Research 101: How to Pick a Good Research Problem

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Talk Outline

- Motivation

- What is a Research Problem?
- Key Ingredients of a Good Research Problem

- The perils
- Potential rewards
- Conclusions

The Backdrop

- Continuation of two excellent talks given by Dr Tamer Elsayed

- One of the most important decisions in your PG studies

- Share others' ideas and my thoughts
- Have a live discussion about the matter

Research Problem?

- A problem that **needs** to be researched
  - Fills a **gap** between an **actual** real-world situation (or our understanding of it) and the **desirable** or the ideal state of affairs (human knowledge)
- Sounds very simple, but can be a Big Problem ☺

- Who identifies them?
  - **Readymade**: Proposed by the potential advisor(s)
  - **Grand Challenges & Open Problems**: Identified by a group of leading researchers (eg, a global consortium) or a national / international funding agency
  - Identified by **your own research or observation(s)**

MARRIAGE vs. The Ph.D.

	Marriage	Ph.D.
Typical Length:	7.5 years	7 years
Begins with:	A proposal	A thesis proposal
Culminates in a ceremony where you walk down an aisle dressed in a gown:	✓	✓
Involves exchange of:	Vows	Know-how
Until death do you part?	If you're lucky	If you're lazy

A Good Research Problem

- Working on a good research problem should:
  - Be intellectually challenging
  - Be an enjoyable experience
  - Help you grow in confidence and self-expression
  - Be rewarding in the end

Key Ingredients

- Listed later in the talk
- Based on some theories
  - The Topic model (Patterson & Eades)
  - The Expertise-Domain matrix
  - The Feasibility-Interest matrix (Uri Alon)

The Topic Model

- Independent topic *vs* Part of a team
- Dangerous *vs* Safe
- Narrow & deep *vs* Wide & shallow

Independence

Independent topic	*Part of a team*
Advantages • Not depending on support from anyone else • More exciting for some people	Advantages • Better support from colleagues as well as your supervisor • Good chance of funding
Disadvantages • Funding unlikely • Dangerous at examination time	Disadvantages • Can be boring for some people

Dangerous *vs* Safe

Dangerous	*Safe*
• May lead nowhere • May be uncompetitive • Can be satisfying for some people • Funding unlikely	• Can be satisfying for some people • More chance of funding

Narrow *vs* Wide

Narrow and deep topic	*Wide and shallow topic*
Advantages • More chance of pushing the boundary of knowledge • More exciting	Advantages • Realistic • Good training for industrial research
Disadvantages • Your “model” may be too abstract and unrealistic • It’s hard to choose the variable parameters	Disadvantages • Mostly boring, like a collection of honours theses • Unlikely to contribute a lot

