Do Good Research!

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I got lucky

Increase impact

Write good papers

Distribute code

How much maintenance?

Inspiration vs perspiration (Edison)

We all can do very good research. maybe not genius, but excellent.

"There's a ruinous misconception that a Ph.D. must be smart.This can't be true. A smart person would know better than to get a Ph.D." http://matt.might.net/articles/successful-phd-students/

We can develop our skills and aim our effort to do better research

There are many styles of research. Find yours.

Nail vs. hammer driven (problem vs. tool)

Narrow vs. broad

Individual vs. collaborative

Trailblazer vs. closer

Interdisciplinary

Know your strengths, cultivate them

breadth, implementation, creativity, polyglot , theory, application

Answers are easy, questions are hard

Half of the work is to figure out a good question

Be curious. challenge the status quo. What is important? What is a problem? What are blind spots?

Hamming: What is important in your field? Are you working on it?

If it's not working, maybe change the question

Yair Weiss as reported by Bill Freeman:

"The strong student starts doing what the advisor has asked, sees that it doesn't work, looks around within some epsilon ball of the original proposal to find what does work, and reports that solution."

Aim high

What is a good question?

Important (solving it has important consequences)

Important to others

Solvable, you have an angle of attack

Assessable (can measure success, ideally quantitatively)

Still relevant in 5-10 years

Exciting to you

Nice pluses:

- *not too crowded*
- *has nice paper-sized sub-problems*

Finding & exploring good questions

Keep a list of questions & half baked questions

Talk to other people

Keep some free time to just think broadly

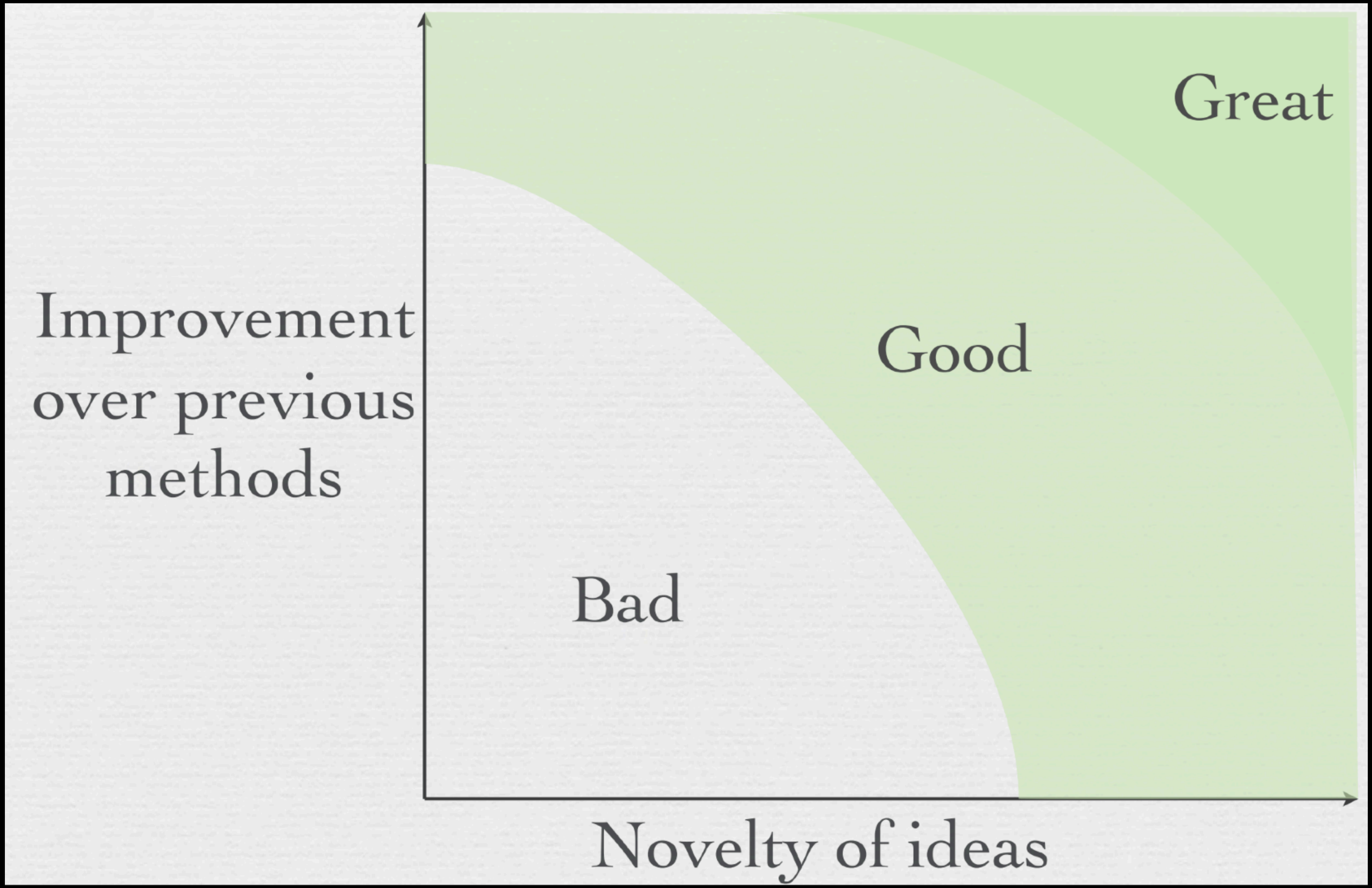
Once in a while, wonder if you're asking the right question (e.g. error control)

