



# HOW TO REGOGNIZE & DO GOOD RESEARCH



Many slides today are borrowed and modified from Feamster&Gray. Thanks Nick!

1.

# *RECOGNIZING GOOD RESEARCH*



## HOW TO EVALUATE A PAPER?

- X Is it solving an important problem?
- X Is the solution sound and evaluated with rigor?
- X What are the original contributions? How does it advance the state-of-the-art?
- X What is the scientific (and societal) impact?
  
- X We should also consider when it was published and how it affected the community then (and since then)
- X More from the “How to Review a Paper” class down the road



## (SOME) CHARACTERISTICS OF GOOD RESEARCH

- X The problem is difficult
- X Solving the problem creates new knowledge
- X The problem changes conventional thinking
- X The problem has a solution

## THE PROBLEM IS DIFFICULT

- X Easy problems could be tackled by most people
- X The problem should be important and hard
- X The research question must be articulated clearly
  - What are the challenges in solving this problem?



## SOLVING THE PROBLEM CREATES NEW KNOWLEDGE

- X Recognizing the difference between research and a simple matter of “engineering” is important
- X Many problems are difficult, but if an army of programmers could solve the problem with what we know today, it's probably not research

## THE PROBLEM CHANGES CONVENTIONAL THINKING

- X While there is value in confirming conventional wisdom, great research typically offers surprising results, creates a new way of thinking about or approaching problems



## THE PROBLEM HAS A SOLUTION

- X Not every paper needs to solve a problem completely (lots of papers discuss “future work”)
- X Still, while posing a good question is paramount, the solution or finding presented in a paper should represent a significant advance beyond the previous body of knowledge



## MEASURES OF IMPACT

- X Publication, citation count, etc.
- X Best paper award vs test-of-time award
- X Technical transfer
- X Open-sourcing

2.

## DOING GOOD RESEARCH

# HOW CAN I DO GOOD RESEARCH?

- X Problem finding
- X How to be a “great” researcher



## AVOIDING THE STEAMROLLER

- X Work in areas where industry does not have the resources to devote
  - Industry is driven by bottom lines (\$)
  - New, out-of-the-box thinking may take away from exactly what the customer is asking for
  
- X Our advantage: We can propose innovative ideas that are unencumbered by current constraints



## FINDING THE RIGHT RESEARCH PROBLEM

One of the most important and challenging things a researcher must do

Again, there is no “right” answer to this; depends on your taste

Sometimes a good research problem needs time for others to recognize



# FINDING THE RIGHT PROBLEMS

Identifying the right problems to address is usually the hardest part of research. What do you recommend to young researchers?

“My main advice is to ***avoid incremental work***. You should be aware of what others have done and take advantage of it. But rather than thinking of small ways to improve it, try to think of how to solve the problem differently, or how to apply the technique to a different area, or how to abstract from what has been done to come up with something that’s broadly applicable.”



-- Barbara Liskov on Programming, Career, and the Future, *IEEE DS Online*



## FINDING PROBLEMS

- X Hop on a trend
- X Find a nail that fits your hammer
- X Revisit old problems (with new perspective)
- X Making life easier
  - Pain points
  - Wish lists
- X “\*-ations”
  - Generalization
  - Specialization
  - Automation
- X Work on cross-disciplinary research

## HOP ON A TREND

X Need places to discover trends

X Funding agencies

- Funded proposals
- Calls for proposals

X Conference calls for papers

X Industry/technology trends

## FINDING A NAIL FOR YOUR HAMMER

- X Become an expert at something
  - You'll become valuable to a lot of people
- X Develop a system that sets you ahead of the pack
- X Apply your “secret weapon” to one or more problem areas
  - Algorithm
  - System
  - Expertise

## REVISITING PROBLEMS

- X Previous solutions may have assumed certain problem constraints
  
- X What has changed since the problem was “solved”?
  - Processing power
  - Cost of memory
  - New protocols
  - New applications
  - ...

## PAIN POINTS

- X Look to industry, other researchers, etc. for problems that recur
- X In programming, if you have to do something more than a few times, script!
- X In research, if the same problem is recurring and solved the same silly way, there may be a better way...



