results thus obtained, as data whence to deduce, by means of the machine, the other intermediate values. We see that the machine here performs the office of the third section of calculators mentioned in describing the tables computed by order of the French government, and that the end originally proposed is thus fulfilled by it.

Such is the nature of the first machine which Mr. Babbage conceived. We see that its use is confined to cases where the numbers required are such as can be obtained by means of simple additions or subtractions; that the machine is, so to speak, merely the expression of one* particular theorem of analysis; and that, in short, its operations cannot be extended so as to embrace the solution of an infinity of other questions included within the domain of mathematical analysis. It was while contemplating the vast field which yet remained to be traversed, that Mr. Babbage, renouncing his original essays, conceived the plan of another system of mechanism whose operations should themselves possess all the generality of algebraical notation, and which, on this account, he denominates the *Analytical Engine.*

Having now explained the state of the question, it is time for me to develope the principle on which is based the construction of this latter machine. When analysis is employed for the solution of any problem, there are usually two classes of operations to execute: firstly, the numerical calculation of the various coefficients; and secondly, their distribution in relation to the quantities affected by them. If, for example, we have to obtain

* See Note A.

the product of two binomials $(a + b x)$ $(m + n x)$, the result will be represented by $a m + (a n + b m)$ $x + b n x^2$, in which expression we must first calculate $a m$, $a n$, $b m$, $b n$; then take the sum of $a n + b m$; and lastly, respectively distribute the coefficients thus obtained, amongst the powers of the variable. In order to reproduce these operations by means of a machine, the latter must therefore possess two distinct sets of powers: first, that of executing numerical calculations; secondly, that of rightly distributing the values so obtained.

But if human intervention were necessary for directing each of these partial operations, nothing would be gained under the heads of correctness and œconomy of time; the machine must therefore have the additional requisite of executing by itself all the successive operations required for the solution of a problem proposed to it, when once the *primitive numerical data* for this same problem have been introduced. Therefore, since from the moment that the nature of the calculation to be executed or of the problem to be resolved have been indicated to it, the machine is, by its own intrinsic power, of itself to go through all the intermediate operations which lead to the proposed result, it must exclude all methods of trial and guess-work, and can only admit the direct processes of calculation*.

It is necessarily thus; for the machine is not a thinking being, but simply an automaton which acts according to the laws imposed upon it. This being fundamental, one of the earliest researches its author had to undertake, was that of finding means for effecting the division of one number by another without using the method of guessing indicated by the usual rules of arithmetic. The difficulties of effecting this combination were far from being among the least; but upon it depended the success of every other. Under the impossibility of my here explaining the process through which this end is attained, we must limit ourselves to admitting that the four first operations of arithmetic, that is addition, subtraction, multiplication and division, can be performed in a direct manner through the intervention of the machine. This granted, the machine is thence capable of performing every species of numerical calculation, for all such calculations ultimately resolve themselves into the four operations

* This must not be understood in too unqualified a manner. The engine is capable, under certain circumstances, of feeling about to discover which of two or more possible contingencies has occurred, and of then shaping its future course accordingly.—NOTE BY TRANSLATOR.

we have just named. To conceive how the machine can now go through its functions according to the laws laid down, we will begin by giving an idea of the manner in which it materially represents numbers.

Let us conceive a pile or vertical column consisting of an indefinite number of circular discs, all pierced through their centres by a common axis, around which each of them can take an independent rotatory movement. If round the edge of each of these discs are written the ten figures which constitute our numerical alphabet, we may then, by arranging a series of these figures in the same vertical line, express in this manner any number whatever. It is sufficient for this purpose that the first disc represent units, the second tens, the third hundreds, and so on. When two numbers have been thus written on two distinct columns, we may propose to combine them arithmetically with each other, and to obtain the result on a third column. In general, if we have a series of columns* consisting of discs, which columns we will designate as V_0, V_1, V_2, V_3, V_4, &c., we may require, for instance, to divide the number written on the column V_1 by that on the column V_4, and to obtain the result on the column V_7. To effect this operation, we must impart to the machine two distinct arrangements; through the first it is prepared for executing *a division*, and through the second the columns it is to operate on are indicated to it, and also the column on which the result is to be represented. If this division is to be followed, for example, by the addition of two numbers taken on other columns, the two original arrangements of the machine must be simultaneously altered. If, on the contrary, a series of operations of the same nature is to be gone through, then the first of the original arrangements will remain, and the second alone must be altered. Therefore, the arrangements that may be communicated to the various parts of the machine, may be distinguished into two principal classes:

First, that relative to the *Operations*.

Secondly, that relative to the *Variables*.

By this latter we mean that which indicates the columns to be operated on. As for the operations themselves, they are executed by a special apparatus, which is designated by the name of *mill*, and which itself contains a certain number of columns, similar to those of the Variables. When two numbers are to be

* See Note B.

combined together, the machine commences by effacing them from the columns where they are written, that is it places *zero* * on every disc of the two vertical lines on which the numbers were represented; and it transfers the numbers to the mill. There, the apparatus having been disposed suitably for the required operation, this latter is effected, and, when completed, the result itself is transferred to the column of Variables which shall have been indicated. Thus the mill is that portion of the machine which works, and the columns of Variables constitute that where the results are represented and arranged. After the preceding explanations, we may perceive that all fractional and irrational results will be represented in decimal fractions. Supposing each column to have forty discs, this extension will be sufficient for all degrees of approximation generally required.

It will now be inquired how the machine can of itself, and without having recourse to the hand of man, assume the successive dispositions suited to the operations. The solution of this problem has been taken from Jacquard's apparatus †, used for the manufacture of brocaded stuffs, in the following manner:—

Two species of threads are usually distinguished in woven stuffs; one is the *warp* or longitudinal thread, the other the *woof* or transverse thread, which is conveyed by the instrument called the shuttle, and which crosses the longitudinal thread or warp. When a brocaded stuff is required, it is necessary in turn to prevent certain threads from crossing the woof, and this according to a succession which is determined by the nature of the design that is to be reproduced. Formerly this process was lengthy and difficult, and it was requisite that the workman, by attending to the design which he was to copy, should himself regulate the movements the threads were to take. Thence arose the high price of this description of stuffs, especially if threads of various colours entered into the fabric. To simplify this manufacture, Jacquard devised the plan of connecting each group of threads that were to act together, with a distinct lever belonging exclusively to that group. All these levers terminate in rods, which are united together in one bundle, having usually the form of a parallelopiped with a rectangular base. The rods are cylindrical, and are separated from each other by small in-

* Zero is not *always* substituted when a number is transferred to the mill. This is explained further on in the memoir, and still more fully in Note D.—NOTE BY TRANSLATOR.

† See Note C.

tervals. The process of raising the threads is thus resolved into that of moving these various lever-arms in the requisite order. To effect this, a rectangular sheet of pasteboard is taken, somewhat larger in size than a section of the bundle of lever-arms. If this sheet be applied to the base of the bundle, and an advancing motion be then communicated to the pasteboard, this latter will move with it all the rods of the bundle, and consequently the threads that are connected with each of them. But if the pasteboard, instead of being plain, were pierced with holes corresponding to the extremities of the levers which meet it, then, since each of the levers would pass through the pasteboard during the motion of the latter, they would all remain in their places. We thus see that it is easy so to determine the position of the holes in the pasteboard, that, at any given moment, there shall be a certain number of levers, and consequently of parcels of threads, raised, while the rest remain where they were. Supposing this process is successively repeated according to a law indicated by the pattern to be executed, we perceive that this pattern may be reproduced on the stuff. For this purpose we need merely compose a series of cards according to the law required, and arrange them in suitable order one after the other; then, by causing them to pass over a polygonal beam which is so connected as to turn a new face for every stroke of the shuttle, which face shall then be impelled parallelly to itself against the bundle of lever-arms, the operation of raising the threads will be regularly performed. Thus we see that brocaded tissues may be manufactured with a precision and rapidity formerly difficult to obtain.

Arrangements analogous to those just described have been introduced into the Analytical Engine. It contains two principal species of cards: first, Operation cards, by means of which the parts of the machine are so disposed as to execute any determinate series of operations, such as additions, subtractions, multiplications, and divisions; secondly, cards of the Variables, which indicate to the machine the columns on which the results are to be represented. The cards, when put in motion, successively arrange the various portions of the machine according to the nature of the processes that are to be effected, and the machine at the same time executes these processes by means of the various pieces of mechanism of which it is constituted.

In order more perfectly to conceive the thing, let us select as an example the resolution of two equations of the first degree with two unknown quantities. Let the following be the two equations, in which x and y are the unknown quantities:—

$$\begin{cases} m x + n y = d \\ m' x + n' y = d'. \end{cases}$$

We deduce $x = \frac{d n' - d' n}{n' m - n m'}$, and for y an analogous expression. Let us continue to represent by V_0, V_1, V_2, &c. the different columns which contain the numbers, and let us suppose that the first eight columns have been chosen for expressing on them the numbers represented by m, n, d, m', n', d', n and n', which implies that $V_0 = m$, $V_1 = n$, $V_2 = d$, $V_3 = m'$, $V_4 = n'$, $V_5 = d'$, $V_6 = n$, $V_7 = n'$.

The series of operations commanded by the cards, and the results obtained, may be represented in the following table:—

Number of the operations.	Operation-cards.	Cards of the variables.		Progress of the operations.
	Symbols indicating the nature of the operations.	Columns on which operations are to be performed.	Columns which receive results of operations.	
1	$\times$	$V_2 \times V_4 =$	V_8	$= d n'$
2	$\times$	$V_5 \times V_1 =$	V_9	$= d' n$
3	$\times$	$V_4 \times V_0 =$	V_{10}	$= n' m$
4	$\times$	$V_1 \times V_3 =$	V_{11}	$= n m'$
5	$-$	$V_8 - V_9 =$	V_{12}	$= d n' - d' n$
6	$-$	$V_{10} - V_{11} =$	V_{13}	$= n' m - n m'$
7	$\div$	$\frac{V_{12}}{V_{13}} =$	V_{14}	$= x = \frac{d n' - d' n}{n' m - n m'}$

Since the cards do nothing but indicate in what manner and on what columns the machine shall act, it is clear that we must still, in every particular case, introduce the numerical data for the calculation. Thus, in the example we have selected, we must previously inscribe the numerical values of m, n, d, m', n', d', in the order and on the columns indicated, after which the machine when put in action will give the value of the unknown quantity x for this particular case. To obtain the value of y, another series of operations analogous to the preceding must be performed. But we see that they will be only four in number, since the denominator of the expression for y, excepting the sign, is the same as that for x, and equal to $n' m - n m'$. In the preceding table it will be remarked that the column for operations indicates four successive *multiplications*, two *subtractions*, and one *division*. Therefore, if desired, we need only use three operation cards; to manage which, it is sufficient to introduce into the machine an apparatus which shall, after the first multiplication, for instance, retain the card which relates to this operation, and not allow it to advance so as to be replaced by another one, until after this same operation shall have been four times repeated. In the preceding example we have seen, that to find the value of x we must begin by writing the coefficients m, n, d, m', n', d', upon eight columns, thus repeating n and n' twice. According to the same method, if it were required to calculate y likewise, these coefficients must be written on twelve different columns. But it is possible to simplify this process, and thus to diminish the chances of errors, which chances are greater, the larger the number of the quantities that have to be inscribed previous to setting the machine in action. To understand this simplification, we must remember that every number written on a column must, in order to be arithmetically combined with another number, be effaced from the column on which it is, and transferred to the *mill*. Thus, in the example we have discussed, we will take the two coefficients m and n', which are each of them to enter into *two* different products, that is m into $m n'$ and $m d'$, n' into $m n'$ and $n' d$. These coefficients will be inscribed on the columns V_0 and V_4. If we commence the series of operations by the product of m into n', these numbers will be effaced from the columns V_0 and V_4, that they may be transferred to the mill, which will multiply them into each other, and will then command the machine to represent the result, say on the column V_6. But as these numbers are each to be used again in another operation, they must again be inscribed somewhere; therefore, while the mill is working out their product, the machine will inscribe them anew on any two columns that may be indicated to it through the cards; and, as in the actual case, there is no reason why they should not resume their former places, we will suppose them again inscribed on V_0 and V_4, whence in short they would not finally disappear, to be reproduced no more, until they should have gone through all the combinations in which they might have to be used.

We see, then, that the whole assemblage of operations requisite for resolving the two* above equations of the first degree, may be definitively represented in the following table:—

* See Note D.

Columns on which are inscribed the primitive data.	Number of the operations.	Cards of the operations.		Variable cards.			Statement of results.
		Number of the Operation cards.	Nature of each operation.	Columns acted on by each operation.	Columns that receive the result of each operation.	Indication of change of value on any column.	
${}^1V_0 = m$	1	1	$\times$	${}^1V_0 \times {}^1V_4 =$	${}^1V_6$	$\left\{ \begin{array}{l} {}^1V_0 = {}^1V_0 \\ {}^1V_4 = {}^1V_4 \end{array} \right\}$	${}^1V_6 = m\,n'$
${}^1V_1 = n$	2	,,	$\times$	${}^1V_3 \times {}^1V_1 =$	${}^1V_7$	$\left\{ \begin{array}{l} {}^1V_3 = {}^1V_3 \\ {}^1V_1 = {}^1V_1 \end{array} \right\}$	${}^1V_7 = m'\,n$
${}^1V_2 = d$	3	,,	$\times$	${}^1V_2 \times {}^1V_4 =$	${}^1V_8$	$\left\{ \begin{array}{l} {}^1V_2 = {}^1V_2 \\ {}^1V_4 = {}^0V_4 \end{array} \right\}$	${}^1V_8 = d\,n'$
${}^1V_3 = m'$	4	,,	$\times$	${}^1V_5 \times {}^1V_1 =$	${}^1V_9$	$\left\{ \begin{array}{l} {}^1V_5 = {}^1V_5 \\ {}^1V_1 = {}^0V_1 \end{array} \right\}$	${}^1V_9 = d'\,n$
${}^1V_4 = n'$	5	,,	$\times$	${}^1V_0 \times {}^1V_5 =$	${}^1V_{10}$	$\left\{ \begin{array}{l} {}^1V_0 = {}^0V_0 \\ {}^1V_5 = {}^0V_5 \end{array} \right\}$	${}^1V_{10} = d'\,m$
${}^1V_5 = d'$	6	,,	$\times$	${}^1V_2 \times {}^1V_3 =$	${}^1V_{11}$	$\left\{ \begin{array}{l} {}^1V_2 = {}^0V_2 \\ {}^1V_3 = {}^0V_3 \end{array} \right\}$	${}^1V_{11} = d\,m'$
	7	2	$-$	${}^1V_6 - {}^1V_7 =$	${}^1V_{12}$	$\left\{ \begin{array}{l} {}^1V_6 = {}^0V_6 \\ {}^1V_7 = {}^0V_7 \end{array} \right\}$	${}^1V_{12} = m\,n' - m'\,n$
	8	,,	$-$	${}^1V_8 - {}^1V_9 =$	${}^1V_{13}$	$\left\{ \begin{array}{l} {}^1V_8 = {}^0V_8 \\ {}^1V_9 = {}^0V_9 \end{array} \right\}$	${}^1V_{13} = d\,n' - d'\,n$
	9	,,	$-$	${}^1V_{10} - {}^1V_{11} =$	${}^1V_{14}$	$\left\{ \begin{array}{l} {}^1V_{10} = {}^0V_{10} \\ {}^1V_{11} = {}^0V_{11} \end{array} \right\}$	${}^1V_{14} = d'\,m - d\,m'$
	10	3	$\div$	${}^1V_{13} \div {}^1V_{12} =$	${}^1V_{15}$	$\left\{ \begin{array}{l} {}^1V_{13} = {}^0V_{13} \\ {}^1V_{12} = {}^1V_{12} \end{array} \right\}$	${}^1V_{15} = \dfrac{d\,n' - d'\,n}{m\,n' - m'\,n} = x$
	11	,,	$\div$	${}^1V_{14} \div {}^1V_{12} =$	${}^1V_{16}$	$\left\{ \begin{array}{l} {}^1V_{14} = {}^0V_{14} \\ {}^1V_{12} = {}^0V_{12} \end{array} \right\}$	${}^1V_{16} = \dfrac{d'\,m - d\,m'}{m\,n' - m'\,n} = y$
1	2	3	4	5	6	7	8

In order to diminish to the utmost the chances of error in inscribing the numerical data of the problem, they are successively placed on one of the columns of the mill; then, by means of cards arranged for this purpose, these same numbers are caused to arrange themselves on the requisite columns, without the operator having to give his attention to it; so that his undivided mind may be applied to the simple inscription of these same numbers.

According to what has now been explained, we see that the collection of columns of Variables may be regarded as a *store* of numbers, accumulated there by the mill, and which, obeying the orders transmitted to the machine by means of the cards, pass alternately from the mill to the store, and from the store to the mill, that they may undergo the transformations demanded by the nature of the calculation to be performed.