Expertise *vs* Problem Domains

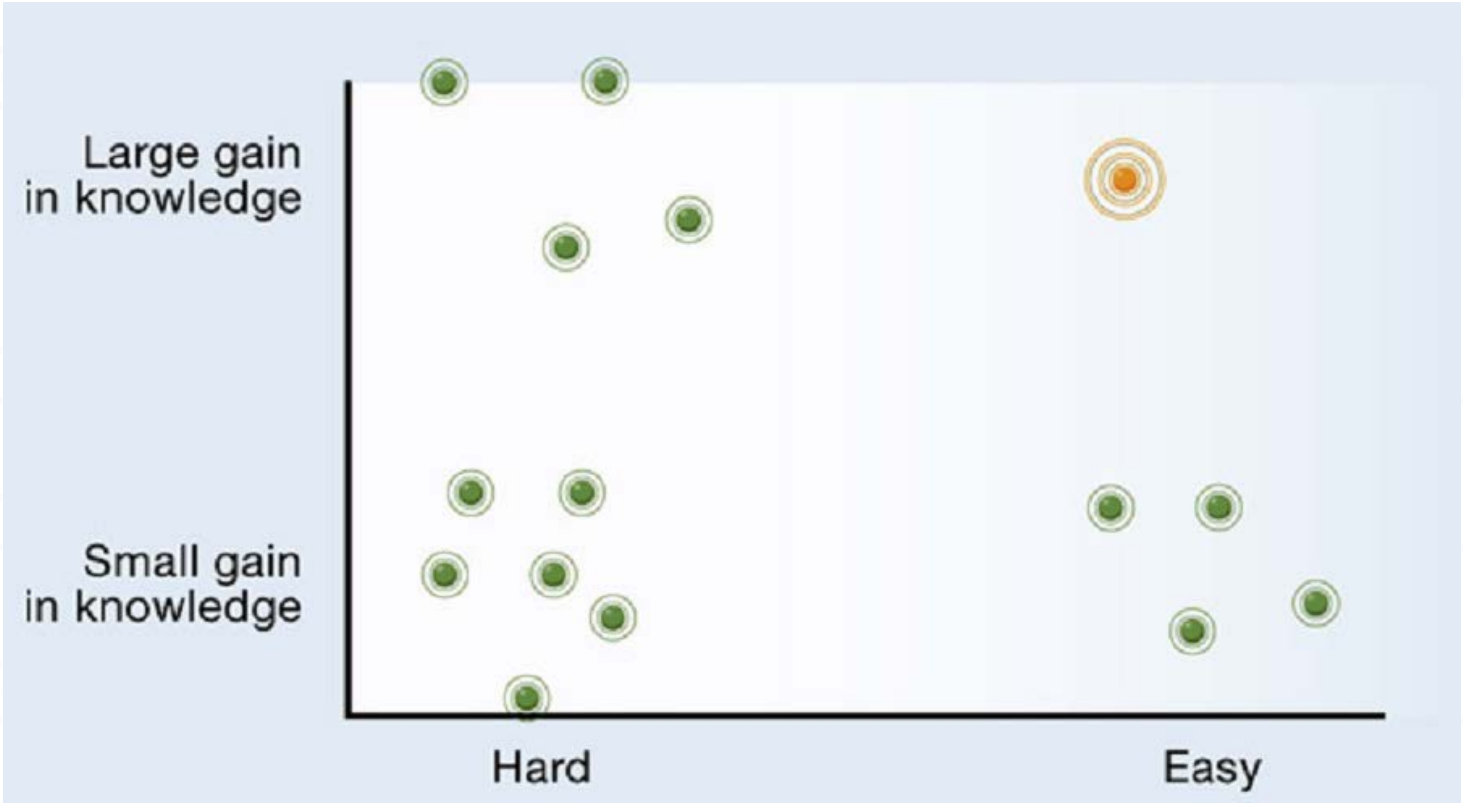
	Big Data	Social Networks	Bio-informatics	Traffic Accidents
Statistics				
Data Mining				
Machine Learning				
Data Analysis				

The Expertise vs. Problem Domains (E-PD) Matrix

The E-PD Matrix

- ✓ Good for scoping potential projects
- × Only considers existing (not acquirable) expertise and potential problem/application domains
- × Does not take into account related **social aspects** such as self-motivation, group dynamics, time etc
- × Does not take into account **feasibility**, an important factor

Feasibility *vs* Interest



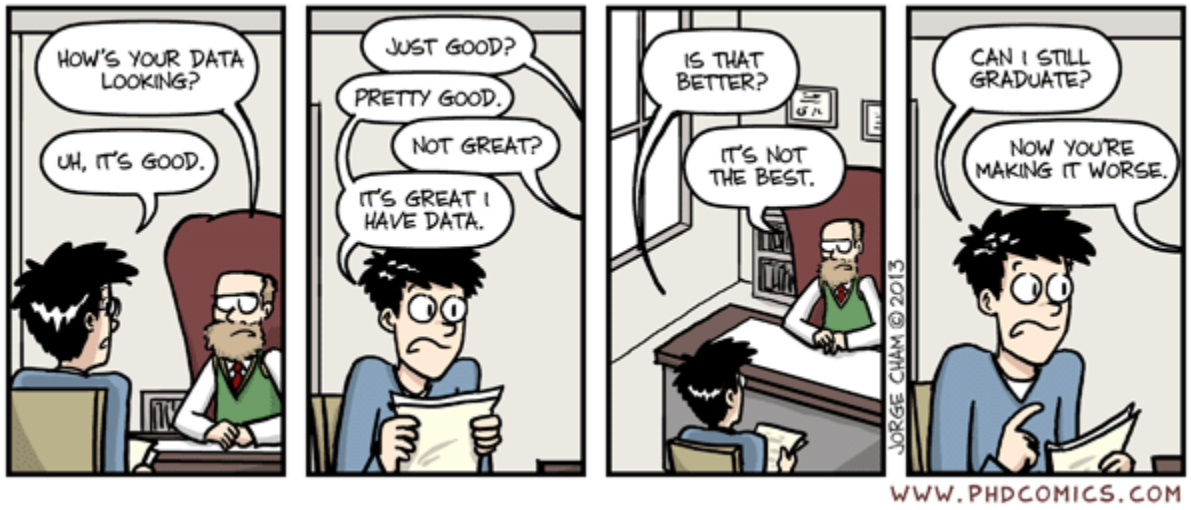
Uri Alon, 2009

Feasibility

- Feasibility can be gauged in terms of the following 3 types of factors:
  1. Background knowledge/skills required
  2. Availability of resources:
     - Computational resources required
     - Minimum/Maximum time required
     - Data availability
  3. Availability of Required Personnel:
     - Team members
     - Collaborators
     - Advisor