Constantly reflect on the scope of projects. Sometimes needs to be narrowed down, sometimes broadened up

e.g. deep demosaicking

Novelty vs. results

See also Pasteur quadrants
Hardest case: novel answer to old questions



Dealing with uncertainty

"Great scientists tolerate ambiguity very well. They believe the theory enough to go ahead; they doubt it enough to notice the errors and faults so they can step forward and create the new replacement theory." Hamming

In research, you often don't even know the question!

Be able to build on unknown territory. Assume some sub-problems will be solved and build on hypothetical solutions. At the same time anticipate problems.

Good quality: sloppy math, story telling, hand waving

Focus on the big picture story, ignore unsolved details

Try to fail quickly. Avoid sunk cost fallacy.

Make mistakes. We all do.

Research is multiscale, leverage abstraction

Understand and explain why it works

before, during, after

toy problems are useful

and why it doesn't work

Your work will never be perfect

Don’t let the better be the enemy of the good.

Make hard decisions. get things done. Narrow the scope. Simplify the problem.

Assess costs/benefits.

Deadlines can help, just don’t let them dictate everything

Research can be lonely, discouraging, tedious

We all go through it.

Be persistent.

Be passionate about your topic.

But research is also extremely rewarding

creating / discovering ideas that did not exist before

intellectual freedom

working with smart people

learning new things

Believe in yourself

Have ambition — try greater things

Don’t get stuck thinking you’ll fail

Assume things will get solved somehow, build on quicksand

Have great collaborators

Peers, mentors, underlings

Internal or external to your field

Don't be threatened by people who are smarter than you

Communication is half the job

Communication is critical (oral, written, informal)

big picture.

contextualize. what does you audience/readers know? hold their hand.

Explaining the question is half the job

Focus on why, not how

How much time you spend solving something doesn't necessarily translate in communication time

Communicate early and often (get feedback, collaborate.)

What you do vs. what you achieve

what you have achieved vs. what you contribute (in both directions: some tedious stuff is not as important as you think, but demos are important).

Demos matter

Read lots of papers

Acquire background, tools

Understand the state of the art and what is missing

Develop a taste in communication/writing

Be critical. does it work? why does it work? is it well written?

Manage your time

Don’t procrastinate

Keep time for thinking

Keep time for reading

make hard decisions

let deadlines motivate you/structure your time but don’t be a slave to them

important vs urgent. Focus on important & not urgent.

To do lists, cards for the day

http://www.dgp.toronto.edu/~hertzman/courses/gradSkills/2010/TimeManagement.pdf

Compare, demonstrate

What is the right comparison?

Some pointers

http://people.csail.mit.edu/billf/www/papers/doresearch.pdf

http://www.cs.virginia.edu/~robins/YouAndYourResearch.html

http://people.csail.mit.edu/fredo/student.html

http://people.csail.mit.edu/billf/talks/10minFreeman2013.pdf

http://www.csee.umbc.edu/~mariedj/papers/advice.pdf

http://www.dgp.toronto.edu/~hertzman/courses/gradSkills/2010/

In the end, what matters

Questions

Answers a little bit

Communications

Bila

clarity, code

Coded aperture

believe

saliency

understanding & evaluating deconvolution

Experimental Analysis of BRDF Models.

Eulerian video magnification for revealing subtle changes in the world

Billboard clouds for extreme model simplification

Procedural modeling of structurally-sound masonry buildings