## WISH LISTS

- X What systems do you wish you had that would make your life easier?
  - Less spam?
  - Faster file transfer, automatic file sync?
  - ...
  
- X What questions would you like to know the answer to?
  - Chances are there is data out there to help you find the answer...

# GENERALIZE FROM SPECIFIC PROBLEMS

- X Previous work may outline many points in the design space
- X There may be a general algorithm, system, framework, etc., that solves a large class of problems instead of going after “point solutions”

## SPECIALIZE A GENERAL PROBLEM

- X Finding general problems
  - Look for general “problem areas”
  - Look for taxonomies and surveys that lay out a problem space
  
- X Applying constraints to the problem in different ways may yield a new class of problems
  - Example: Routing (in wireless, sensor networks, wired, delay-tolerant networks, etc.)

# AUTOMATION

- X Some existing problems, tasks, etc. are manual and painful
  - Automation could make a huge difference
  - It's also often very difficult because it requires complex reasoning
  
- X Related to pain points

# THE INTERSECTION: THE BEST CHANCE TO INNOVATE

- X Where ideas from one area meet another
- X Research areas can become mired in a set way of thinking
- X Working at *intersections* permits
  - Leaps in new directions
  - Opening up new fields
  - Creation of niches

## METHODS FOR FINDING INTERSECTIONS

- X Break down barriers: *Reverse/drop assumptions* and see if the problem is still solvable
- X Try brainstorming: apply random combination of ideas
- X Interact with diverse groups of people





# HOW TO BECOME A GREAT RESEARCHER



## WE MEAN *GREAT*

- X We are talking about what's needed to be the equivalent of an Olympic athlete, in research
- X Even if you don't attain a high goal – striving for it will take you much farther
- X If you're here to just survive... please leave!

# A (CARTOON) MATHEMATICAL THEORY OF RESEARCH

$$\text{Greatness} = 1 / \text{Prob}(\text{Significance})$$

- X The Greatness of a result is inversely proportional to how common results of its Significance are (think: a “one-in-a-million” 100-yard-dash time; Olympic-level times are Great)
- X Significance is the size of the advance, or deviation from the norm, or the current state of the art

# GRAY'S FIRST LAW OF RESEARCH

$$\text{Significance} = \text{Significance}(\text{problem}) \times \text{Significance}(\text{solution})$$

- X Significance(problem) = importance; Significance(solution) = effectiveness
- X We are all well-trained in solution methods... but not in thinking about what problem to solve
- X Major implication: good problem selection is where you can make the biggest difference

# WHAT ARE THE ELEMENTS OF SIGNIFICANT RESULTS?

(WHETHER IN PROBLEM SPACE OR SOLUTION SPACE)

Significance  $\alpha$  Ability

Ability = Brains

Right?

- X Let's think of Einstein – he was simply *really* smart, right?
- X Okay, yes, but let's also somehow account for the amount of work he put into the problem

# WORK

$$\text{Significance} = \text{LeapSize}^{\text{NumLeaps}}$$

$$\text{LeapSize} \propto \text{Ability} \propto \text{Brains}$$

$$\text{NumLeaps} \propto \text{TimePutIntoProblem}$$

$$\text{So: Significance} \propto \text{Brains}^{\text{TimePutIntoProblem}}$$

X We assume your Ability > 1 ☺

X Note the exponential importance of time, or work, put into the problem



# KNOWLEDGE AND SKILL

$$\text{Ability} \propto \text{Brains} \times \text{Knowledge} \times \text{Skill}$$

- X Brains = raw CPU speed
- X Knowledge = what's in textbooks and papers
- X Skill = techniques, strategies, experience for solving problems of the relevant type
- X Note that it is clearly about more than just Brains

# KNOWLEDGE AND SKILL

Knowledge  $\propto$  TimeGainingKnowledge  
Skill  $\propto$  TimeGainingSkill

- X Note that Time enters again
- X TimeGainingKnowledge = classes, independent reading
- X TimeGainingSkills = classes, working on many projects of the relevant type
- X TotalTime = TimeGainingKnowledge +  
TimeGainingSkill +  
TimePutIntoProblem
- X Hmm, there seems to be a lot of Time needed..