Hitherto no mention has been made of the *signs* in the results, and the machine would be far from perfect were it incapable of expressing and combining amongst each other positive and negative quantities. To accomplish this end, there is, above every column, both of the mill and of the store, a disc, similar to the discs of which the columns themselves consist. According as the digit on this disc is even or uneven, the number inscribed on the corresponding column below it will be considered as positive or negative. This granted, we may, in the following manner, conceive how the signs can be algebraically combined in the machine. When a number is to be transferred from the store to the mill, and *vice versâ*, it will always be transferred with its sign, which will be effected by means of the cards, as has been explained in what precedes. Let any two numbers then, on which we are to operate arithmetically, be placed in the mill with their respective signs. Suppose that we are first to add them together; the operation-cards will command the addition: if the two numbers be of the same sign, one of the two will be entirely effaced from where it was inscribed, and will go to add itself on the column which contains the other number; the machine will, during this operation, be able, by means of a certain apparatus, to prevent any movement in the disc of signs which belongs to the column on which the addition is made, and thus the result will remain with the sign which the two given numbers originally had. When two numbers have two different signs, the addition commanded by the card will be changed into a subtraction through the intervention of mechanisms which are brought into play by this very difference of sign. Since the subtraction can only be effected on the larger of the two numbers, it must be arranged that the disc of signs of the larger number shall not move while the smaller of the two numbers is being effaced from its column and subtracted from the other, whence the result will have the sign of this latter, just as in fact it ought to be. The combinations to which algebraical subtraction give rise, are analogous to the preceding. Let us pass on to multiplication. When two numbers to be multiplied are of the same sign, the result is positive; if the signs are different, the product must be negative. In order that the machine may act conformably to this law, we have but to conceive that on the column containing the product of the two given numbers, the digit which indicates the sign of that product, has been formed by the mutual addition of the two digits that respectively indicated the signs of the two given numbers; it is then obvious that if the digits of the signs are both even, or both odd, their sum will be an even number, and consequently will express a positive number; but that if, on the contrary, the two digits of the signs are one even and the other odd, their sum will be an odd number, and will consequently express a negative number. In the case of division, instead of adding the digits of the discs, they must be subtracted one from the other, which will produce results analogous to the preceding; that is to say, that if these figures are both even or both uneven, the remainder of this subtraction will be even; and it will be uneven in the contrary case. When I speak of mutually adding or subtracting the numbers expressed by the digits of the signs, I merely mean that one of the sign-discs is made to advance or retrograde a number of divisions equal to that which is expressed by the digit on the other sign-disc. We see, then, from the preceding explanation, that it is possible mechanically to combine the signs of quantities so as to obtain results conformable to those indicated by algebra*.

The machine is not only capable of executing those numerical calculations which depend on a given algebraical formula, but it is also fitted for analytical calculations in which there are one or several variables to be considered. It must be assumed that the analytical expression to be operated on can be developed according to powers of the variable, or according to determinate functions of this same variable, such as circular functions, for instance; and similarly for the result that is to be attained. If we then suppose that above the columns of the store, we have inscribed the powers or the functions of the variable, arranged according to whatever is the prescribed law of development, the coefficients of these several terms may be respectively placed on the corresponding column below each. In this manner we shall have a representation of an analytical development; and, supposing the position of the several terms composing it to be invariable, the problem will be reduced to that of calculating their coefficients according to the laws demanded by the nature of the question. In order to make this more clear, we shall take the

* Not having had leisure to discuss with Mr. Babbage the manner of introducing into his machine the combination of algebraical signs, I do not pretend here to expose the method he uses for this purpose; but I considered that I ought myself to supply the deficiency, conceiving that this paper would have been imperfect if I had omitted to point out one means that might be employed for resolving this essential part of the problem in question.

following* very simple example, in which we are to multiply $(a + b\,x^1)$ by $(A + B\cos^1 x)$. We shall begin by writing x^0, x^1, $\cos^0 x$, $\cos^1 x$, above the columns V_0, V_1, V_2, V_3; then, since from the form of the two functions to be combined, the terms which are to compose the products will be of the following nature, $x^0 . \cos^0 x$, $x^0 . \cos^1 x$, $x^1 . \cos^0 x$, $x^1 . \cos^1 x$; these will be inscribed above the columns V_4, V_5, V_6, V_7. The coefficients of x^0, x^1, $\cos^0 x$, $\cos^1 x$ being given, they will, by means of the mill, be passed to the columns V_0, V_1, V_2 and V_3. Such are the primitive data of the problem. It is now the business of the machine to work out its solution, that is to find the coefficients which are to be inscribed on V_4, V_5, V_6, V_7. To attain this object, the law of formation of these same coefficients being known, the machine will act through the intervention of the cards, in the manner indicated by the following table:—

† Columns above which are written the functions of the variable.	Coefficients.		Cards of the operations.		Cards of the variables.			
	Given.	To be formed.	Number of the operations.	Nature of the operation.	Columns on which operations are to be performed.	Columns on which are to be inscribed the results of the operations.	Indication of change of value on any column submitted to an operation.	Results of the operations.
x^0 .... $^1V_0$	a	”	”	”	”	”	”	” ”
x^1 .... $^1V_1$	b	”	”	”	”	”	”	” ”
$\mathrm{Cos}^0 x$ .... $^1V_2$	A	”	”	”	”	”	”	” ”
$\mathrm{Cos}^1 x$ .... $^1V_3$	B	”	”	”	”	”	”	” ”
$x^0 \cos^0 x$ .. $^0V_4$	...	a A	1	$\times$	$^1V_0 \times {}^1V_2 =$	$^1V_4$	$\left\{\begin{matrix} ^1V_0 = {}^1V_0 \\ ^1V_2 = {}^1V_2 \end{matrix}\right\}$	$^1V_4 = a\,A$ coefficients of $x^0 \cos^0 x$
$x^0 \cos^1 x$ .. $^0V_5$	...	a B	2	$\times$	$^1V_0 \times {}^1V_3 =$	$^1V_5$	$\left\{\begin{matrix} ^1V_0 = {}^0V_0 \\ ^1V_3 = {}^1V_3 \end{matrix}\right\}$	$^1V_5 = a\,B$ ... ... $x^0 \cos^1 x$
$x^1 \cos^0 x$ .. $^0V_6$	...	b A	3	$\times$	$^1V_1 \times {}^1V_2 =$	$^1V_6$	$\left\{\begin{matrix} ^1V_1 = {}^1V_1 \\ ^1V_2 = {}^0V_2 \end{matrix}\right\}$	$^1V_6 = b\,A$ ... ... $x^1 \cos^0 x$
$x^1 \cos^1 x$ .. $^0V_7$	...	b B	4	$\times$	$^1V_1 \times {}^1V_3 =$	$^1V_7$	$\left\{\begin{matrix} ^1V_1 = {}^0V_1 \\ ^1V_3 = {}^0V_3 \end{matrix}\right\}$	$^1V_7 = b\,B$ ... ... $x^1 \cos^1 x$

It will now be perceived that a general application may be made of the principle developed in the preceding example, to every species of process which it may be proposed to effect on series submitted to calculation. It is sufficient that the law of formation of the coefficients be known, and that this law be inscribed on the cards of the machine, which will then of itself execute all

* See Note E.

† For an explanation of the upper left-hand indices attached to the V's in this and in the preceding Table, we must refer the reader to Note D, amongst those appended to the memoir.—NOTE BY TRANSLATOR.

the calculations requisite for arriving at the proposed result. If, for instance, a recurring series were proposed, the law of formation of the coefficients being here uniform, the same operations which must be performed for one of them will be repeated for all the others; there will merely be a change in the locality of the operation, that is it will be performed with different columns. Generally, since every analytical expression is susceptible of being expressed in a series ordered according to certain functions of the variable, we perceive that the machine will include all analytical calculations which can be definitively reduced to the formation of coefficients according to certain laws, and to the distribution of these with respect to the variables.

We may deduce the following important consequence from these explanations, viz. that since the cards only indicate the nature of the operations to be performed, and the columns of Variables with which they are to be executed, these cards will themselves possess all the generality of analysis, of which they are in fact merely a translation. We shall now further examine some of the difficulties which the machine must surmount, if its assimilation to analysis is to be complete. There are certain functions which necessarily change in nature when they pass through zero or infinity, or whose values cannot be admitted when they pass these limits. When such cases present themselves, the machine is able, by means of a bell, to give notice that the passage through zero or infinity is taking place, and it then stops until the attendant has again set it in action for whatever process it may next be desired that it shall perform. If this process has been foreseen, then the machine, instead of ringing, will so dispose itself as to present the new cards which have relation to the operation that is to succeed the passage through zero and infinity. These new cards may follow the first, but may only come into play contingently upon one or other of the two circumstances just mentioned taking place.

Let us consider a term of the form $a\,b^n$; since the cards are but a translation of the analytical formula, their number in this particular case must be the same, whatever be the value of n; that is to say, whatever be the number of multiplications required for elevating b to the nth power (we are supposing for the moment that n is a whole number). Now, since the exponent n indicates that b is to be multiplied n times by itself, and all these operations are of the same nature, it will be sufficient to employ one single operation-card, viz. that which orders the multiplication.

But when n is given for the particular case to be calculated, it will be further requisite that the machine limit the number of its multiplications according to the given values. The process may be thus arranged. The three numbers a, b and n will be written on as many distinct columns of the store; we shall designate them V_0, V_1, V_2; the result $a\,b^n$ will place itself on the column V_3. When the number n has been introduced into the machine, a card will order a certain registering-apparatus to mark $(n-1)$, and will at the same time execute the multiplication of b by b. When this is completed, it will be found that the registering-apparatus has effaced a unit, and that it only marks $(n-2)$; while the machine will now again order the number b written on the column V_1 to multiply itself with the product b^2 written on the column V_3, which will give b^3. Another unit is then effaced from the registering-apparatus, and the same processes are continually repeated until it only marks zero. Thus the number b^n will be found inscribed on V_3, when the machine, pursuing its course of operations, will order the product of b^n by a; and the required calculation will have been completed without there being any necessity that the number of operation-cards used should vary with the value of n. If n were negative, the cards, instead of ordering the multiplication of a by b^n, would order its division; this we can easily conceive, since every number, being inscribed with its respective sign, is consequently capable of reacting on the nature of the operations to be executed. Finally, if n were fractional, of the form $\frac{p}{q}$, an additional column would be used for the inscription of q, and the machine would bring into action two sets of processes, one for raising b to the power p, the other for extracting the qth root of the number so obtained.

Again, it may be required, for example, to multiply an expression of the form $a\,x^m + b\,x^n$ by another $A\,x^p + B\,x^q$, and then to reduce the product to the least number of terms, if any of the indices are equal. The two factors being ordered with respect to x, the general result of the multiplication would be $A\,a\,x^{m+p} + A\,b\,x^{n+p} + B\,a\,x^{m+q} + B\,b\,x^{n+q}$. Up to this point the process presents no difficulties; but suppose that we have $m=p$ and $n=q$, and that we wish to reduce the two middle terms to a single one $(A\,b + B\,a)\,x^{m+q}$. · For this purpose, the cards may order $m + q$ and $n + p$ to be transferred into the mill, and there subtracted one from the other; if the remainder is nothing, as would be the case on the present hypothesis, the mill will order other cards to bring to it the coefficients $A\,b$ and $B\,a$, that it may add them together and give them in this state as a coefficient for the single term $x^{n+p} = x^{m+q}$.

This example illustrates how the cards are able to reproduce all the operations which intellect performs in order to attain a determinate result, if these operations are themselves capable of being precisely defined.

Let us now examine the following expression:—

$$2 \cdot \frac{2^2 . 4^2 . 6^2 . 8^2 . 10^2 \ldots\ldots (2\,n)^2}{1^2 . 3^2 . 5^2 . 7^2 . 9^2 \ldots\ldots (2\,n - 1)^2 . (2\,n + 1)^2},$$

which we know becomes equal to the ratio of the circumference to the diameter, when n is infinite. We may require the machine not only to perform the calculaiton of this fractional expression, but further to give indication as soon as the value becomes identical with that of the ratio of the circumference to the diameter when n is infinite, a case in which the computation would be impossible. Observe that we should thus require of the machine to interpret a result not of itself evident, and that this is not amongst its attributes, since it is no thinking being. Nevertheless, when the cos of $n = \infty$ has been foreseen, a card may immediately order the substitution of the value of π (π being the ratio of the circumference to the diameter), without going through the series of calculations indicated. This would merely require that the machine contain a special card, whose office it should be to place the number π in a direct and independent manner on the column indicated to it. And here we should introduce the mention of a third species of cards, which may be called *cards of numbers*. There are certain numbers, such as those expressing the ratio of the circumference to the diameter, the Numbers of Bernoulli, &c., which frequently present themselves in calculations. To avoid the necessity for computing them every time they have to be used, certain cards may be combined specially in order to give these numbers ready made into the mill, whence they afterwards go and place themselves on those columns of the store that are destined for them. Through this means the machine will be susceptible of those simplifications afforded by the use of numerical tables. It would be equally possible to introduce, by means of these cards, the logarithms of numbers; but perhaps it might not be in this case either the shortest or the most appropriate method; for the machine might be able to perform the same calculations by other more expeditious combinations, founded on the rapidity with which it executes the four first operations of arithmetic. To give an idea of this rapidity, we need only mention that Mr. Babbage believes he can, by his engine, form the product of two numbers, each containing twenty figures, in *three minutes*.

Perhaps the immense number of cards required for the solution of any rather complicated problem may appear to be an obstacle; but this does not seem to be the case. There is no limit to the number of cards that can be used. Certain stuffs require for their fabrication not less than *twenty thousand* cards, and we may unquestionably far exceed even this quantity *.

Resuming what we have explained concerning the Analytical Engine, we may conclude that it is based on two principles: the first, consisting in the fact that every arithmetical calculation ultimately depends on four principal operations—addition, subtraction, multiplication, and division; the second, in the possibility of reducing every analytical calculation to that of the coefficients for the several terms of a series. If this last principle be true, all the operations of analysis come within the domain of the engine. To take another point of view: the use of the cards offers a generality equal to that of algebraical formulæ, since such a formula simply indicates the nature and order of the operations requisite for arriving at a certain definite result, and similarly the cards merely command the engine to perform these same operations; but in order that the mechanisms may be able to act to any purpose, the numerical data of the problem must in every particular case be introduced. Thus the same series of cards will serve for all questions whose sameness of nature is such as to require nothing altered excepting the numerical data. In this light the cards are merely a translation of algebraical formulæ, or, to express it better, another form of analytical notation.

Since the engine has a mode of acting peculiar to itself, it will in every particular case be necessary to arrange the series of calculations conformably to the means which the machine pos-

* See Note F.

sesses; for such or such a process which might be very easy for a calculator, may be long and complicated for the engine, and *vice versâ*.

Considered under the most general point of view, the essential object of the machine being to calculate, according to the laws dictated to it, the values of numerical coefficients which it is then to distribute appropriately on the columns which represent the variables, it follows that the interpretation of formulæ and of results is beyond its province, unless indeed this very interpretation be itself susceptible of expression by means of the symbols which the machine employs. Thus, although it is not itself the being that reflects, it may yet be considered as the being which executes the conceptions of intelligence*. The cards receive the impress of these conceptions, and transmit to the various trains of mechanism composing the engine the orders necessary for their action. When once the engine shall have been constructed, the difficulty will be reduced to the making out of the cards; but as these are merely the translation of algebraical formulæ, it will, by means of some simple notations, be easy to consign the execution of them to a workman. Thus the whole intellectual labour will be limited to the preparation of the formulæ, which must be adapted for calculation by the engine.

Now, admitting that such an engine can be constructed, it may be inquired: what will be its utility? To recapitulate; it will afford the following advantages:—First, rigid accuracy. We know that numerical calculations are generally the stumbling-block to the solution of problems, since errors easily creep into them, and it is by no means always easy to detect these errors. Now the engine, by the very nature of its mode of acting, which requires no human intervention during the course of its operations, presents every species of security under the head of correctness; besides, it carries with it its own check; for at the end of every operation it prints off, not only the results, but likewise the numerical data of the question; so that it is easy to verify whether the question has been correctly proposed. Secondly, economy of time: to convince ourselves of this, we need only recollect that the multiplication of two numbers, consisting each of twenty figures, requires at the very utmost three minutes. Likewise, when a long series of identical computations is to be performed, such as those required for the formation of

* See Note G.

numerical tables, the machine can be brought into play so as to give several results at the same time, which will greatly abridge the whole amount of the processes. Thirdly, economy of intelligence: a simple arithmetical computation requires to be performed by a person possessing some capacity; and when we pass to more complicated calculations, and wish to use algebraical formulæ in particular cases, knowledge must be possessed which pre-supposes preliminary mathematical studies of some extent. Now the engine, from its capability of performing by itself all these purely material operations, spares intellectual labour, which may be more profitably employed. Thus the engine may be considered as a real manufactory of figures, which will lend its aid to those many useful sciences and arts that depend on numbers. Again, who can foresee the consequences of such an invention? In truth, how many precious observations remain practically barren for the progress of the sciences, because there are not powers sufficient for computing the results! And what discouragement does the perspective of a long and arid computation cast into the mind of a man of genius, who demands time exclusively for meditation, and who beholds it snatched from him by the material routine of operations! Yet it is by the laborious route of analysis that he must reach truth; but he cannot pursue this unless guided by numbers; for without numbers it is not given us to raise the veil which envelopes the mysteries of nature. Thus the idea of constructing an apparatus capable of aiding human weakness in such researches, is a conception which, being realized, would mark a glorious epoch in the history of the sciences. The plans have been arranged for all the various parts, and for all the wheel-work, which compose this immense apparatus, and their action studied; but these have not yet been fully combined together in the drawings* and mechanical notation †. The confidence which the genius of Mr. Babbage must inspire, affords legitimate ground for hope that this enterprise will be crowned with success; and while we render homage to the intelligence which directs it, let us breathe aspirations for the accomplishment of such an undertaking.

* This sentence has been slightly altered in the translation in order to express more exactly the present state of the engine.—NOTE BY TRANSLATOR.

† The notation here alluded to is a most interesting and important subject, and would have well deserved a separate and detailed Note upon it, amongst those appended to the Memoir. It has, however, been impossible, within the space allotted, even to touch upon so wide a field.—NOTE BY TRANSLATOR.

NOTES BY THE TRANSLATOR.

NOTE A.—Page 674.

The particular function whose integral the Difference Engine was constructed to tabulate, is

$$\Delta^7 u_x = 0.$$

The purpose which that engine has been specially intended and adapted to fulfil, is the computation of nautical and astronomical tables. The integral of

$$\Delta^7 u_x = 0$$

being $$u_x = a + bx + cx^2 + dx^3 + ex^4 + fx^5 + gx^6,$$

the constants a, b, c, &c. are represented on the seven columns of discs, of which the engine consists. It can therefore tabulate *accurately* and to an *unlimited extent*, all series whose general term is comprised in the above formula; and it can also tabulate *approximatively* between *intervals of greater or less extent*, all other series which are capable of tabulation by the Method of Differences.

The Analytical Engine, on the contrary, is not merely adapted for *tabulating* the results of one particular function and of no other, but for *developing and tabulating* any function whatever. In fact the engine may be described as being the material expression of any indefinite function of any degree of generality and complexity, such as for instance,

$$F(x, y, z, \log x, \sin y, x^p, \&c.),$$

which is, it will be observed, a function of all other possible functions of any number of quantities.