# YOU WILL NEED LOTS OF KNOWLEDGE AND SKILL, DEEP AND BROAD

- X Problems in many areas
- X Subtleties of those problems
- X Solutions (theories and concepts) in many areas
- X Subtleties of those solutions
- X A mental map of what's known/resolved and what's unknown/unresolved
- X Experience applying various solutions to various problems

# NOVELTY

LeapSize  $\propto$  Ability  $\times$   
Novelty

Novelty  $\propto$  Creativity

- X Hmm, Creativity is an in-born thing, right?
- X Not completely: creative thinking is a skill as well

# TALKING TO PEOPLE

Knowledge  $\propto$  Social/Communication Effort

- X Where is Knowledge? Much of it is *not* in books
- X Unwritten knowledge is distributed over many people
- X Access to data and resources is often equivalent to knowledge

# NOVELTY

$$\text{Novelty} \propto \frac{\text{Creativity} \times \text{NumConnections}}{\text{NumConnections}}$$

$$\begin{aligned} \text{NumConnections} &\propto \text{Knowledge}_1 \times \text{Knowledge}_2 \times \dots \\ &\propto \text{Knowledge}^{\text{NumAreas}} \end{aligned}$$

- X Suppose now you have an average amount of Knowledge, in multiple areas
- X Thus: knowing multiple areas well can result in bigger payoff than knowing more in one

# TEAMS

$$\text{Novelty} \propto \text{Creativity} \times \text{NumConnections}$$

$$\begin{aligned} \text{NumConnections} &\propto \text{Knowledge}_1 \times \text{Knowledge}_2 \times \dots \\ &\propto \text{Knowledge}^{\text{NumAreas}} \end{aligned}$$

- X Knowledge of multiple areas can also be achieved through a team of multiple brains
- X Note, however, that many deep cross-area connections can only be made within a single brain



# GUTS

$$\text{LeapSize} \propto \text{Ability} \times \text{Novelty} \times \text{Guts}$$

- X The more distant the connection or radical the approach, the bigger the leap; but this takes Guts to pursue
- X Guts = courage / self-confidence / insanity

# GUTS

$$\text{LeapSize} \propto \text{Ability} \times \text{Novelty} \times \text{Guts}$$

- X It takes courage to pursue things which are different from what everyone else is pursuing
- X Be unafraid to be alone
- X Be unafraid to be wrong
- X Have initiative – don't wait for 'validation from the literature' first

# BACK TO GREATNESS

$$\text{Greatness} \propto 1 / \text{Prob}(\text{Significance})$$

$$\text{Greatness} \propto e^{\text{Significance}^2}$$

- X Under a Gaussian model... (ahem)
- X We see that Greatness is exponential in the Significance, which means it might not be as hard as you think to achieve some Greatness

# GRAY'S SECOND LAW OF RESEARCH

$$\log(\text{Greatness}) \propto C^2 \times \text{TimePutIntoProblem}$$
$$C \propto \text{Brains} \times \text{Guts} \times \text{Creativity} \times \text{Skill} \times \text{Knowledge/Social}^{\text{NumAreas} + 1}$$

- X These are the elements of Greatness in research
- X Note the things in exponents: amount of work you put in, breadth of your knowledge
- X These Laws imply certain things about your *way of working*, and your entire *lifestyle*



## WHAT ABOUT *ME*?

X Can I do significant research?

X If so, what do I have to do?

# TALENT

People who do significant research...

- X Have at least average Talent (Brains, Guts, and Creativity) or potential Talent
  - If you are dumb, very timid, or completely uncreative... this is not for you 😞
  - But: remember that Skill (at thinking, being courageous, and creating) can supplant your natural inclinations
- X Are represented by all ages and backgrounds

# INQUISITIVENESS

People who do significant research...

X Are curious about many things

X Poke into non-standard places

- Read about things most others are not reading about
- Talk to people with real, dirty, unformalized problems

X “Cross-train” outside of their main area



## LEARNING AND KNOWLEDGE

People who do significant research...