In this, which we may call the *neutral* or *zero* state of the engine, it is ready to receive at any moment, by means of cards constituting a portion of its mechanism (and applied on the principle of those used in the Jacquard-loom), the impress of whatever *special* function we may desire to develope or to tabulate. These cards contain within themselves (in a manner explained in the Memoir itself, pages 677 and 678) the law of development of the particular function that may be under consideration, and they compel the mechanism to act accordingly in a certain corresponding order. One of the simplest cases would be, for example, to suppose that

$$F(x, y, z, \&c. \&c.)$$

is the particular function

$$\Delta^n u_x = 0$$

which the Difference Engine tabulates for values of n only up to 7. In this case the cards would order the mechanism to go through that succession of operations which would tabulate

$$u_x = a + bx + cx^2 + \ldots . m x^{n-1},$$

where n might be any number whatever.

These cards, however, have nothing to do with the regulation of the particular *numerical* data. They merely determine the *operations** to be effected, which operations may of course be performed on an infinite variety of particular numerical values, and do not bring out any definite numerical results unless the numerical data of the problem have been impressed on the requisite portions of the train of mechanism. In the above example, the first essential step towards an arithmetical result, would be the substitution of specific numbers for n, and for the other primitive quantities which enter into the function.

Again, let us suppose that for F we put two complete equations of the fourth degree between x and y. We must then express on the cards the law of elimination for such equations. The engine would follow out those laws, and would ultimately give the equation of one variable which results from such elimination. Various *modes* of elimination might be selected; and of course the cards must be made out accordingly. The following is one mode that might be adopted. The engine is able to multiply together any two functions of the form

$$a + bx + cx^2 + \ldots . px^n.$$

This granted, the two equations may be arranged according to the powers of y, and the coefficients of the powers of y may be arranged according to powers of x. The elimination of y will result from the successive multiplications and subtractions of several such functions. In this, and in all other instances, as was explained above, the particular *numerical* data and the *numerical* results are determined by means and by portions of the mechanism which act quite independently of those that regulate the *operations*.

In studying the action of the Analytical Engine, we find that the peculiar and independent nature of the considerations which in all mathematical analysis belong to *operations*, as distinguished from *the objects operated upon* and from the *results* of the operations performed upon those objects, is very strikingly defined and separated.

It is well to draw attention to this point, not only because its full appreciation is essential to the attainment of any very just and adequate general comprehension of the powers and mode of action of the Analytical Engine, but also because it is one which is perhaps too little kept in view in the study of mathematical science in general. It is, however, impossible to confound it with other considerations, either when we trace the manner in which that engine attains its results, or when we prepare the data for its attainment of those results. It were much to be desired, that when mathematical processes pass through the human brain instead of through the medium of inanimate mechanism, it were equally a necessity of things that the reasonings connected with *operations* should hold the same just place as a clear and well-defined branch of the subject of analysis, a fundamental but yet independent

* We do not mean to imply that the *only* use made of the Jacquard cards is that of regulating the algebraical *operations*. But we mean to explain that *those* cards and portions of mechanism which regulate these *operations*, are wholly independent of those which are used for other purposes. M. Menabrea explains that there are *three* classes of cards used in the engine for three distinct sets of objects, viz. *Cards of the Operations*, *Cards of the Variables*, and certain *Cards of Numbers*. (See pages 678 and 687.)

ingredient in the science, which they must do in studying the engine. The confusion, the difficulties, the contradictions which, in consequence of a want of accurate distinctions in this particular, have up to even a recent period encumbered mathematics in all those branches involving the consideration of negative and impossible quantities, will at once occur to the reader who is at all versed in this science, and would alone suffice to justify dwelling somewhat on the point, in connexion with any subject so peculiarly fitted to give forcible illustration of it, as the Analytical Engine. It may be desirable to explain, that by the word *operation*, we mean *any process which alters the mutual relation of two or more things*, be this relation of what kind it may. This is the most general definition, and would include all subjects in the universe. In abstract mathematics, of course operations alter those particular relations which are involved in the considerations of number and space, and the *results* of operations are those peculiar results which correspond to the nature of the subjects of operation. But the science of operations, as derived from mathematics more especially, is a science of itself, and has its own abstract truth and value; just as logic has its own peculiar truth and value, independently of the subjects to which we may apply its reasonings and processes. Those who are accustomed to some of the more modern views of the above subject, will know that a few fundamental relations being true, certain other combinations of relations must of necessity follow; combinations unlimited in variety and extent if the deductions from the primary relations be carried on far enough. They will also be aware that one main reason why the separate nature of the science of operations has been little felt, and in general little dwelt on, is the *shifting* meaning of many of the symbols used in mathematical notation. First, the symbols of *operation* are frequently *also* the symbols of the *results* of operations. We may say that these symbols are apt to have both a *retrospective* and a *prospective* signification. They may signify either relations that are the consequence of a series of processes already performed, or relations that are yet to be effected through certain processes. Secondly, figures, the symbols of *numerical magnitude*, are frequently *also* the symbols of *operations*, as when they are the indices of powers. Wherever terms have a shifting meaning, independent sets of considerations are liable to become complicated together, and reasonings and results are frequently falsified. Now in the Analytical Engine the operations which come under the first of the above heads, are ordered and combined by means of a notation and of a train of mechanism which belong exclusively to themselves; and with respect to the second head, whenever numbers meaning *operations* and not *quantities* (such as the indices of powers), are inscribed on any column or set of columns, those columns immediately act in a wholly separate and independent manner, becoming connected with the *operating mechanism* exclusively, and re-acting upon this. They never come into combination with numbers upon any other columns meaning *quantities*; though, of course, if there are numbers meaning *operations* upon n columns, these may *combine amongst each other*, and will often be required to do so, just as numbers meaning *quantities* combine with each other in any variety. It might have been arranged that all numbers meaning *operations* should have appeared on some separate portion of the engine from that which presents numerical *quantities*; but the present mode is in some cases more simple, and offers in reality quite as much distinctness when understood.

The operating mechanism can even be thrown into action independently of any object to operate upon (although of course no *result* could then be developed). Again, it might act upon other things besides *number*, were objects found whose mutual fundamental relations could be expressed by those of the abstract science of operations, and which should be also susceptible of adaptations to the action of the operating notation and mechanism of the engine. Supposing, for instance, that the fundamental relations of pitched sounds in the science of harmony and of musical composition were susceptible of such expression and adaptations, the engine might compose elaborate and scientific pieces of music of any degree of complexity or extent.

The Analytical Engine is an *embodying of the science of operations*, constructed with peculiar reference to abstract number as the subject of those operations. The Difference Engine is the embodying of *one particular and very limited set of operations*, which (see the notation used in note B) may be expressed thus, $(+, +, +, +, +, +)$, or thus, $6(+)$. Six repetitions of the one operation, $+$, is, in fact, the whole sum and object of that engine. It has seven columns, and a number on any column can add itself to a number on the next column to its *right-hand*. So that, beginning with the column furthest to the left, six additions can be effected, and the result appears on the seventh column, which is the last on the right-hand. The *operating* mechanism of this engine acts in as separate and independent a manner as that of the Analytical Engine; but being susceptible of only one unvarying and restricted combination, it has little force or interest in illustration of the distinct nature of the *science of operations*. The importance of regarding the Analytical Engine under this point of view will, we think, become more and more obvious, as the reader proceeds with M. Menabrea's clear and masterly article. The calculus of operations is likewise in itself a topic of so much interest, and has of late years been so much more written on and thought on than formerly, that any bearing which that engine, from its mode of constitution, may possess upon the illustration of this branch of mathematical science, should not be overlooked. Whether the inventor of this engine had any such views in his mind while working out the invention, or whether he may subsequently ever have regarded it under this phase, we do not know; but it is one that forcibly occurred to ourselves on becoming acquainted with the means through which analytical combinations are actually attained by the mechanism. We cannot forbear suggesting one practical result which it appears to us must be greatly facilitated by the independent manner in which the engine orders and combines its *operations*: we allude to the attainment of those combinations into which *imaginary quantities* enter. This is a branch of its processes into which we have not had the opportunity of inquiring, and our conjecture therefore as to the principle on which we conceive the accomplishment of such results may have been made to depend, is very probably not in accordance with the fact, and less subservient for the purpose than some other principles, or at least requiring the cooperation of others. It seems to us obvious, however, that where operations are so independent in their mode of acting, it must be easy by means of a few simple provisions and additions in arranging the mechanism, to bring out a *double* set of *results*, viz.—1st, the *numerical magnitudes* which are the results of operations performed on *numerical data*. (These results are the *primary* object of the engine). 2ndly, the *symbolical results* to be attached to those numerical results, which symbolical results are not less the necessary and logical consequences of operations performed upon *symbolical data*, than are numerical results when the data are numerical*.

If we compare together the powers and the principles of construction of the Difference and of the Analytical Engines, we shall perceive that the capabilities of the latter are immeasurably more extensive than those of the former, and that they in fact hold to each other the same relationship as that of analysis to arithmetic. The Difference Engine can effect but one particular series of operations, viz. that required for tabulating the integral of the special function

$$\Delta^n u_x = 0;$$

and as it can only do this for values of n up to 7†, it cannot be considered as being the most *general* expression even of *one particular* function, much less as being the expression of any and all possible functions of all degrees of generality. The Difference Engine can in reality (as has been already partly explained) do nothing but *add*; and any other processes, not excepting those of simple subtraction, multiplication and division, can be performed by it only just to that extent in which it is possible, by judicious mathematical arrangement and artifices, to reduce them to a *series of additions*. The method of differences is, in fact, a method of additions; and as it includes within its means a larger number of results attainable by *addition* simply, than any other mathematical principle, it was very appropriately selected as the basis on which to construct *an Adding Machine*, so as to give to the powers of such a machine the widest possible range. The Analytical Engine, on the contrary, can either add, subtract, multiply or divide with equal facility; and performs each of these four operations in a direct manner, without the aid of any of the other three. This one fact implies everything; and it is scarcely necessary to point out, for instance, that while the Difference Engine can merely *tabulate*,

* In fact such an extension as we allude to, would merely constitute a further and more perfected development of any system introduced for making the proper combinations of the signs *plus* and *minus*. How ably M. Menabrea has touched on this restricted case is pointed out in Note B.

† The machine might have been constructed so as to tabulate for a higher value of n than seven. Since, however, every unit added to the value of n increases the extent of the mechanism requisite, there would on this account be a limit beyond which it could not be practically carried. Seven is sufficiently high for the calculation of all ordinary tables.

The fact that, in the Analytical Engine, the same extent of mechanism suffices for the solution of $\Delta^n u_x = 0$, whether $n = 7$, $n = 100{,}000$, or $n =$ any number whatever, at once suggests how entirely distinct must be the *nature of the principles* through whose application matter has been enabled to become the working agent of abstract mental operations in each of these engines respectively; and it affords an equally obvious presumption, that in the case of the Analytical Engine, not only are those principles in themselves of a higher and more comprehensive description, but also such as must vastly extend the *practical* value of the engine whose basis they constitute.

and is incapable of *developing*, the Analytical Engine can *either tabulate or develope*.

The former engine is in its nature strictly *arithmetical*, and the results it can arrive at lie within a very clearly defined and restricted range, while there is no finite line of demarcation which limits the powers of the Analytical Engine. These powers are co-extensive with our knowledge of the laws of analysis itself, and need be bounded only by our acquaintance with the latter. Indeed we may consider the engine as the *material and mechanical representative* of analysis, and that our actual working powers in this department of human study will be enabled more effectually than heretofore to keep pace with our theoretical knowledge of its principles and laws, through the complete control which the engine gives us over the *executive manipulation* of algebraical and numerical symbols.

Those who view mathematical science not merely as a vast body of abstract and immutable truths, whose intrinsic beauty, symmetry and logical completeness, when regarded in their connexion together as a whole, entitle them to a prominent place in the interest of all profound and logical minds, but as possessing a yet deeper interest for the human race, when it is remembered that this science constitutes the language through which alone we can adequately express the great facts of the natural world, and those unceasing changes of mutual relationship which, visibly or invisibly, consciously or unconsciously to our immediate physical perceptions, are interminably going on in the agencies of the creation we live amidst: those who thus think on mathematical truth as the instrument through which the weak mind of man can most effectually read his Creator's works, will regard with especial interest all that can tend to facilitate the translation of its principles into explicit practical forms.

The distinctive characteristic of the Analytical Engine, and that which has rendered it possible to endow mechanism with such extensive faculties as bid fair to make this engine the executive right-hand of abstract algebra, is the introduction into it of the principle which Jacquard devised for regulating, by means of punched cards, the most complicated patterns in the fabrication of brocaded stuffs. It is in this that the distinction between the two engines lies. Nothing of the sort exists in the Difference Engine. We may say most aptly that the Analytical Engine *weaves algebraical patterns* just as the Jacquard-loom weaves flowers and leaves. Here, it seems to us, resides much more of originality than the Difference Engine can be fairly entitled to claim. We do not wish to deny to this latter all such claims. We believe that it is the only proposal or attempt ever made to construct a calculating machine *founded on the principle of successive orders of differences*, and capable of *printing off its own results*; and that this engine surpasses its predecessors, both in the extent of the calculations which it can perform, in the facility, certainty and accuracy with which it can effect them, and in the absence of all necessity for the intervention of human intelligence *during the performance of its calculations*. Its nature is, however, limited to the strictly arithmetical, and it is far from being the first or only scheme for constructing *arithmetical* calculating machines with more or less of success.

The bounds of *arithmetic* were however outstepped the moment the idea of applying the cards had occurred; and the Analytical Engine does not occupy common ground with mere "calculating machines." It holds a position wholly its own; and the considerations it suggests are most interesting in their nature. In enabling mechanism to combine together *general* symbols, in successions of unlimited variety and extent, a uniting link is established between the operations of matter and the abstract mental processes of the *most abstract* branch of mathematical science. A new, a vast, and a powerful language is developed for the future use of analysis, in which to wield its truths so that these may become of more speedy and accurate practical application for the purposes of mankind than the means hitherto in our possession have rendered possible. Thus not only the mental and the material, but the theoretical and the practical in the mathematical world, are brought into more intimate and effective connexion with each other. We are not aware of its being on record that anything partaking in the nature of what is so well designated the *Analytical* Engine has been hitherto proposed, or even thought of, as a practical possibility, any more than the idea of a thinking or of a reasoning machine.

We will touch on another point which constitutes an important distinction in the modes of operating of the Difference and Analytical Engines. In order to enable the former to do its business, it is necessary to put into its columns the series of numbers constituting the first terms of the several orders of differences for whatever is the particular table under consideration. The machine then works *upon* these as its data. But these data must themselves have been already computed through a series of calculations by a human head. Therefore that engine can only produce results depending on data which have been arrived at by the explicit and actual working out of processes that are in their nature different from any that come within the sphere of its own powers. In other words, an *analysing* process must have been gone through by a human mind in order to obtain the data upon which the engine then *synthetically* builds its results. The Difference Engine is in its character exclusively *synthetical*, while the Analytical Engine is equally capable of analysis or of synthesis.

It is true that the Difference Engine can calculate to a much greater extent with these few preliminary data, than the data themselves required for their own determination. The table of squares, for instance, can be calculated to any extent whatever, when the numbers *one* and *two* are furnished; and a very few differences computed at any part of a table of logarithms would enable the engine to calculate many hundreds or even thousands of logarithms. Still the circumstance of its requiring, as a previous condition, that any function whatever shall have been numerically worked out, makes it very inferior in its nature and advantages to an engine which, like the Analytical Engine, requires merely that we should know the *succession and distribution of the operations* to be performed; without there being any occasion*, in order to obtain data on which it can work, for our ever having gone through either the same particular operations which it is itself to effect, or any others. Numerical data must of course be given it, but they are mere arbitrary ones; not data that could only be arrived at through a systematic and necessary series of previous numerical calculations, which is quite a different thing.

* This subject is further noticed in Note F.

To this it may be replied that an analysing process must equally have been performed in order to furnish the Analytical Engine with the necessary *operative* data; and that herein may also lie a possible source of error. Granted that the actual mechanism is unerring in its processes, the *cards* may give it wrong orders. This is unquestionably the case; but there is much less chance of error, and likewise far less expenditure of time and labour, where operations only, and the distribution of these operations, have to be made out, than where explicit numerical results are to be attained. In the case of the Analytical Engine we have undoubtedly to lay out a certain capital of analytical labour in one particular line; but this is in order that the engine may bring us in a much larger return in another line. It should be remembered also that the cards when once made out for any formula, have all the generality of algebra, and include an infinite number of particular cases.

We have dwelt considerably on the distinctive peculiarities of each of these engines, because we think it essential to place their respective attributes in strong relief before the apprehension of the public; and to define with clearness and accuracy the wholly different nature of the principles on which each is based, so as to make it self-evident to the reader (the mathematical reader at least) in what manner and degree the powers of the Analytical Engine transcend those of an engine, which, like the Difference Engine, can only work out such results as may be derived from *one restricted and particular series of processes*, such as those included in $\Delta^n u_z = 0$. We think this of importance, because we know that there exists considerable vagueness and inaccuracy in the mind of persons in general on the subject. There is a misty notion amongst most of those who have attended at all to it, that *two* "calculating machines" have been successively invented by the same person within the last few years; while others again have never heard but of the one original "calculating machine," and are not aware of there being any extension upon this. For either of these two classes of persons the above considerations are appropriate. While the latter require a knowledge of the fact that there *are two* such inventions, the former are not less in want of accurate and well-defined information on the subject. No very clear or correct ideas prevail as to the characteristics of each engine, or their respective advantages or disadvantages; and, in meeting with those incidental allusions, of a more or less direct kind, which occur in so many publications of the day, to these machines, it must frequently be matter of doubt *which* "calculating machine" is referred to, or whether *both* are included in the general allusion.

We are desirous likewise of removing two misapprehensions which we know obtain, to some extent, respecting these engines. In the first place it is very generally supposed that the Difference Engine, after it had been completed up to a certain point, *suggested* the idea of the Analytical Engine; and that the second is in fact the improved offspring of the first, and *grew out* of the existence of its predecessor, through some natural or else accidental combination of ideas suggested by this one. Such a supposition is in this instance contrary to the facts; although it seems to be almost an obvious inference, wherever two inventions, similar in their nature and objects, succeed each other closely in order of *time*, and strikingly in order of *value*; more especially when the same individual is the author of both. Nevertheless the ideas which led to the Analytical Engine occurred in a manner wholly independent of any that were connected with the Difference Engine. These ideas are indeed in their own intrinsic nature independent of the latter engine, and might equally have occurred had it never existed nor been even thought of at all.

The second of the misapprehensions above alluded to, relates to the well-known suspension, during some years past, of all progress in the construction of the Difference Engine. Respecting the circumstances which have interfered with the actual completion of either invention, we offer no opinion; and in fact are not possessed of the data for doing so, had we the inclination. But we know that some persons suppose these obstacles (be they what they may) to have arisen *in consequence* of the subsequent invention of the Analytical Engine while the former was in progress. We have ourselves heard it even *lamented* that an idea should ever have occurred at all, which had turned out to be merely the means of arresting what was already in a course of successful execution, without substituting the superior invention in its stead. This notion we can contradict in the most unqualified manner. The progress of the Difference Engine had long been suspended, before there were even the least crude glimmerings of any invention superior to it. Such glimmerings, therefore, and their subsequent development, were in no way the original *cause* of that suspension; although, where difficulties of some kind or other evidently already existed, it was not perhaps calculated to remove or lessen them that an invention should have been meanwhile thought of, which, while including all that the first was capable of, possesses powers so extended as to eclipse it altogether.

We leave it for the decision of each individual (*after he has possessed himself* of competent information as to the characteristics of each engine), to determine how far it ought to be matter of regret that such an accession has been made to the powers of human science, even if it *has* (which we greatly doubt) increased to a certain limited extent some already existing difficulties that had arisen in the way of completing a valuable but lesser work. We leave it for each to satisfy himself as to the wisdom of desiring the obliteration (were that now possible) of all records of the more perfect invention, in order that the comparatively limited one might be finished. The Difference Engine would doubtless fulfil all those practical objects which it was originally destined for. It would certainly calculate all the tables that are more directly necessary for the physical purposes of life, such as nautical and other computations. Those who incline to very strictly utilitarian views, may perhaps feel that the peculiar powers of the Analytical Engine bear upon questions of abstract and speculative science, rather than upon those involving every-day and ordinary human interests. These persons being likely to possess but little sympathy, or possibly acquaintance, with any branches of science which they do not find to be *useful* (according to *their* definition of that word), may conceive that the undertaking of that engine, now that the other one is already in progress, would be a barren and unproductive laying out of yet more money and labour; in fact, a work of supererogation. Even in the utilitarian aspect, however, we do not doubt that very valuable practical results would be developed by the extended faculties of the Analytical Engine; some of which results we think we could now hint at, had we the space; and others, which it may not yet be possible to foresee, but which would be brought forth by the daily increasing requirements of science, and by a more intimate practical acquaintance with the powers of the engine, were it in actual existence.

On general grounds, both of an *à priori* description as well as those founded on the scientific history and experience of mankind, we see strong presumptions that such would be the case. Nevertheless all will probably concur in feeling that the completion of the Difference Engine would be far preferable to the non-completion of any calculating engine at all. With whomsoever or wheresoever may rest the present causes of difficulty that apparently exist towards either the completion of the old engine, or the commencement of the new one, we trust they will not ultimately result in this generation's being acquainted with these inventions through the medium of pen, ink and paper merely; and still more do we hope, that for the honour of our country's reputation in the future pages of history, these causes will not lead to the completion of the undertaking by some *other* nation or government. This could not but be matter of just regret; and equally so, whether the obstacles may have originated in private interests and feelings, in considerations of a more public description, or in causes combining the nature of both such solutions.

We refer the reader to the 'Edinburgh Review' of July 1834, for a very able account of the Difference Engine. The writer of the article we allude to, has selected as his prominent matter for exposition, a wholly different view of the subject from that which M. Menabrea has chosen. The former chiefly treats it under its mechanical aspect, entering but slightly into the mathematical principles of which that engine is the representative, but giving, in considerable length, many details of the mechanism and contrivances by means of which it tabulates the various orders of differences. M. Menabrea, on the contrary, exclusively developes the analytical view; taking it for granted that mechanism is able to perform certain processes, but without attempting to explain *how*; and devoting his whole attention to explanations and illustrations of the manner in which analytical laws can be so arranged and combined as to bring every branch of that vast subject within the grasp of the assumed powers of mechanism. It is obvious that, in the invention of a calculating engine, these two branches of the subject are equally essential fields of investigation, and that on their mutual adjustment, one to the other, must depend all success. They must be made to meet each other, so that the weak points in the powers of either department may be compensated by the strong points in those of the other. They are indissolubly connected, though so different in their intrinsic nature that perhaps the same mind might not be likely to prove equally profound or successful in both. We know those who doubt whether the powers of mechanism will in practice prove adequate in all respects to the demands made upon them in the working of such complicated trains of machinery as those of the above engines, and who apprehend that unforeseen practical difficulties and disturbances will arise in the way of accuracy and of facility of operation. The Difference Engine, however, appears to us to be in a great measure an answer to these doubts. It is complete as far as it goes, and it does work with all the anticipated success. The Analytical Engine, far from being more complicated, will in many respects be of simpler construction; and it is a remarkable circumstance attending it, that with very *simplified* means it is so much more powerful.

The article in the 'Edinburgh Review' was written some time previous to the occurrence of any ideas such as afterwards led to the invention of the Analytical Engine; and in the nature of the Difference Engine there is much less that would invite a writer to take exclusively, or even prominently, the mathematical view of it, than in that of the Analytical Engine; although mechanism has undoubtedly gone much further to meet mathematics, in the case of this engine, than of the former one. Some publication embracing the *mechanical* view of the Analytical Engine is a desideratum which we trust will be supplied before long.

Those who may have the patience to study a moderate quantity of rather dry details, will find ample compensation, after perusing the article of 1834, in the clearness with which a succinct view will have been attained of the various practical steps through which mechanism can accomplish certain processes; and they will also find themselves still further capable of appreciating M. Menabrea's more comprehensive and generalized memoir. The very difference in the style and object of these two articles, makes them peculiarly valuable to each other; at least for the purposes of those who really desire something more than a merely superficial and popular comprehension of the subject of calculating engines. A. A. L.

Note B.—Page 676.

That portion of the Analytical Engine here alluded to is called the storehouse. It contains an indefinite number of the columns of discs described by M. Menabrea. The reader may picture to himself a pile of rather large draughtsmen heaped perpendicularly one above another to a considerable height, each counter having the digits from 0 to 9 inscribed on its *edge* at equal intervals; and if he then conceives that the counters do not actually lie one upon another so as to be in contact, but are fixed at small intervals of vertical distance on a common axis which passes perpendicularly through their centres, and around which each disc can *revolve horizontally* so that any required digit amongst those inscribed on its margin can be brought into view, he will have a good idea of one of these columns. The *lowest* of the discs on any column belongs to the units, the next above to the tens, the next above this to the hundreds, and so on. Thus, if we wished to inscribe 1345 on a column of the engine, it would stand thus:—

1
3
4
5

In the Difference Engine there are seven of these columns placed side by side in a row, and the working mechanism extends behind them; the general form of the whole mass of machinery is that of a quadrangular prism (more or less approaching to the cube); the results always appearing on that perpendicular face of the engine which contains the columns of discs, opposite to which face a spectator may place himself. In the Analytical Engine there would be many more of these columns, probably at least two hundred. The precise form and arrangement which the whole mass of its mechanism will assume is not yet finally determined.

We may conveniently represent the columns of discs on paper in a diagram like the following:—

V_1	V_2	V_3	V_4	&c.
○	○	○	○	&c.
0	0	0	0	
0	0	0	0	
0	0	0	0	&c.
0	0	0	0	
□	□	□	□	&c.

The V's are for the purpose of convenient reference to any column, either in writing or speaking, and are consequently numbered. The reason why the letter V is chosen for this purpose in preference to any other letter, is because these columns are designated (as the reader will find in proceeding with the Memoir) the *Variables*, and sometimes the *Variable columns*, or the *columns of Variables*. The origin of this appellation is, that the values on the columns are destined to change, that is to *vary*, in every conceivable manner. But it is necessary to guard against the natural misapprehension that the columns are only intended to receive the values of the *variables* in an analytical formula, and not of the *constants*. The columns are called Variables on a ground wholly unconnected with the *analytical* distinction between constants and variables. In order to prevent the possibility of confusion, we have, both in the translation and in the notes, written Variable with a capital letter when we use the word to signify a *column of the engine*, and variable with a small letter when we mean the *variable of a formula*. Similarly, *Variable-cards* signify any cards that belong to a column of the engine.

To return to the explanation of the diagram: each circle at the top is intended to contain the algebraic sign + or −, either of which can be substituted* for the other, according as the number represented on the column below is positive or negative. In a similar manner any other purely *symbolical* results of algebraical processes might be made to appear in these circles. In Note A. the practicability of developing *symbolical* with no less ease than *numerical* results has been touched on.

The zeros beneath the *symbolic* circles represent each of them a disc, supposed to have the digit 0 presented in front. Only four tiers of zeros have been figured in the diagram, but these may be considered as representing thirty or forty, or any number of tiers of discs that may be required. Since each disc can present any digit, and each circle any sign, the discs of every column may be so adjusted† as to express any positive or negative number whatever within the limits of the machine; which limits depend on the *perpendicular* extent of the mechanism, that is, on the number of discs to a column.

* A fuller account of the manner in which the *signs* are regulated, is given in Mons. Menabrea's Memoir, pages 682, 683. He himself expresses doubts (in a note of his own at the bottom of the latter page) as to his having been likely to hit on the precise methods really adopted; his explanation being merely a conjectural one. That it *does* accord precisely with the fact is a remarkable circumstance, and affords a convincing proof how completely Mons. Menabrea has been imbued with the true spirit of the invention. Indeed the whole of the above Memoir is a striking production, when we consider that Mons. Menabrea had had but very slight means for obtaining any adequate ideas respecting the Analytical Engine. It requires however a considerable acquaintance with the abstruse and complicated nature of such a subject, in order fully to appreciate the penetration of the writer who could take so just and comprehensive a view of it upon such limited opportunity.

† This adjustment is done by hand merely.

Each of the squares below the zeros is intended for the inscription of any *general* symbol or combination of symbols we please; it being understood that the number represented on the column immediately above, is the numerical value of that symbol, or combination of symbols. Let us, for instance, represent the three quantities a, n, x, and let us further suppose that $a = 5$, $n = 7$, $x = 98$. We should have—

V_1	V_2	V_3	V_4 &c.
+ *	+	+	+
0	0	0	0
0	0	0	0 &c.
0	0	9	0
5	7	8	0 &c.
$\boxed{a}$	$\boxed{n}$	$\boxed{x}$	$\boxed{}$

We may now combine these symbols in a variety of ways, so as to form any required function or functions of them, and we may then inscribe each such function below brackets, every bracket uniting together those quantities (and those only) which enter into the function inscribed below it. We must also, when we have decided on the particular function whose numerical value we desire to calculate, assign another column to the right-hand for receiving the *results*, and must inscribe the function in the square below this column. In the above instance we might have any one of the following functions:—

$$a x^n, \quad x^{a^n}, \quad a . n . x, \quad \frac{a}{n} x, \quad a + n + x, \text{ \&c. \&c.}$$

Let us select the first. It would stand as follows, previous to calculation:—

V_1	V_2	V_3	V_4 &c.
+	+	+	+
0	0	0	0 &c.
0	0	0	0
0	0	9	0
5	7	8	0 &c.
$\boxed{a}$	$\boxed{n}$	$\boxed{x}$	$\boxed{ax^n}$ &c.

$$\underbrace{\boxed{a} \quad \boxed{n} \quad \boxed{x}}_{a x_n}$$

The data being given, we must now put into the engine the cards proper for directing the operations in the case of the particular function chosen. These operations would in this instance be,—

Firstly, six multiplications in order to get x^n ($= 98^7$ for the above particular data).

Secondly, one multiplication in order then to get $a . x^n$ ($= 5.98^7$).

In all, seven multiplications to complete the whole process. We may thus represent them:—

$$(\times, \times, \times, \times, \times, \times, \times), \text{ or } 7 (\times).$$

The multiplications would, however, at successive stages in the solution of the problem, operate on pairs of numbers, derived from *different* columns. In other words, the *same operation* would be performed

* It is convenient to omit the circles whenever the signs + or − can be actually represented.

on different *subjects of operation*. And here again is an illustration of the remarks made in the preceding Note on the independent manner in which the engine directs its *operations*. In determining the value of ax^n, the *operations* are *homogeneous*, but are distributed amongst different *subjects of operation*, at successive stages of the computation. It is by means of certain punched cards, belonging to the Variables themselves, that the action of the operations is so *distributed* as to suit each particular function. The *Operation-cards* merely determine the succession of operations in a general manner. They in fact throw all that portion of the mechanism included in the *mill*, into a series of different *states*, which we may call the *adding state*, or the *multiplying state*, &c. respectively. In each of these states the mechanism is ready to act in the way peculiar to that state, on any pair of numbers which may be permitted to come within its sphere of action. Only *one* of these operating states of the mill can exist at a time; and the nature of the mechanism is also such that only *one pair of numbers* can be received and acted on at a time. Now, in order to secure that the mill shall receive a constant supply of the proper pairs of numbers in succession, and that it shall also rightly locate the result of an operation performed upon any pair, each Variable has cards of its own belonging to it. It has, first, a class of cards whose business it is to *allow* the number on the Variable to pass into the mill, there to be operated upon. These cards may be called the *Supplying-cards*. *They* furnish the mill with its proper food. Each Variable has, secondly, another class of cards, whose office it is to allow the Variable to *receive* a number *from* the mill. These cards may be called the *Receiving-cards*. *They* regulate the location of results, whether temporary or ultimate results. The Variable-cards in general (including both the preceding classes) might, it appears to us, be even more appropriately designated the Distributive-cards, since it is through their means that the action of the operations, and the results of this action, are rightly *distributed*.

There are *two varieties* of the *Supplying* Variable-cards, respectively adapted for fulfilling two distinct subsidiary purposes: but as these modifications do not bear upon the present subject, we shall notice them in another place.

In the above case of ax^n, the Operation-cards merely order seven multiplications, that is, they order the mill to be in the *multiplying state* seven successive times (without any reference to the particular columns whose numbers are to be acted upon). The proper Distributive Variable-cards step in at each successive multiplication, and cause the distributions requisite for the particular case.

For x^{an}	the operations would be	$34\,(\times)$
... $a.n.x$	... ...	$(\times, \times)$, or $2\,(\times)$
... $\frac{a}{n}.x$	... ...	$(\div, \times)$
... $a+n+x$	... ...	$(+, +)$, or $2\,(+)$

The engine might be made to calculate all these in succession. Having completed ax^n, the function x^{an} might be written under the brackets instead of ax^n, and a new calculation commenced (the appropriate Operation and Variable-cards for the new function of course coming into play). The results would then appear on V_5. So on for any number of different functions of the quantities a, n, x. Each *result* might either permanently remain on its column during the succeeding calculations, so that when all the functions had been computed, their values would simultaneously exist on V_4, V_5, V_6, &c.; or each result might (after being printed off, or used in any specified manner) be effaced, to make way for its successor. The square under V_4 ought, for the latter arrangement, to have the functions $a x^n$, $x^{a n}$, $a n x$, &c. successively inscribed in it.

Let us now suppose that we have *two* expressions whose values have been computed by the engine independently of each other (each having its own group of columns for data and results). Let them be $a x^n$, $b \,.\, p \,.\, y$. They would then stand as follows on the columns:—

V_1	V_2	V_3	V_4	V_5	V_6	V_7	V_8	V_9
+	+	+	+	+	+	+	+	+
0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0
$\boxed{a}$	$\boxed{n}$	$\boxed{x}$	$\boxed{a x^n}$	$\boxed{b}$	$\boxed{p}$	$\boxed{y}$	$\boxed{b p y}$	$\boxed{\frac{a x^n}{b p y}}$

We may now desire to combine together these two *results*, in any manner we please; in which case it would only be necessary to have an additional card or cards, which should order the requisite operations to be performed with the numbers on the two result-columns, V_4 and V_8, and the *result of these further operations* to appear on a new column, V_9. Say that we wish to divide $a x^n$ by $b \,.\, p \,.\, y$. The numerical value of this division would then appear on the column V_9, beneath which we have inscribed $\frac{a x^n}{b p y}$. The whole series of operations from the beginning would be as follows (n being $= 7$):—

$$\left\{7(\times), 2(\times), \div\right\}, \text{or} \left\{9(\times), \div\right\}.$$

This example is introduced merely to show that we may, if we please, retain separately and permanently any *intermediate* results (like $a x^n$, $b.p.y.$), which occur in the course of processes having an ulterior and more complicated result as their chief and final object $\left(\text{like } \frac{a x^n}{b p y}\right)$.

Any group of columns may be considered as representing a *general* function, until a *special* one has been implicitly impressed upon them through the introduction into the engine of the Operation and Variable-cards made out for a *particular* function. Thus, in the preceding example, V_1, V_2, V_3, V_5, V_6, V_7 represent the *general* function $\phi(a, n, b, p, x, y)$ until the function $\frac{a x^n}{b \,.\, p \,.\, y}$ has been determined on, and *implicitly* expressed by the placing of the right cards in the engine. The actual working of the mechanism, as regulated by these cards, then *explicitly* developes the value of the function. The inscription of a function under the brackets, and in the square under the result-column, in no way influences the processes or the results, and is merely a memorandum for the observer, to remind him of what is going on. It is the Operation and the Variable-cards only, which in reality determine the function. Indeed it should be distinctly kept in mind that the inscriptions within *any* of the squares, are quite independent of the mechanism or workings of the engine, and are nothing but arbitrary memorandums placed there at pleasure to assist the spectator.

The further we analyse the manner in which such an engine performs its processes and attains its results, the more we perceive how distinctly it places in a true and just light the mutual relations and connexion of the various steps of mathematical analysis, how clearly it separates those things which are in reality distinct and independent, and unites those which are mutually dependent. A. A. L.