- X Are good at obtaining knowledge and skill
  - Can learn on their own, outside of classes and textbooks
  - Are not intimidated by the initial impenetrability of a topic
  - Are not shy about talking to others to learn from and team with them
- X Become good at whatever they need to become good at

## LEARNING AND KNOWLEDGE

People who do significant research...

X Know a lot (as a result of the above) in general, broadly and deeply

X Have a good picture of:

- What is known and what is *unknown*
- What the real *weaknesses* of approaches are
- Exactly *why* some things seemed to work
- What are at the *boundaries* of the field

# SKEPTICISM

People who do significant research...

- X Maintain doubt whenever learning or seeing anything
- X Avoid mental traps like trends and blind adherence to the leaders
- X Can think clearly in the presence of “noise” from many incorrect or subtly incorrect voices

## FRUITFUL DIRECTION

People who do significant research...

### X Work on the right thing

- Are good at good-problem-finding as well as good-solution-finding
- Identify the true objective and don't allow themselves to deviate from it

### X Entertain big thoughts

- Don't *ever* work on unimportant problems – there are too many important problems
- Revisit simple questions, which lie at the beginnings of all fields

# GUTS

People who do significant research...

- X Are not afraid to buck the field, ignore authority
  - Are prepared to be wrong
  - Are prepared to be alone
  - Have initiative – don't wait for “validation from the literature” first
- X Note: This is initially scary; but right always wins (and is rewarded) in the end!
- X Also: Your credibility in “normal science” helps you when you become a revolutionary

## WORK AND PRODUCTIVITY

People who do significant research...

- X Work a lot: every day, every week – think of compounding interest
- X Are effective and efficient with their work time
- X Have developed mechanisms for
  - focusing
  - staying motivated
  - avoiding procrastination
  - staying healthy, well-rested, and happy



## WORK AND PRODUCTIVITY

People who do significant research...

- X Don't give up easily
- X Emotionally commit to solving their problem
- X Live, breathe, and dream about their problem
- X Find ways to relax barriers between work and off-work



# COMMUNICATION

People who do significant research...

- X Generate interest in (and recognition for) their problem/solution
- X Think and communicate clearly
  - Focus on their audience's understanding, rather than appearing smart
  - Create the same mental pictures you use
  - Tell the story using the chronology of your own development of the ideas

# PEOPLE

People who do significant research...

- X Are generous – appreciate and help all the people who (can) help them
  - Put a lot of energy into developing their team; try to make them as great as you
  - Work well in institutional environments and teams
- X Mitigate their suboptimal personal interaction issues
  - shyness, ego, rudeness, anger, weird sense of humor, unprofessional look, etc



# CAN I DO SIGNIFICANT RESEARCH?

## MAIN POINTS

People who do significant research...

- X Work on important problems
- X Know a lot
- X Have courage
- X Commit deeply to a lifestyle
- X Enjoy the process and the rewards



## NEAR-TERM ADVICE

- X Do the best job you can on every project you get
  - You are building experiencing in observing different types of problems and appropriate solutions, and in applying the relevant techniques
  - Doing a great job on things is a habit; so is doing a mediocre job on things
  - Every research project is an opportunity for greatness – look at it closely



## NEAR-TERM ADVICE

### X Be patient

- You are building a lifelong research career
- Don't get anxious about your thesis; it's really just your *first* independent research project

### X Be confident

- There are many ways to achieve greatness, i.e. there is an important place for you in the research universe, if you want it

NEXT TUESDAY

Student panel on “How I selected my research area and advisor”

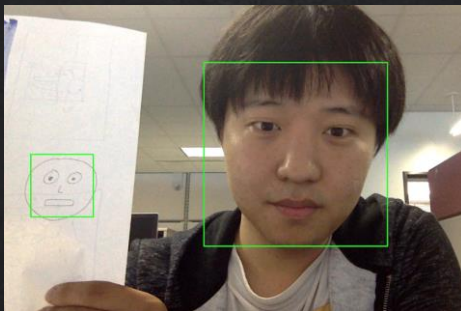




# PANELISTS



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