Note C.—Page 677.

Those who may desire to study the principles of the Jacquard-loom in the most effectual manner, viz. that of practical observation, have only to step into the Adelaide Gallery or the Polytechnic Institution. In each of these valuable repositories of scientific *illustration*, a weaver is constantly working at a Jacquard-loom, and is ready to give any information that may be desired as to the construction and modes of acting of his apparatus. The volume on the manufacture of silk, in Lardner's Cyclopædia, contains a chapter on the Jacquard-loom, which may also be consulted with advantage.

The mode of application of the cards, as hitherto used in the art of weaving, was not found, however, to be sufficiently powerful for all the simplifications which it was desirable to attain in such varied and complicated processes as those required in order to fulfil the purposes of an Analytical Engine. A method was devised of what was technically designated *backing* the cards in certain groups according to certain laws. The object of this extension is to secure the possibility of bringing any particular card or set of cards into use *any number of times successively* in the solution of one problem. Whether this power shall be taken advantage of or not, in each particular instance, will depend on the nature of the operations which the problem under consideration may require. The process is alluded to by M. Menabrea in page 680, and it is a very important simplification. It has been proposed to use it for the reciprocal benefit of that art, which, while it has itself no apparent connexion with the domains of abstract science, has yet proved so valuable to the latter, in suggesting the principles which, in their new and singular field of application, seem likely to place *algebraical* combinations not less completely within the province of mechanism, than are all those varied intricacies of which *intersecting threads* are susceptible. By the introduction of the system of *backing* into the Jacquard-loom itself, patterns which should possess symmetry, and follow regular laws of any extent, might be woven by means of comparatively few cards.

Those who understand the mechanism of this loom will perceive that the above improvement is easily effected in practice, by causing the prism over which the train of pattern-cards is suspended, to revolve *backwards* instead of *forwards*, at pleasure, under the requisite circumstances; until, by so doing, any particular card, or set of cards, that has done duty once, and passed on in the ordinary regular succession, is brought back to the position it occupied just before it was used the preceding time. The prism then resumes its *forward* rotation, and thus brings the card or set of cards in question into play a second time. This process may obviously be repeated any number of times. A. A. L.

NOTE D.—Page 680.

We have represented the solution of these two equations, with every detail, in a diagram* similar to those used in Note B.; but additional explanations are requisite, partly in order to make this more complicated case perfectly clear, and partly for the comprehension of certain indications and notations not used in the preceding diagrams. Those who may wish to understand Note G. completely, are recommended to pay particular attention to the contents of the present Note, or they will not otherwise comprehend the similar notation and indications when applied to a much more complicated case.

In all calculations, the columns of Variables used may be divided into three classes:—

1st. Those on which the data are inscribed:

2ndly. Those intended to receive the final results:

3dly. Those intended to receive such intermediate and temporary combinations of the primitive data as are not to be permanently retained, but are merely needed for *working with*, in order to attain the ultimate results. Combinations of this kind might properly be called *secondary data*. They are in fact so many *successive stages* towards the final result. The columns which receive them are rightly named *Working-Variables*, for their office is in its nature purely *subsidiary* to other purposes. They develope an intermediate and transient class of results, which unite the original data with the final results.

The Result-Variables sometimes partake of the nature of Working-Variables. It frequently happens that a Variable destined to receive a final result is the recipient of one or more intermediate values successively, in the course of the processes. Similarly, the Variables for data often become Working-Variables, or Result-Variables, or even both in succession. It so happens, however, that in the case of the present equations the three sets of offices remain throughout perfectly separate and independent.

It will be observed, that in the squares below the *Working*-Variables nothing is inscribed. Any one of these Variables is in many cases destined to pass through various values successively during the performance of a calculation (although in these particular equations no instance of this occurs). Consequently no *one fixed* symbol, or combination of symbols, should be considered as properly belonging to a merely *Working*-Variable; and as a general rule their squares are left blank. Of course in this, as in all other cases where we mention a *general* rule, it is understood that many particular exceptions may be expedient.

In order that all the indications contained in the diagram may be completely understood, we shall now explain two or three points, not hitherto touched on. When the value on any Variable is called into use, one of two consequences may be made to result. Either the value may *return* to the Variable after it has been used, in which case it is ready for a second use if needed; or the Variable may be made zero. (We are of course not considering a third case, of not unfrequent occurrence, in which the same Variable is destined to receive the *result* of the very operation which it has just supplied with a number.) Now the ordinary rule is, that the value *returns* to the Variable; unless it has been foreseen that no use for that value can recur, in which case

* See the diagram of page 711.

zero is substituted. At the *end* of a calculation, therefore, every column ought as a general rule to be zero, excepting those for results. Thus it will be seen by the diagram, that when m, the value on V_0, is used for the second time by Operation 5, V_0 becomes 0, since m is not again needed; that similarly, when $(m\,n' - m'\,n)$, on V_{12}, is used for the third time by Operation 11, V_{12} becomes zero, since $(m\,n' - m'n)$ is not again needed. In order to provide for the one or the other of the courses above indicated, there are *two* varieties of the *Supplying* Variable-cards. One of these varieties has provisions which cause the number given off from any Variable to *return* to that Variable after doing its duty in the mill. The other variety has provisions which cause *zero* to be substituted on the Variable, for the number given off. These two varieties are distinguished, when needful, by the respective appellations of the *Retaining* Supply-cards and the *Zero* Supply-cards. We see that the *primary* office (see Note B.) of both these varieties of cards is the same; they only differ in their *secondary* office.

Every Variable thus has belonging to it *one* class of *Receiving* Variable-cards and *two* classes of *Supplying* Variable-cards. It is plain however that only the *one* or the *other* of these two latter classes can be used by any one Variable for *one* operation; never *both* simultaneously; their respective functions being mutually incompatible.

It should be understood that the Variable-cards are not placed in *immediate contiguity* with the columns. Each card is connected by means of wires with the column it is intended to act upon.

Our diagram ought in reality to be placed side by side with M. Menabrea's corresponding table, so as to be compared with it, line for line belonging to each operation. But it was unfortunately inconvenient to print them in this desirable form. The diagram is, in the main, merely another manner of indicating the various relations denoted in M. Menabrea's table. Each mode has some advantages and some disadvantages. Combined, they form a complete and accurate method of registering every step and sequence in all calculations performed by the engine.

No notice has yet been taken of the *upper* indices which are added to the left of each V in the diagram; an addition which we have also taken the liberty of making to the V's in M. Menabrea's tables of pages 681, 684, since it does not *alter* anything therein represented by him, but merely *adds* something to the previous indications of those tables. The *lower* indices are obviously indices of *locality* only, and are wholly independent of the operations performed or of the results obtained, their value continuing unchanged during the performance of calculations. The *upper* indices, however, are of a different nature. Their office is to indicate any *alteration* in the value which a Variable represents; and they are of course liable to changes during the processes of a calculation. Whenever a Variable has only zeros upon it, it is called ${}^0V$; the moment a value appears on it (whether that value be placed there arbitrarily, or appears in the natural course of a calculation), it becomes ${}^1V$. If this value gives place to another value, the Variable becomes ${}^2V$, and so forth. Whenever a *value* again gives place to *zero*, the Variable again becomes ${}^0V$, even if it have been ${}^nV$ the moment before. If a *value* then again be substituted, the Variable becomes ${}^{n+1}V$ (as it would have done if it had not passed through the intermediate ${}^0V$); &c. &c. Just before any calculation is commenced, and after the data have been given, and everything adjusted and prepared for setting the mechanism in action, the upper indices of the Variables for data are all unity, and those for the Working and Result-variables are all zero. In this state the diagram represents them*.

There are several advantages in having a set of indices of this nature; but these advantages are perhaps hardly of a kind to be immediately perceived, unless by a mind somewhat accustomed to trace the successive steps by means of which the engine accomplishes its purposes. We have only space to mention in a general way, that the whole notation of the tables is made more consistent by these indices, for they are able to mark a *difference* in certain cases, where there would otherwise be an apparent *identity* confusing in its tendency. In such a case as $V_n = V_p + V_n$ there is more clearness and more consistency with the usual laws of algebraical notation, in being able to write ${}^{m+1}V_n = {}^{q}V_p + {}^{m}V_n$. It is also obvious that the indices furnish a powerful means of tracing back the derivation of any result; and of registering various circumstances concerning that *series of successive substitutions*, of which every *result* is in fact merely the final consequence; circumstances that may in certain cases involve relations which it is important to observe, either for purely analytical reasons, or for practically adapting the workings of the engine to their occurrence. The series of substitutions which lead to the equations of the diagram are as follow:—

$$\overset{(1.)}{{}^{1}V^{*}_{16}} = \overset{(2.)}{\frac{{}^{1}V_{14}}{{}^{1}V_{12}}} = \overset{(3.)}{\frac{{}^{1}V_{10} - {}^{1}V_{11}}{{}^{1}V_{6} - {}^{1}V_{7}}} = \overset{(4.)}{\frac{{}^{1}V_{0} \cdot {}^{1}V_{5} - {}^{1}V_{2} \cdot {}^{1}V_{3}}{{}^{1}V_{0} \cdot {}^{1}V_{4} - {}^{1}V_{3} \cdot {}^{1}V_{1}}} = \frac{d'm - dm'}{mn' - m'n}$$

$$\overset{(1.)}{{}^{1}V_{15}} = \overset{(2.)}{\frac{{}^{1}V_{13}}{{}^{1}V_{12}}} = \overset{(3.)}{\frac{{}^{1}V_{8} - {}^{1}V_{9}}{{}^{1}V_{6} - {}^{1}V_{7}}} = \overset{(4.)}{\frac{{}^{1}V_{2} \cdot {}^{1}V_{4} - {}^{1}V_{5} \cdot {}^{1}V_{1}}{{}^{1}V_{0} \cdot {}^{1}V_{4} - {}^{1}V_{3} \cdot {}^{1}V_{1}}} = \frac{dn' - d'n}{mn' - m'n}.$$

There are *three* successive substitutions for each of these equations. The formulæ (2.), (3.), and (4.) are *implicitly* contained in (1.), which latter we may consider as being in fact the *condensed* expression of any of the former. It will be observed that every succeeding substitution must contain *twice* as many V's as its predecessor. So that if a problem require n substitutions, the successive series of numbers for the V's in the whole of them will be $2, 4, 8, 16 \ldots 2^n$.

The substitutions in the preceding equations happen to be of little value towards illustrating the power and uses of the upper indices; for owing to the nature of these particular equations the indices are all unity throughout. We wish we had space to enter more fully into the relations which these indices would in many cases enable us to trace.

M. Menabrea incloses the three centre columns of his table under the general title *Variable-cards*. The V's however in reality all represent the actual *Variable-columns* of the engine, and not the cards that belong to them. Still the title is a very just one, since it is through the special action of certain Variable-cards (when *combined* with the more generalised agency of the Operation-cards) that every one of the particular relations he has indicated under that title is brought about.

Suppose we wish to ascertain how often any *one* quantity, or combi-

* We recommend the reader to trace the successive substitutions backwards from (1.) to (4.), in Mons. Menabrea's Table. This he will easily do by means of the upper and lower indices, and it is interesting to observe how each V successively ramifies (so to speak) into two other V's in some other column of the Table; until at length the V's of the original data are arrived at.

nation of quantities, is brought into use during a calculation. We easily ascertain *this*, from the inspection of any vertical column or columns of the diagram in which that quantity may appear. Thus, in the present case, we see that all the data, and all the intermediate results likewise, are used twice, excepting $(m n' - m' n)$, which is used three times.

The *order* in which it is possible to perform the operations for the present example, enables us to effect all the eleven operations of which it consists, with only *three Operation-cards*; because the problem is of such a nature that it admits of each *class* of operations being performed in a group together; all the multiplications one after another, all the subtractions one after another, &c. The operations are $\left\{6(\times), 3(-), 2(+)\right\}$.

Since the very definition of an operation implies that there must be *two* numbers to act upon, there are of course *two Supplying* Variable-cards necessarily brought into action for every operation, in order to furnish the two proper numbers. (See Note B.) Also, since every operation must produce a *result*, which must be placed *somewhere*, each operation entails the action of a *Receiving* Variable-card, to indicate the proper locality for the result. Therefore, at least three times as many Variable-cards as there are *operations* (not *Operation-cards*, for these, as we have just seen, are by no means always as numerous as the *operations*) are brought into use in every calculation. Indeed, under certain contingencies, a still larger proportion is requisite; such, for example, would probably be the case when the same result has to appear on more than one Variable simultaneously (which is not unfrequently a provision necessary for subsequent purposes in a calculation), and in some other cases which we shall not here specify. We see therefore that a great disproportion exists between the amount of *Variable* and of *Operation*-cards requisite for the working of even the simplest calculation.

All calculations do not admit, like this one, of the operations of the same nature being performed in groups together. Probably very few do so without exceptions occurring in one or other stage of the progress; and some would not admit it at all. The *order* in which the operations shall be performed in every particular case, is a very interesting and curious question, on which our space does not permit us fully to enter. In almost every computation a great *variety* of arrangements for the succession of the processes is possible, and various considerations must influence the selection amongst them for the purposes of a Calculating Engine. One essential object is to choose that arrangement which shall tend to reduce to a minimum the *time* necessary for completing the calculation.

It must be evident how multifarious and how mutually complicated are the considerations which the workings of such an engine involve. There are frequently several distinct *sets of effects* going on simultaneously; all in a manner independent of each other, and yet to a greater or less degree exercising a mutual influence. To adjust each to every other, and indeed even to perceive and trace them out with perfect correctness and success, entails difficulties whose nature partakes to a certain extent of those involved in every question where *conditions* are very numerous and inter-complicated; such as for instance the estimation of the mutual relations amongst *statistical* phænomena, and of those involved in many other classes of facts. A. A. L.

Diagram belonging to Note D.

Number of Operations.	Nature of Operations.	Variables for Data.						Working Variables.									Variables for Results.	
		$^1V_0$	$^1V_1$	$^1V_2$	$^1V_3$	$^1V_4$	$^1V_5$	$^0V_6$	$^0V_7$	$^0V_8$	$^0V_9$	$^0V_{10}$	$^0V_{11}$	$^0V_{12}$	$^0V_{13}$	$^0V_{14}$	$^0V_{15}$	$^0V_{16}$
		+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
		0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
		0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
		0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
		0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
		$\boxed{m}$	$\boxed{n}$	$\boxed{d}$	$\boxed{m'}$	$\boxed{n'}$	$\boxed{d'}$	$\boxed{\ }$	$\boxed{\ }$	$\boxed{\ }$	$\boxed{\ }$	$\boxed{\ }$	$\boxed{\ }$	$\boxed{\ }$	$\boxed{\ }$	$\boxed{\ }$	$\boxed{\frac{dn'-d'n}{mn'-m'n}=x}$	$\boxed{\frac{d'm-dm'}{mn'-m'n}=y}$
1	$\times$	m	......	......	......	n'	......	mn'										
2	$\times$	......	n	......	m'	......	......	......	$m'n$									
3	$\times$	......	......	d	......	......	......	......	......	dn'								
4	$\times$	......	0	......	......	......	d'	......	......	......	$d'n$							
5	$\times$	0	......	......	......	......	0	......	......	......	......	$d'm$						
6	$\times$	......	......	0	0	......	......	......	......	......	......	......	dm'					
7	$-$	......	......	......	......	......	......	0	0	......	......	......	......	$(mn'-m'n)$				
8	$-$	......	......	......	......	......	......	......	......	0	0	......	......	......	$(dn'-d'n)$			
9	$-$	......	......	......	......	......	......	......	......	......	......	0	0	......	......	$(d'm-dm')$		
10	$\div$	......	......	......	......	......	......	......	......	......	......	......	......	$(mn'-m'n)$	0	......	$\frac{dn'-d'n}{mn'-m'n}=x$	
11	$+$	......	......	......	......	......	......	......	......	......	......	......	......	0	......	0	......	$\frac{d'm-dm'}{mn'-m'n}=y$

NOTE E.—Page 684.

This example has evidently been chosen on account of its brevity and simplicity, with a view merely to explain the *manner* in which the engine would proceed in the case of an *analytical calculation containing variables*, rather than to illustrate the *extent of its powers* to solve cases of a difficult and complex nature. The equations of page 679 are in fact a more complicated problem than the present one.

We have not subjoined any diagram of its development for this new example, as we did for the former one, because this is unnecessary after the full application already made of those diagrams to the illustration of M. Menabrea's excellent tables.

It may be remarked that a slight discrepancy exists between the formulæ

$$(a + b\,x')$$
$$(A + B\cos' x)$$

given in the Memoir as the *data* for calculation, and the *results* of the calculation as developed in the last division of the table which accompanies it. To agree perfectly with this latter, the data should have been given as

$$(a\,x^0 + b\,x')$$
$$(A\cos^0 x + B\cos' x).$$

The following is a more complicated example of the manner in which the engine would compute a trigonometrical function containing variables. To multiply

$$A + A_1\cos\theta + A_2\cos 2\theta + A_3\cos 3\theta + \ldots$$

by $\qquad B + B_1\cos\theta$

Let the resulting products be represented under the general form

$$C_0 + C_1\cos\theta + C_2\cos 2\theta + C_3\cos 3\theta + \quad\ldots\ldots \qquad (1.)$$

This trigonometrical series is not only in itself very appropriate for illustrating the processes of the engine, but is likewise of much practical interest from its frequent use in astronomical computations. Before proceeding further with it, we shall point out that there are three very distinct classes of ways in which it may be desired to deduce numerical values from any analytical formula.

First. We may wish to find the collective numerical value of the *whole formula*, without any reference to the quantities of which that formula is a function, or to the particular mode of their combination and distribution, of which the formula is the result and representative. Values of this kind are of a strictly arithmetical nature in the most limited sense of the term, and retain no trace whatever of the processes through which they have been deduced. In fact, any one such numerical value may have been attained from an *infinite variety* of data, or of problems. The values for x and y in the two equations (see Note D.), come under this class of numerical results.

Secondly. We may propose to compute the collective numerical value of *each term* of a formula, or of a series, and to keep these results separate. The engine must in such a case appropriate as many columns to *results* as there are terms to compute.

Thirdly. It may be desired to compute the numerical value of various *subdivisions of each term*, and to keep all these results separate. It may be required, for instance, to compute each coefficient separately from its variable, in which particular case the engine must appropriate *two* result-columns to *every term that contains both a variable and coefficient.*

There are many ways in which it may be desired in special cases to distribute and keep separate the numerical values of different parts of an algebraical formula; and the power of effecting such distributions to any extent is essential to the *algebraical* character of the Analytical Engine. Many persons who are not conversant with mathematical studies, imagine that because the business of the engine is to give its results in *numerical notation*, the *nature of its processes* must consequently be *arithmetical* and *numerical*, rather than *algebraical* and *analytical*. This is an error. The engine can arrange and combine its numerical quantities exactly as if they were *letters* or any other *general* symbols; and in fact it might bring out its results in algebraical *notation*, were provisions made accordingly. It might develope three sets of results simultaneously, viz. *symbolic* results (as already alluded to in Notes A. and B.); *numerical* results (its chief and primary object); and *algebraical* results in *literal* notation. This latter however has not been deemed a necessary or desirable addition to its powers, partly because the necessary arrangements for effecting it would increase the complexity and extent of the mechanism to a degree that would not be commensurate with the advantages, where the main object of the invention is to translate into *numerical* language general formulæ of analysis already known to us, or whose laws of formation are known to us. But it would be a mistake to suppose that because its *results* are given in the *notation* of a more restricted science, its *processes* are therefore restricted to those of that science. The object of the engine is in fact to give the *utmost practical efficiency* to the resources of *numerical interpretations* of the higher science of analysis, while it uses the processes and combinations of this latter.

To return to the trigonometrical series. We shall only consider the four first terms of the factor $(A + A_1 \cos\theta + \&c.)$, since this will be sufficient to show the method. We propose to obtain separately the numerical value of *each coefficient* C_0, C_1, &c. of (1.). The direct multiplication of the two factors gives

$$\left.\begin{array}{l} BA + BA_1\cos\theta + BA_2\cos 2\theta + BA_3\cos 3\theta + \ldots\ldots\ldots\ldots \\ \quad + B_1A\cos\theta + B_1A_1\cos\theta.\cos\theta + B_1A_2\cos 2\theta.\cos\theta + B_1A_3\cos 3\theta.\cos\theta \end{array}\right\}(2.)$$

a result which would stand thus on the engine :—

Variables for Data.

V_0	V_1	V_2	V_3	V_{10}	V_{11}
$\boxed{A}$	$\boxed{A_1}$	$\boxed{A_2}$	$\boxed{A_3}$	$\boxed{B}$	$\boxed{B_1}$
	$\cos\theta$	$\cos 2\theta$	$\cos 3\theta$		$\cos\theta$

Variables for Results.

V_{20}	V_{21}	V_{22}	V_{23}	V_{31}	V_{32}	V_{33}	V_{34}
$\boxed{BA}$	$\boxed{BA_1}$	$\boxed{BA_2}$	$\boxed{BA_3}$	$\boxed{B_1A}$	$\boxed{B_1A_1}$	$\boxed{B_1A_2}$	$\boxed{B_1A_3}$
	$\cos\theta$	$\cos 2\theta$	$\cos 3\theta$	$\cos\theta$	$(\cos\theta.\cos\theta)$	$(\cos 2\theta.\cos\theta)$	$(\cos 3\theta.\cos\theta)$

The variable belonging to each coefficient is written below it, as we have done in the diagram, by way of memorandum. The only further reduction which is at first apparently possible in the preceding result, would be the addition of V_{21} to V_{31} (in which case $B_1 A$ should be effaced from V_{31}). The whole operations from the beginning would then be—

First Series of Operations.	Second Series of Operations.	Third Series, which contains only one (final) operation.
${}^1V_{10} \times {}^1V_0 = {}^1V_{20}$	${}^1V_{11} \times {}^1V_0 = {}^1V_{31}$	${}^1V_{21} + {}^1V_{31} = {}^2V_{21}$, and V_{31} becomes $=0$.
${}^1V_{10} \times {}^1V_1 = {}^1V_{21}$	${}^1V_{11} \times {}^1V_1 = {}^1V_{32}$	
${}^1V_{10} \times {}^1V_2 = {}^1V_{22}$	${}^1V_{11} \times {}^1V_2 = {}^1V_{33}$	
${}^1V_{10} \times {}^1V_3 = {}^1V_{23}$	${}^1V_{11} \times {}^1V_3 = {}^1V_{34}$	

We do not enter into the same detail of *every* step of the processes as in the examples of Notes D. and G., thinking it unnecessary and tedious to do so. The reader will remember the meaning and use of the upper and lower indices, &c., as before explained.

To proceed: we know that

$$\cos n\theta . \cos\theta = \tfrac{1}{2}\cos \overline{n+1}\,\theta + \tfrac{1}{2}\,\overline{n-1}\,.\theta \quad . \; . \; . \quad (3.)$$

Consequently, a slight examination of the second line of (2.) will show that by making the proper substitutions, (2.) will become

$$\begin{array}{r|l|l|l|l}
BA & +BA_1\ .\cos\theta & +BA_2\ .\cos 2\theta & +BA_3\ .\cos 3\theta & \\
 & +B_1A\ .\cos\theta & & & \\
+\frac{1}{2}B_1A_1 & & +\frac{1}{2}B_1A_1\,.\cos 2\theta & & \\
 & +\frac{1}{2}B_1A_2\,.\cos\theta & & +\frac{1}{2}B_1A_2\,.\cos 3\theta & \\
 & & +\frac{1}{2}B_1A_3\,.\cos 2\theta & & +\frac{1}{2}B_1A_3\,.\cos 4\theta \\
C_0 & C_1 & C_2 & C_3 & C_4
\end{array}$$

These coefficients should respectively appear on

V_{20} $\quad V_{21}$ $\quad V_{22}$ $\quad V_{23}$ $\quad V_{24}$.

We shall perceive, if we inspect the particular arrangement of the results in (2.) on the Result-columns as represented in the diagram, that, in order to effect this transformation, each successive coefficient upon V_{32}, V_{33}, &c. (beginning with V_{32}), must through means of proper cards be divided by *two**; and that one of the halves thus

* This division would be managed by ordering the number two to appear on any separate new column which should be conveniently situated for the purpose, and then directing this column (which is in the strictest sense a *Working*-Variable) to divide itself successively with V_{33}, V_{34}, &c.

obtained must be added to the coefficient on the Variable which precedes it by ten columns, and the other half to the coefficient on the Variable which precedes it by twelve columns; V_{32}, V_{33}, &c. themselves becoming zeros during the process.

This series of operations may be thus expressed:—

Fourth Series.

$$\begin{cases} {}^{1}V_{32} \div 2 + {}^{1}V_{22} = {}^{2}V_{22} = B\,A_2 + \tfrac{1}{2}B_1\,A_1 \\ {}^{1}V_{32} \div 2 + {}^{1}V_{20} = {}^{2}V_{20} = B\,A + \tfrac{1}{2}B_1\,A_1 \ldots\ldots = C_0 \end{cases}$$

$$\begin{cases} {}^{1}V_{33} \div 2 + {}^{1}V_{23} = {}^{2}V_{23} = B\,A_3 + \tfrac{1}{2}B_1\,A_2 \ldots\ldots = C_3{}^{*} \\ {}^{1}V_{33} \div 2 + {}^{2}V_{21} = {}^{3}V_{21} = B\,A_1 + B_1\,A + \tfrac{1}{2}B_1\,A_2 = C_1 \end{cases}$$

$$\begin{cases} {}^{1}V_{34} \div 2 + {}^{0}V_{24} = {}^{1}V_{24} = \tfrac{1}{2}B_1\,A_3 \ldots\ldots\ldots\ldots = C_4 \\ {}^{1}V_{34} \div 2 + {}^{2}V_{22} = {}^{3}V_{22} = B\,A_2 + \tfrac{1}{2}B_1\,A_1 + \tfrac{1}{2}B_1\,A_3 = C_2. \end{cases}$$

The calculation of the coefficients C_0, C_1, &c. of (1.), would now be completed, and they would stand ranged in order on V_{20}, V_{21}, &c. It will be remarked, that from the moment the fourth series of operations is ordered, the Variables V_{31}, V_{32}, &c. cease to be *Result*-Variables, and become mere *Working*-Variables.

The substitution made by the engine of the processes in the second side of (3.) for those in the first side, is an excellent illustration of the manner in which we may arbitrarily order it to substitute any function, number, or process, at pleasure, for any other function, number or process, on the occurrence of a specified contingency.

We will now suppose that we desire to go a step further, and to obtain the numerical value of each *complete* term of the product (1.), that is of each *coefficient and variable united*, which for the $(n+1)$th term would be $C_n \,.\, \cos n\,\theta$.

We must for this purpose place the variables themselves on another set of columns, V_{41}, V_{42}, &c., and then order their successive multiplication by V_{21}, V_{22}, &c., each for each. There would thus be a final series of operations as follows:—

Fifth and Final Series of Operations.

$$\begin{aligned} {}^{2}V_{20} \times {}^{0}V_{40} &= {}^{1}V_{40} \\ {}^{3}V_{21} \times {}^{0}V_{41} &= {}^{1}V_{41} \\ {}^{3}V_{22} \times {}^{0}V_{42} &= {}^{1}V_{42} \\ {}^{2}V_{23} \times {}^{0}V_{43} &= {}^{1}V_{43} \\ {}^{1}V_{24} \times {}^{0}V_{44} &= {}^{1}V_{44} \end{aligned}$$

(N.B. that V_{40} being intended to receive the coefficient on V_{20} which has *no* variable, will only have $\cos 0\,\theta\,(=1)$ inscribed on it, preparatory to commencing the fifth series of operations.)

From the moment that the fifth and final series of operations is ordered, the Variables V_{20}, V_{21}, &c. then in their turn cease to be *Result*-

* It should be observed, that were the rest of the factor ($A + A \cos\theta + $&c.) taken into account, instead of *four* terms only, C_3 would have the additional term $\tfrac{1}{2}B_1\,A_4$; and C_4 the two additional terms, $B\,A_4$, $\tfrac{1}{2}B_1\,A_5$. This would indeed have been the case had even *six* terms been multiplied.

Variables and become mere *Working*-Variables; V_{40}, V_{41}, &c. being now the recipients of the ultimate results.

We should observe, that if the variables $\cos\theta$, $\cos 2\theta$, $\cos 3\theta$, &c. are furnished, they would be placed directly upon V_{41}, V_{42}, &c., like any other data. If not, a separate computation might be entered upon in a separate part of the engine, in order to calculate them, and place them on V_{41}, &c.

We have now explained how the engine might compute (1.) in the most direct manner, supposing we knew nothing about the *general* term of the resulting series. But the engine would in reality set to work very differently, whenever (as in this case) we *do* know the law for the general term.

The two first terms of (1.) are

$$(\mathrm{B}\,\mathrm{A} + \tfrac{1}{2}\mathrm{B}_1\,\mathrm{A}_1) + (\overline{\mathrm{B}\,\mathrm{A}_1 + \mathrm{B}_1\,\mathrm{A} + \tfrac{1}{2}\mathrm{B}_1\,\mathrm{A}_2}\,.\cos\theta) \ldots\ldots (4.)$$

and the general term for all after these is

$$(\mathrm{B}\,\mathrm{A}_n + \tfrac{1}{2}\mathrm{B}_1\,.\,\overline{\mathrm{A}_{n-1} + \mathrm{A}_{n+1}})\cos n\,\theta \ldots\ldots\ldots\ldots\ldots (5.)$$

which is the coefficient of the $(n+1)^{\text{th}}$ term. The engine would calculate the two first terms by means of a separate set of suitable Operation-cards, and would then need another set for the third term; which last set of Operation-cards would calculate all the succeeding terms *ad infinitum*; merely requiring certain new Variable-cards for each term to direct the operations to act on the proper columns. The following would be the successive sets of operations for computing the coefficients of $n+2$ terms:—

$$(\times, \times, \div, +), (\times, \times, \times, \div, +, +), n(\times, +, \times, \div, +).$$

Or we might represent them as follows, according to the numerical order of the operations:—

$$(1, 2 \ldots 4), (5, 6 \ldots 10), n\,(11, 12 \ldots 15).$$

The brackets, it should be understood, point out the relation in which the operations may be *grouped*, while the comma marks *succession*. The symbol + might be used for this latter purpose, but this would be liable to produce confusion, as + is also necessarily used to represent one class of the actual operations which are the subject of that succession. In accordance with this meaning attached to the comma, care must be taken when any one group of operations recurs more than once, as is represented above by $n\,(11 \ldots 15)$, not to insert a comma after the number or letter prefixed to that group. $n, (11 \ldots 15)$ would stand for *an operation n, followed by the group of operations* $(11 \ldots 15)$; instead of denoting *the number of groups which are to follow each other.*

Wherever a *general term* exists, there will be a *recurring group* of operations, as in the above example. Both for brevity and for distinctness, a *recurring group* is called a *cycle*. A *cycle* of operations, then, must be understood to signify any *set of operations* which is repeated *more than once*. It is equally a *cycle*, whether it be repeated *twice* only, or an indefinite number of times; for it is the fact *of a repetition occurring at all* that constitutes it such. In many cases of analysis there is a *recurring group* of one or more *cycles*; that is, a *cycle of a cycle*, or a *cycle of cycles*. For instance: suppose we wish to divide a series by a series,

$$(1.)\qquad \frac{a + b\,x + c\,x^2 + \ldots\ldots}{a' + b'\,x + c'\,x^2 + \ldots\ldots},$$

it being required that the result shall be developed, like the dividend and the divisor, in successive powers of x. A little consideration of (1.), and of the steps through which algebraical division is effected, will show that (if the denominator be supposed to consist of p terms) the first partial quotient will be completed by the following operations:—

$$(2.)\qquad \Big\{(\div), p\,(\times, -)\Big\} \quad \text{or} \quad \Big\{(1), p\,(2, 3)\Big\},$$

that the second partial quotient will be completed by an exactly similar set of operations, which acts on the remainder obtained by the first set, instead of on the original dividend. The whole of the processes therefore that have been gone through, by the time the *second* partial quotient has been obtained, will be,—

$$(3.)\qquad 2\Big\{(\div), p\,(\times, -)\Big\} \quad \text{or}\ 2\Big\{(1), p\,(2, 3)\Big\},$$

which is a cycle that includes a cycle, or a cycle of the second order. The operations for the *complete* division, supposing we propose to obtain n terms of the series constituting the quotient, will be,—

$$(4.)\qquad n\Big\{(\div), p\,(\times, -)\Big\} \quad \text{or}\ n\Big\{(1), p\,(2, 3)\Big\}.$$

It is of course to be remembered that the process of algebraical division in reality continues *ad infinitum*, except in the few exceptional cases which admit of an exact quotient being obtained. The number n in the formula (4.), is always that of the number of terms we propose to ourselves to obtain; and the nth partial quotient is the coefficient of the $(n-1)$th power of x.

There are some cases which entail *cycles of cycles of cycles*, to an indefinite extent. Such cases are usually very complicated, and they are of extreme interest when considered with reference to the engine. The algebraical development in a series, of the nth function of any given function, is of this nature. Let it be proposed to obtain the nth function of

$$(5.)\qquad \phi\,(a, b, c\ \ldots\ldots\ x),\ x \text{ being the variable.}$$

We should premise that we suppose the reader to understand what is meant by an nth function. We suppose him likewise to comprehend distinctly the difference between developing *an* nth *function algebraically*, and merely *calculating an* nth *function arithmetically*. If he does not, the following will be by no means very intelligible; but we have not space to give any preliminary explanations. To proceed: the law, according to which the successive functions of (5.) are to be developed, must of course first be fixed on. This law may be of very various kinds. We may propose to obtain our results in successive *powers* of x, in which case the general form would be

$$C + C_1\,x + C_2\,x^2 + \&c.,$$

or in successive powers of n itself, the index of the function we are ultimately to obtain, in which case the general form would be

$$C + C_1 n + C_2 n^2 + \&c.,$$

and x would only enter in the coefficients. Again, other functions of x or of n instead of *powers*, might be selected. It might be in addition proposed, that the coefficients themselves should be arranged according to given functions of a certain quantity. Another mode would be to make equations arbitrarily amongst the coefficients only, in which case the several functions, according to either of which it might be possible to develop the nth function of (5.), would have to be determined from the combined consideration of these equations and of (5.) itself.

The *algebraical* nature of the engine (so strongly insisted on in a previous part of this Note) would enable it to follow out any of these various modes indifferently; just as we recently showed that it can distribute and separate the numerical results of any one prescribed series of processes, in a perfectly arbitrary manner. Were it otherwise, the engine could merely *compute the arithmetical* nth *function*, a result which, like any other purely arithmetical results, would be simply a collective number, bearing no traces of the data or the processes which had led to it.

Secondly, the *law* of development for the nth function being selected, the next step would obviously be to develope (5.) itself, according to this law. This result would be the first function, and would be obtained by a determinate series of processes. These in most cases would include amongst them one or more *cycles* of operations.

The third step (which would consist of the various processes necessary for effecting the actual substitution of the series constituting the *first function*, for the *variable* itself) might proceed in either of two ways. It might make the substitution either wherever x occurs in the original (5.), or it might similarly make it wherever x occurs in the first function itself which is the equivalent of (5.). In some cases the former mode might be best, and in others the latter.

Whichever is adopted, it must be understood that the result is to appear arranged in a series following the law originally prescribed for the development of the nth function. This result constitutes the second function; with which we are to proceed exactly as we did with the first function, in order to obtain the third function; and so on, $n - 1$ times, to obtain the nth function. We easily perceive that since every successive function is arranged in a series *following the same law*, there would (after the *first* function is obtained) be a *cycle, of a cycle, of a cycle*, &c. of operations *, one, two, three, up to $n - 1$ times, in order to get the nth function. We say, *after the first function is obtained*, because (for reasons on which we cannot here enter) the *first*

* A cycle that includes n other cycles, successively *contained one within another*, is called a cycle of the $n + 1$th order. A cycle may simply *include* many other cycles, and yet only be of the second order. If a series follows a certain law for a certain number of terms, and then another law for another number of terms, there will be a cycle of operations for every new law; but these cycles will not be *contained one within another*,—they merely *follow each other*. Therefore their number may be infinite without influencing the *order* of a cycle that includes a repetition of such a series.

function might in many cases be developed through a set of processes peculiar to itself, and not recurring for the remaining functions.

We have given but a very slight sketch, of the principal *general* steps which would be requisite for obtaining an nth function of such a formula as (5.). The question is so exceedingly complicated, that perhaps few persons can be expected to follow, to their own satisfaction, so brief and general a statement as we are here restricted to on this subject. Still it is a very important case as regards the engine, and suggests ideas peculiar to itself, which we should regret to pass wholly without allusion. Nothing could be more interesting than to follow out, in every detail, the solution by the engine of such a case as the above; but the time, space and labour this would necessitate, could only suit a very extensive work.

To return to the subject of *cycles* of operations: some of the notation of the integral calculus lends itself very aptly to express them: (2.) might be thus written:—

$$(6.)\qquad (\div), \Sigma(+1)^p(\times, -) \text{ or } (1), \Sigma(+1)^p(2, 3),$$

where p stands for the variable; $(+1)^p$ for the function of the variable, that is, for ϕp; and the limits are from 1 to p, or from 0 to $p-1$, each increment being equal to unity. Similarly, (4.) would be,—

$$(7.)\qquad \Sigma(+1)^n\left\{(\div), \Sigma(+1)^p(\times, -)\right\}$$

the limits of n being from 1 to n, or from 0 to $n-1$,

$$(8.)\qquad \text{or } \Sigma(+1)^n\left\{(1), \Sigma(+1)^p(2, 3)\right\}.$$

Perhaps it may be thought that this notation is merely a circuitous way of expressing what was more simply and as effectually expressed before; and, in the above example, there may be some truth in this. But there is another description of cycles which *can* only effectually be expressed, in a condensed form, by the preceding notation. We shall call them *varying cycles*. They are of frequent occurrence, and include successive cycles of operations of the following nature:—

$$(9.)\ p\,(1, 2, \ldots m), \overline{p-1}\,(1, 2 \ldots m), \overline{p-2}\,(1, 2 \ldots m) \ldots \overline{p-n}\,(1, 2 \ldots m),$$

where each cycle contains the same group of operations, but in which the number of repetitions of the group varies according to a fixed rate, with every cycle. (9.) can be well expressed as follows:—

$$(10.)\qquad \Sigma p\,(1, 2, \ldots m), \text{ the limits of } p \text{ being from } p-n \text{ to } p.$$

Independent of the intrinsic advantages which we thus perceive to result in certain cases from this use of the notation of the integral calculus, there are likewise considerations which make it interesting, from the connections and relations involved in this new application. It has been observed in some of the former Notes, that the processes used in analysis form a logical system of much higher generality than the applications to number merely. Thus, when we read over any algebraical formula, considering it exclusively with reference to the processes of the engine, and putting aside for the moment its abstract signification as to the relations of quantity, the symbols $+$, $\times$, &c., in reality represent (as their immediate and proximate effect, when the formula is applied to the engine) that a certain prism which is a part of the mechanism (see Note C.), turns a new face, and thus presents a new card to act on the bundles of levers of the engine; the new card being perforated with holes, which are arranged according to the peculiarities of the operation of addition, or of multiplication, &c. Again, the *numbers* in the preceding formula (8.), each of them really represents one of these very pieces of card that are hung over the prism.

Now in the use made in the formulæ (7.), (8.) and (10.), of the notation of the integral calculus, we have glimpses of a similar new application of the language of the *higher* mathematics. Σ, in reality, here indicates that when a certain number of cards have acted in succession, the prism over which they revolve must *rotate backwards*, so as to bring those cards into their former position; and the limits 1 to n, 1 to p, &c., regulate how often this backward rotation is to be repeated. A. A. L.

Note F.—Page 688.

There is in existence a beautiful woven portrait of Jacquard, in the fabrication of which 24,000 cards were required.

The power of *repeating* the cards, alluded to by M. Menabrea in page 680, and more fully explained in Note C., reduces to an immense extent the number of cards required. It is obvious that this mechanical improvement is especially applicable wherever *cycles* occur in the mathematical operations, and that, in preparing data for calculations by the engine, it is desirable to arrange the order and combination of the processes with a view to obtain them as much as possible *symmetrically* and in cycles, in order that the mechanical advantages of the *backing* system may be applied to the utmost. It is here interesting to observe the manner in which the value of an *analytical* resource is *met* and *enhanced* by an ingenious *mechanical* contrivance. We see in it an instance of one of those mutual *adjustments* between the purely mathematical and the mechanical departments, mentioned in Note A. as being a main and essential condition of success in the invention of a calculating engine. The nature of the resources afforded by such adjustments would be of two principal kinds. In some cases, a difficulty (perhaps in itself insurmountable) in the one department, would be overcome by facilities in the other; and sometimes (as in the present case) a strong point in the one, would be rendered still stronger and more available, by combination with a corresponding strong point in the other.

As a mere example of the degree to which the combined systems of cycles and of backing can diminish the *number* of cards requisite, we shall choose a case which places it in strong evidence, and which has likewise the advantage of being a perfectly different *kind* of problem from those that are mentioned in any of the other Notes. Suppose it be required to eliminate nine variables from ten simple equations of the form—

$$a x_0 + b x_1 + c x_2 + d x_3 + \ldots\ldots = p \quad (1.)$$
$$a^1 x_0 + b^1 x_1 + c^1 x_2 + d^1 x_3 + \ldots\ldots = p^1 \quad (2.)$$
$$\&c. \qquad \&c. \qquad \&c. \qquad \&c.$$

We should explain, before proceeding, that it is not our object to consider this problem with reference to the actual arrangement of the data on the Variables of the engine, but simply as an abstract question of the *nature* and *number* of the *operations* required to be performed during its complete solution.

The first step would be the elimination of the first unknown quantity x_0 between the two first equations. This would be obtained by the form—

$$(a^1 a - a a^1)x_0 + (a^1 b - a b^1)x_1 + (a^1 c - a c^1)x_2 + $$
$$+ (a^1 d - a d^1)x_3 + \ldots\ldots\ldots\ldots\ldots\ldots = a^1 p - a p^1,$$

for which the operations $10(\times, \times, -)$ would be needed. The second step would be the elimination of x_0 between the second and third equations, for which the operations would be precisely the same. We should then have had altogether the following operations:—

$$10(\times, \times, -), 10(\times, \times, -), = 20(\times, \times, -).$$

Continuing in the same manner, the total number of operations for the complete elimination of x_0 between all the successive pairs of equations, would be—

$$9.10(\times, \times, -) = 90(\times, \times, -).$$

We should then be left with nine simple equations of nine variables from which to eliminate the next variable x_1; for which the total of the processes would be—

$$8.9(\times, \times, -) = 72(\times, \times, -).$$

We should then be left with eight simple equations of eight variables from which to eliminate x_2, for which the processes would be—

$$7.8(\times, \times, -) = 56(\times, \times, -),$$

and so on. The total operations for the elimination of all the variables would thus be—

$$9.10+8.9+7.8+6.7.+5.6+4.5+3.4+2.3+1.2 = 330.$$

So that *three* Operation-cards would perform the office of 330 such cards.

If we take n simple equations containing $n-1$ variables, n being a number unlimited in magnitude, the case becomes still more obvious, as the same three cards might then take the place of thousands or millions of cards.

We shall now draw further attention to the fact, already noticed, of its being by no means necessary that a formula proposed for solution should ever have been actually worked out, as a condition for enabling the engine to solve it. Provided we know the *series of operations* to be gone through, that is sufficient. In the foregoing instance this will be obvious enough on a slight consideration. And it is a circumstance which deserves particular notice, since herein may reside a latent value of such an engine almost incalculable in its possible ultimate results. We already know that there are functions whose numerical value it is of importance for the purposes both of abstract and of practical science to ascertain, but whose determination requires processes so lengthy and so complicated, that, although it is possible to arrive at them through great expenditure of time, labour and money, it is yet on these accounts practically almost unattainable; and we can conceive there being some results which it may be *absolutely impossible* in practice to attain with any accuracy, and whose precise determination it may prove highly important for some of the future wants of science in its manifold, complicated and rapidly-developing fields of inquiry, to arrive at.

Without, however, stepping into the region of conjecture, we will mention a particular problem which occurs to us at this moment as being an apt illustration of the use to which such an engine may be turned for determining that which human brains find it difficult or impossible to work out unerringly. In the solution of the famous problem of the Three Bodies, there are, out of about 295 coefficients of lunar perturbations given by M. Clausen (Astro^e. Nachrichten, No. 406) as the result of the calculations by Burg, of two by Damoiseau, and of one by Burckhardt, fourteen coefficients that differ in the nature of their algebraic sign; and out of the remainder there are only 101 (or about one-third) that agree precisely both in signs and in amount. These discordances, which are generally small in individual magnitude, may arise either from an erroneous determination of the abstract coefficients in the development of the problem, or from discrepancies in the data deduced from observation, or from both causes combined. The former is the most ordinary source of error in astronomical computations, and this the engine would entirely obviate.

We might even invent laws for series or formulæ in an arbitrary manner, and set the engine to work upon them, and thus deduce numerical results which we might not otherwise have thought of obtaining. But this would hardly perhaps in any instance be productive of any great practical utility, or calculated to rank higher than as a kind of philosophical amusement. A. A. L.

Note G.—Page 689.

It is desirable to guard against the possibility of exaggerated ideas that might arise as to the powers of the Analytical Engine. In considering any new subject, there is frequently a tendency, first, to *overrate* what we find to be already interesting or remarkable; and, secondly, by a sort of natural reaction, to *undervalue* the true state of the case, when we do discover that our notions have surpassed those that were really tenable.

The Analytical Engine has no pretensions whatever to *originate* any thing. It can do whatever we *know how to order it* to perform. It can *follow* analysis; but it has no power of *anticipating* any analytical relations or truths. Its province is to assist us in making *available* what we are already acquainted with. This it is calculated to effect primarily and chiefly of course, through its executive faculties; but it is likely to exert an *indirect* and reciprocal influence on science itself in another manner. For, in so distributing and combining the truths and the formulæ of analysis, that they may become most easily and rapidly amenable to the mechanical combinations of the engine, the relations and the nature of many subjects in that science are necessarily thrown into new lights, and more profoundly investigated. This is a decidedly indirect, and a somewhat *speculative*, consequence of such an invention. It is however pretty evident, on general principles, that in devising for mathematical truths a new form in which to record and throw themselves out for actual use, views are likely to be induced, which should again react on the more theoretical phase of the subject. There are in all extensions of human power, or additions to human knowledge, various *collateral* influences, besides the main and primary object attained.

To return to the executive faculties of this engine: the question must arise in every mind, are they *really* even able to *follow* analysis in its whole extent? No reply, entirely satisfactory to all minds, can be given to this query, excepting the actual existence of the engine, and actual experience of its practical results. We will however sum up for each reader's consideration the chief elements with which the engine works:—

1. It performs the four operations of simple arithmetic upon any numbers whatever.

2. By means of certain artifices and arrangements (upon which we cannot enter within the restricted space which such a publication as the present may admit of), there is no limit either to the *magnitude* of the *numbers* used, or to the *number* of *quantities* (either variables or constants) that may be employed.

3. It can combine these numbers and these quantities either algebraically or arithmetically, in relations unlimited as to variety, extent, or complexity.

4. It uses algebraic *signs* according to their proper laws, and developes the logical consequences of these laws.

5. It can arbitrarily substitute any formula for any other; effacing the first from the columns on which it is represented, and making the second appear in its stead.

6. It can provide for singular values. Its power of doing this is referred to in M. Menabrea's memoir, page 685, where he mentions the passage of values through zero and infinity. The practicability of causing it arbitrarily to change its processes at any moment, on the occurrence of any specified contingency (of which its substitution of $(\frac{1}{2}\cos.\overline{n+1}\,\theta + \frac{1}{2}\cos.\overline{n-1}\,\theta)$ for $(\cos.n\theta.\cos.\theta)$ explained in Note E., is in some degree an illustration), at once secures this point.

The subject of integration and of differentiation demands some notice. The engine can effect these processes in either of two ways:—

First. We may order it, by means of the Operation and of the Variable-cards, to go through the various steps by which the required *limit* can be worked out for whatever function is under consideration.

Secondly. It may (if we know the form of the limit for the function in question) effect the integration or differentiation by direct* substitu-

* The engine cannot of course compute limits for perfectly *simple* and *uncompounded* functions, except in this manner. It is obvious that it has no power of representing or of manipulating with any but *finite* increments or decrements; and consequently that wherever the computation of limits (or of any other functions) depends upon the *direct* introduction of quantities which either increase or decrease *indefinitely*, we are absolutely beyond the sphere of its powers. Its nature and arrangements are remarkably adapted for taking into account all *finite* increments or decrements (however small or large), and for developing the true and logical modifications of form or value dependent upon differences of this nature. The engine may indeed be considered as including the whole Calculus of Finite Differences; many of whose theorems would be especially and beautifully fitted for development by its processes, and would offer peculiarly interesting considerations. We may mention, as an example, the calculation of the Numbers of Bernoulli by means of the *Differences of Nothing*.

tion. We remarked in Note B., that any *set* of columns on which numbers are inscribed, represents merely a *general* function of the several quantities, until the special function have been impressed by means of the Operation and Variable-cards. Consequently, if instead of requiring the value of the function, we require that of its integral, or of its differential coefficient, we have merely to order whatever particular combination of the ingredient quantities may constitute that integral or that coefficient. In $a x^n$, for instance, instead of the quantities

$$\underbrace{\begin{array}{ccc} V_0 & V_1 & V_2 \\ \boxed{a} & \boxed{n} & \boxed{x} \end{array}}_{a x^n} \quad \begin{array}{c} V_3 \\ \boxed{a x^n} \end{array}$$

being ordered to appear on V_3 in the combination $a x^n$, they would be ordered to appear in that of

$$a n x^{n-1}.$$

They would then stand thus:—

$$\underbrace{\begin{array}{ccc} V_0 & V_1 & V_2 \\ \boxed{a} & \boxed{n} & \boxed{x} \end{array}}_{a n x^{n-1}} \quad \begin{array}{c} V_3 \\ \boxed{a n x^{n-1}} \end{array}$$

Similarly, we might have $\frac{a}{n} x^{(n+1)}$, the integral of $a x_n$.

An interesting example for following out the processes of the engine would be such a form as

$$\int \frac{x^n \, d x}{\sqrt{a^2 - x^2}},$$

or any other cases of integration by successive reductions, where an integral which contains an operation repeated n times can be made to depend upon another which contains the same $n-1$ or $n-2$ times, and so on until by continued reduction we arrive at a certain *ultimate* form, whose value has then to be determined.

The methods in Arbogast's *Calcul des Dérivations* are peculiarly fitted for the notation and the processes of the engine. Likewise the whole of the Combinatorial Analysis, which consists first in a purely numerical calculation of indices, and secondly in the distribution and combination of the quantities according to laws prescribed by these indices.

We will terminate these Notes by following up in detail the steps through which the engine could compute the Numbers of Bernoulli, this being (in the form in which we shall deduce it) a rather complicated example of its powers. The simplest manner of computing these numbers would be from the direct expansion of

$$\frac{x}{e^x - 1} = \frac{1}{1 + \frac{x}{2} + \frac{x^2}{2.3} + \frac{x^3}{2.3.4} + \&c.} \qquad (1.)$$

which is in fact a particular case of the development of

$$\frac{a + b x + c x^2 + \&c.}{a' + b' x + c' x^2 + \&c.}$$

mentioned in Note E. Or again, we might compute them from the well-known form

$$B_{2n-1} = 2 \cdot \frac{1.2.3 \ldots 2n}{(2\pi)^{2n}} \cdot \left\{ 1 + \frac{1}{2^{2n}} + \frac{1}{3^{2n}} + \ldots \right\} \qquad (2.)$$

or from the form

$$B_{2n-1} = \frac{\pm 2n}{(2^{2n}-1)2^{n-1}} \left\{ \begin{array}{l} \frac{1}{2} n^{2n-1} \\ -(n-1)^{2n-1} \left\{ 1 + \frac{1}{2} \cdot \frac{2n}{1} \right\} \\ +(n-2)^{2n-1} \left\{ 1 + \frac{2n}{1} + \frac{1}{2} \cdot \frac{2n.(2n-1)}{1.2} \right\} \\ -(n-3)^{2n-1} \left[\begin{array}{l} 1 + \frac{2n}{1} + \frac{2n.2n-1}{1.2} + \\ \quad + \frac{1}{2} \cdot \frac{2n.(2n-1).(2n-2)}{1.2.3} \end{array} \right] \\ + \ldots \qquad \ldots \qquad \ldots \qquad \ldots \end{array} \right\} \quad (3.)$$

or from many others. As however our object is not simplicity or facility of computation, but the illustration of the powers of the engine, we prefer selecting the formula below, marked (8.). This is derived in the following manner:—

If in the equation

$$\frac{x}{e^x - 1} = 1 - \frac{x}{2} + B_1 \frac{x^2}{2} + B_3 \frac{x^4}{2.3.4} + B_5 \frac{x^6}{2.3.4.5.6} + \ldots . (4.)$$

(in which B_1, B_3 . . . . , &c. are the Numbers of Bernoulli), we expand the denominator of the first side in powers of x, and then divide both numerator and denominator by x, we shall derive

$$1 = \left(1 - \frac{x}{2} + B_1 \frac{x^2}{2} + B_3 \frac{x^4}{2.3.4} + \ldots\right) \left(1 + \frac{x}{2} + \frac{x^2}{2.3} + \frac{x^3}{2.3.4} \ldots\right) (5.)$$

If this latter multiplication be actually performed, we shall have a series of the general form

$$1 + D_1 x + D_2 x^2 + D_3 x^3 + \ldots\ldots\ldots\ldots\ldots\ldots (6.)$$

in which we see, first, that all the coefficients of the powers of x are severally equal to zero; and secondly, that the general form for D_{2n} the co-efficient of the $2n+1$th *term*, (that is of x^{2n} any *even* power of x), is the following:—

$$\left. \begin{array}{l} \frac{1}{2.3 \ldots 2n+1} - \frac{1}{2} \cdot \frac{1}{2.3 \ldots 2n} + \frac{B_1}{2} \cdot \frac{1}{2.3 \ldots 2n-1} + \frac{B_3}{2.3.4} \cdot \frac{1}{2.3 \ldots 2n-3} + \\ \quad + \frac{B_5}{2.3.4.5.6} \cdot \frac{1}{2.3 \ldots 2n-5} + \ldots + \frac{B_{2n-1}}{2.3 \ldots 2n} \cdot 1 = 0 \end{array} \right\} (7.)$$

Multiplying every term by $(2.3\ldots 2n)$, we have

$$\left.\begin{array}{l} 0=-\frac{1}{2}\cdot\frac{2n-1}{2n+1}+B_1\left(\frac{2n}{2}\right)+B_3\left(\frac{2n.2n-1.2n-2}{2.3.4}\right)+ \\ \quad +B_5\left(\frac{2n.2n-1\ldots\ldots\ldots 2n-4}{2.3.4.5.6}\right)+\ldots\ldots+B_{2n-1} \end{array}\right\} \text{(8.)}$$

which it may be convenient to write under the general form:—

$$0=A_0+A_1B_1+A_3B_3+A_5B_5+\ldots+B_{2n-1}\ldots\ldots\ldots\text{(9.)}$$

A_1, A_3, &c. being those functions of n which respectively belong to B_1, B_3, &c.

We might have derived a form nearly similar to (8.), from D_{2n-1} the coefficient of any *odd* power of x in (6.); but the general form is a little different for the coefficients of the *odd* powers, and not quite so convenient.

On examining (7.) and (8.), we perceive that, when these formulæ are isolated from (6.) whence they are derived, and considered in themselves separately and independently, n may be any whole number whatever; although when (7.) occurs *as one of the* D's in (6.), it is obvious that n is then not arbitrary, but is always a certain function of the *distance of that* D *from the beginning*. If that distance be $=d$, then

$$2n+1=d, \text{ and } n=\frac{d-1}{2} \text{ (for any } even \text{ power of } x.)$$

$$2n=d, \text{ and } n=\frac{d}{2} \text{ (for any } odd \text{ power of } x.)$$

It is with the *independent* formula (8.) that we have to do. Therefore it must be remembered that the conditions for the value of n are now modified, and that n is a perfectly *arbitrary* whole number. This circumstance, combined with the fact (which we may easily perceive) that whatever n is, every term of (8.) after the $(n+1)$th is $=0$, and that the $(n+1)$th term itself is always $=B_{2n-1}\cdot\frac{1}{1}=B_{2n-1}$, enables us to find the value (either numerical or algebraical) of any nth Number of Bernoulli B_{2n-1}, *in terms of all the preceding ones*, if we but know the values of $B_1, B_3\ldots\ldots B_{2n-3}$. We append to this Note a Diagram and Table, containing the details of the computation for B_7, (B_1, B_3, B_5 being supposed given).

On attentively considering (8.), we shall likewise perceive that we may derive from it the numerical value of *every* Number of Bernoulli in succession, from the very beginning, *ad infinitum*, by the following series of computations:—

1st Series.—Let $n=1$, and calculate (8.) for this value of n. The result is B_1.

2nd Series.—Let $n=2$. Calculate (8.) for this value of n, substituting the value of B_1 just obtained. The result is B_3.

3rd Series.—Let $n=3$. Calculate (8.) for this value of n, substituting the values of B_1, B_3 before obtained. The result is B_5. And so on, to any extent.

The diagram* represents the columns of the engine when just prepared

* See the diagram at the end of these Notes.

for computing B_{2n-1}, (in the case of $n=4$); while the table beneath them presents a complete simultaneous view of all the successive changes which these columns then severally pass through in order to perform the computation. (The reader is referred to Note D, for explanations respecting the nature and notation of such tables.)

Six numerical *data* are in this case necessary for making the requisite combinations. These data are 1, 2, n $(=4)$, B_1, B_3, B_5. Were $n=5$, the additional datum B_7 would be needed. Were $n=6$, the datum B_9 would be needed; and so on. Thus the actual *number of data* needed will always be $n+2$, for $n=n$; and out of these $n+2$ data, $(\overline{n+2}-3)$ of them are successive Numbers of Bernoulli. The reason why the Bernoulli Numbers used as data, are nevertheless placed on *Result*-columns in the diagram, is because they may properly be supposed to have been previously computed in succession by the *engine* itself; under which circumstances each B will appear as a *result*, previous to being used as a *datum* for computing the succeeding B. Here then is an instance (of the kind alluded to in Note D.) of the same Variables filling more than one office in turn. It is true that if we consider our computation of B_7 as a perfectly *isolated* calculation, we may conclude B_1, B_3, B_5 to have been arbitrarily placed on the columns; and it would then perhaps be more consistent to put them on V_4, V_5, V_6 as data and not results. But we are not taking this view. On the contrary, we suppose the engine to be *in the course of* computing the Numbers to an indefinite extent, from the very beginning; and that we merely single out, by way of example, *one amongst* the successive but distinct series' of computations it is thus performing. Where the B's are fractional, it must be understood that they are computed and appear in the notation of *decimal* fractions. Indeed this is a circumstance that should be noticed with reference to all calculations. In any of the examples already given in the translation and in the Notes, some of the *data*, or of the temporary or permanent results, might be fractional, quite as probably as whole numbers. But the arrangements are so made, that the nature of the processes would be the same as for whole numbers.

In the above table and diagram we are not considering the *signs* of any of the B's, merely their numerical magnitude. The engine would bring out the sign for each of them correctly of course, but we cannot enter on *every* additional detail of this kind, as we might wish to do. The circles for the signs are therefore intentionally left blank in the diagram.

Operation-cards 1, 2, 3, 4, 5, 6 prepare $-\frac{1}{2}\cdot\frac{2n-1}{2n+1}$. Thus, Card 1 multiplies *two* into n, and the three *Receiving* Variable-cards belonging respectively to V_4, V_5, V_6, allow the result $2n$ to be placed on each of these latter columns (this being a case in which a triple receipt of the result is needed for subsequent purposes); we see that the upper indices of the two Variables used, during Operation 1, remain unaltered.

We shall not go through the details of every operation singly, since the table and diagram sufficiently indicate them; we shall merely notice some few peculiar cases.

By Operation 6, a *positive* quantity is turned into a *negative* quantity, by simply subtracting the quantity from a column which has only zero upon it. (The sign at the top of V_8 would become — during this process.)

Operation 7 will be unintelligible, unless it be remembered that if we were calculating for $n = 1$ instead of $n = 4$, Operation 6 would have completed the computation of B_1 itself; in which case the engine, instead of continuing its processes, would have to put B_1 on V_{21}; and then either to stop altogether, or to begin Operations 1, 2....7 all over again for value of n ($= 2$), in order to enter on the computation of B_3; (having however taken care, previous to this recommencement, to make the number on V_3 equal to *two*, by the addition of unity to the former $n = 1$ on that column). Now Operation 7 must either bring out a result equal to zero (if $n = 1$); or a result *greater* than *zero*, as in the present case; and the engine follows the one or the other of the two courses just explained, contingently on the one or the other result of Operation 7. In order fully to perceive the necessity of this *experimental* operation, it is important to keep in mind what was pointed out, that we are not treating a perfectly isolated and independent computation, but one out of a series of antecedent and prospective computations.

Cards 8, 9, 10 produce $-\frac{1}{2}\cdot\frac{2n-1}{2n+1} + B_1\frac{2n}{2}$. In Operation 9 we see an example of an upper index which again becomes a value after having passed from preceding values to zero. V_{11} has sucessively been ${}^0V_{11}$, ${}^1V_{11}$, ${}^2V_{11}$, ${}^0V_{11}$, ${}^3V_{11}$; and, from the nature of the office which V_{11} performs in the calculation, its index will continue to go through further changes of the same description, which, if examined, will be found to be regular and periodic.

Card 12 has to perform the same office as Card 7 did in the preceding section; since, if n had been $= 2$, the 11th operation would have completed the computation of B_3.

Cards 13 to 20 make A_3. Since A_{2n-1} always consists of $2n - 1$ factors, A_3 has three factors; and it will be seen that Cards 13, 14, 15, 16 make the second of these factors, and then multiply it with the first; and that 17, 18, 19, 20 make the third factor, and then multiply this with the product of the two former factors.

Card 23 has the office of Cards 11 and 7 to perform, since if n were $= 3$, the 21st and 22nd operations would complete the computation of B_5. As our case is B_7, the computation will continue one more stage; and we must now direct attention to the fact, that in order to compute A_7 it is merely necessary precisely to repeat the group of Operations 13 to 20; and then, in order to complete the computation of B_7, to repeat Operations 21, 22.

It will be perceived that every unit added to n in B_{2n-1}, entails an additional repetition of operations (13...23) for the computation of B_{2n-1}. Not only are all the *operations* precisely the same however for every such repetition, but they require to be respectively supplied with numbers from the very *same pairs of columns*; with only the one exception of Operation 21, which will of course need B_5 (from V_{23}) instead of B_3 (from V_{22}). This identity in the *columns* which supply the requisite numbers, must not be confounded with identity in the *values* those columns have upon them and give out to the mill. Most of those values undergo alterations during a performance of the operations (13 . . . . 23), and consequently the columns present a new set of values for the *next* performance of (13 . . . . 23) to work on.

At the termination of the *repetition* of operations (13...23) in computing B_7, the alterations in the values on the Variables are, that

$V_6 = 2n - 4$ instead of $2n - 2$.
$V_7 = 6$ . . . . 4.
$V_{10} = 0$ . . . . 1.
$V_{13} = A_0 + A_1 B_1 + A_3 B_3 + A_5 B_5$ instead of $A_0 + A_1 B_1 + A_3 B_3$

In this state the only remaining processes are first: to transfer the value which is on V_{13}, to V_{24}; and secondly to reduce V_6, V_7, V_{13} to zero, and to add * *one* to V_3, in order that the engine may be ready to commence computing B_9. Operations 24 and 25 accomplish these purposes. It may be thought anomalous that Operation 25 is represented as leaving the upper index of V_3 still = unity. But it must be remembered that these indices always begin anew for a separate calculation, and that Operation 25 places upon V_3 the *first* value *for the new calculation*.

It should be remarked, that when the group (13...23) is *repeated*, changes occur in some of the *upper* indices during the course of the repetition: for example, ${}^3V_6$ would become ${}^4V_6$ and ${}^5V_6$.

We thus see that when $n = 1$, nine Operation-cards are used; that when $n = 2$, fourteen Operation-cards are used; and that when $n > 2$, twenty-five Operation-cards are used; but that no *more* are needed, however great n may be; and not only this, but that these same twenty-five cards suffice for the successive computation of all the Numbers from B_1 to B_{2n-1} inclusive. With respect to the number of *Variable*-cards, it will be remembered, from the explanations in previous Notes, that an average of three such cards to each *operation* (not however to each Operation-*card*) is the estimate. According to this the computation of B_1 will require twenty-seven Variable-cards; B_3 forty-two such cards; B_5 seventy-five; and for every succeeding B after B_5, there would be thirty-three additional Variable-cards (since each repetition of the group (13...23) adds eleven to the number of operations required for computing the previous B). But we must now explain, that whenever there is a *cycle of operations*, and if these merely require to be supplied with numbers from the *same pairs of columns* and likewise each operation to place its *result* on the *same* column for every repetition of the whole group, the process then admits of a *cycle of Variable-cards* for effecting its purposes. There is obviously much more symmetry and simplicity in the arrangements, when cases do admit of repeating the Variable as well as the Operation-cards. Our present example is of this nature. The only exception to a *perfect identity* in *all* the processes and columns used, for every repetition of Operations (13...23) is, that Operation 21 always requires one of its factors from a new column, and Operation 24 always puts its result

* It is interesting to observe, that so complicated a case as this calculation of the Bernoullian Numbers, nevertheless, presents a remarkable simplicity in one respect; viz., that during the processes for the computation of *millions* of these Numbers, no other arbitrary modification would be requisite in the arrangements, excepting the above simple and uniform provision for causing one of the data periodically to receive the finite increment unity.

on a new column. But as these variations follow the same law at each repetition, (Operation 21 always requiring its factor from a column *one* in advance of that which it used the previous time, and Operation 24 always putting its result on the column *one* in advance of that which received the previous result), they are easily provided for in arranging the recurring group (or cycle) of Variable-cards.

We may here remark that the average estimate of three Variable-cards coming into use to each operation, is not to be taken as an absolutely and literally correct amount for all cases and circumstances. Many special circumstances, either in the nature of a problem, or in the arrangements of the engine under certain contingencies, influence and modify this average to a greater or less extent. But it is a very safe and correct *general* rule to go upon. In the preceding case it will give us seventy-five Variable-cards as the total number which will be necessary for computing any B after B_3. This is very nearly the precise amount really used, but we cannot here enter into the minutiæ of the few particular circumstances which occur in this example (as indeed at some one stage or other of probably most computations) to modify slightly this number.

It will be obvious that the very *same* seventy-five Variable-cards may be repeated for the computation of every succeeding Number, just on the same principle as admits of the repetition of the thirty-three Variable-cards of Operations (13...23) in the computation of any *one* Number. Thus there will be a *cycle of a cycle* of Variable-cards.

If we now apply the notation for cycles, as explained in Note E, we may express the operations for computing the Numbers of Bernoulli in the following manner:—

$$(1...7), (24, 25) \ldots\ldots\ldots\ldots \text{ gives } B_1 = \text{1st number}; (n \text{ being} = 1).$$
$$(1...7), (8...12), (24, 25) \ldots\ldots\ldots\ldots B_3 = \text{2nd} \ldots\ldots; (n \ldots\ldots = 2).$$
$$(1...7), (8...12), (13...23), (24, 25) \ldots\ldots\ldots B_5 = \text{3rd} \ldots\ldots; (n \ldots\ldots = 3).$$
$$(1...7), (8...12), 2(13...23), (24, 25) \ldots\ldots\ldots B_7 = \text{4th} \ldots\ldots; (n \ldots\ldots = 4).$$

....

....

$$(1...7), (8...12), \sum (+1)^{n-2} (13...23), (24, 25) \ldots B_{2n-1} = n\text{th} \ldots\ldots; (n \ldots\ldots = n).$$

Again,

$$(1...7), (24, 25), \underset{\text{limits 1 to } n}{\sum (+1)^n} \left\{ (1...7), (8...12), \underset{\text{limits 0 to } (n+2)}{\sum (n+2)} (13...23), (24, 25) \right\}$$

represents the total operations for computing every number in succession, from B_1 to B_{2n-1} inclusive.

In this formula we see a *varying cycle* of the *first* order, and an ordinary cycle of the *second* order. The latter cycle in this case includes in it the varying cycle.

On inspecting the ten Working-Variables of the diagram, it will be perceived, that although the *value* on any one of them (excepting V_4 and V_5) goes through a series of changes, the *office* which each performs is in this calculation *fixed* and *invariable*. Thus V_6 always prepares the *numerators* of the factors of any A; V_7 the *denominators*. V_8 always receives the $(2n-3)$th factor of A_{2n-1}, and V_9 the $(2n-1)$th. V_{10} always decides which of two courses the succeeding processes are to follow, by feeling for the value of n through ge 722 et seq.)

Number of Operations			Result Variables.			
	$^{0}V_{12}$ ○ 0 0 0	$^{0}V_{13}$ ○ 0 0 0	$^{1}V_{21}$ ○ B_1 in a decimal fraction.	$^{1}V_{22}$ ○ B_3 in a decimal fraction.	$^{1}V_{23}$ ○ B_5 in a decimal fraction.	$^{0}V_{24}$... ○ 0 0 0
			$\boxed{B_1}$	$\boxed{B_3}$	$\boxed{B_5}$	$\boxed{B_7}$
1						
2						
3						
4						
5						
6	....	$-\frac{1}{2}\cdot\frac{2n-1}{2n+1}=A_0$				
7						
8						
9						
10	$B_1\cdot\frac{2n}{2}=B_1A_1$	....	B_1			
11	0	$\left\{-\frac{1}{2}\cdot\frac{2n-1}{2n+1}+B_1\cdot\frac{2n}{2}\right\}$				
12						
13						
14						
15						
16						
17						
18						
19						
20						
21	B_3A_3	....	......	B_3		
22	0	$\left\{A_3+B_1A_1+B_3A_3\right\}$				
23						
24	....	....	......	......	......	B_7
25						

means of a subtraction; and so on; but we shall not enumerate further. It is desirable in all calculations, so to arrange the processes, that the *offices* performed by the Variables may be as uniform and fixed as possible.

Supposing that it was desired not only to tabulate B_1, B_3, &c., but A_0, A_1, &c.; we have only then to appoint another series of Variables, V_{41}, V_{42}, &c., for receiving these latter results as they are successively produced upon V_{11}. Or again, we may, instead of this, or in addition to this second series of results, wish to tabulate the value of each successive *total* term of the series (8), viz: A_0, $A_1 B_1$, $A_3 B_3$, &c. We have then merely to multiply each B with each corresponding A, as produced; and to place these successive products on Result-columns appointed for the purpose.

The formula (8.) is interesting in another point of view. It is one particular case of the general Integral of the following Equation of Mixed Differences:—

$$\frac{d^2}{dx^2}\left(z_{n+1}\, x^{2n+2}\right) = (2n+1)(2n+2)\, z^n x^{2n}$$

for certain special suppositions respecting z, x and n.

The *general* integral itself is of the form,

$$z_n = f(n) \cdot x + f_1(n) + f_2(n) \cdot x^{-1} + f_3(n) \cdot x^{-3} + \ldots$$

and it is worthy of remark, that the engine might (in a manner more or less similar to the preceding) calculate the value of this formula upon most *other* hypotheses for the functions in the integral, with as much, or (in many cases) with more, ease than it can formula (8.).

A. L. L.

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PRINTED BY RICHARD AND JOHN E. TAYLOR,
RED LION COURT, FLEET STREET.