Feasibility – Data

- Is the required data readily available, or will you need to collect it?
- Does the data collection involve?
  - Collaborators physically far away and working in a different area
  - A recent experimental setup
  - Medical or biological samples

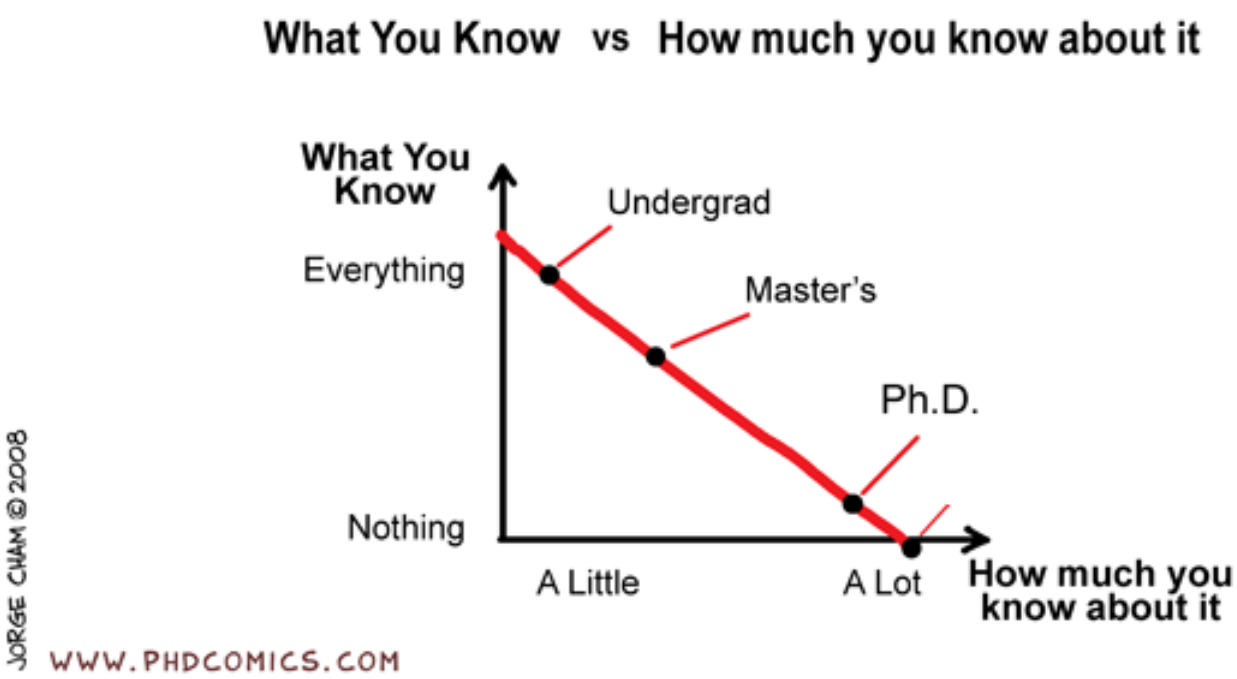


Feasibility – Personnel

- Your advisor should:
  - Have expertise and track record in the area
  - Be interested in the topic
  - Be responsive to answer your questions, provide feedback
- Other PG students in your advisor's team
  - Are more hands-on with related techniques
  - Are more readily available to help
- Collaborators should:
  - Provide complimentary expertise (and the required data)
  - Be at least somewhat interested in your problem

Personnel – Collaborators

Your Advisor Knows Everything?



This is not quite right, but it's true that some PhD students know more than their advisor about the specific problem they are working on.

Interest

- Of personal interest
- Of wider interest to others working in the area
  - Expected to contribute new, verifiable knowledge
- Subjective, by its very nature!
  - Good mentoring can be useful here
- Can impact on self-motivation

Self-Motivation

NEWTON'S THREE LAWS OF GRADUATION

SECOND LAW

"The age, a, of a doctoral process is directly proportional to the flexibility, f, given by the advisor and inversely proportional to the student's motivation, m"

Mathematically, this postulate translates to:

$$age_{PhD} = \frac{flexibility}{motivation}$$

$$a = F / m$$

$$\therefore F = m\,a$$

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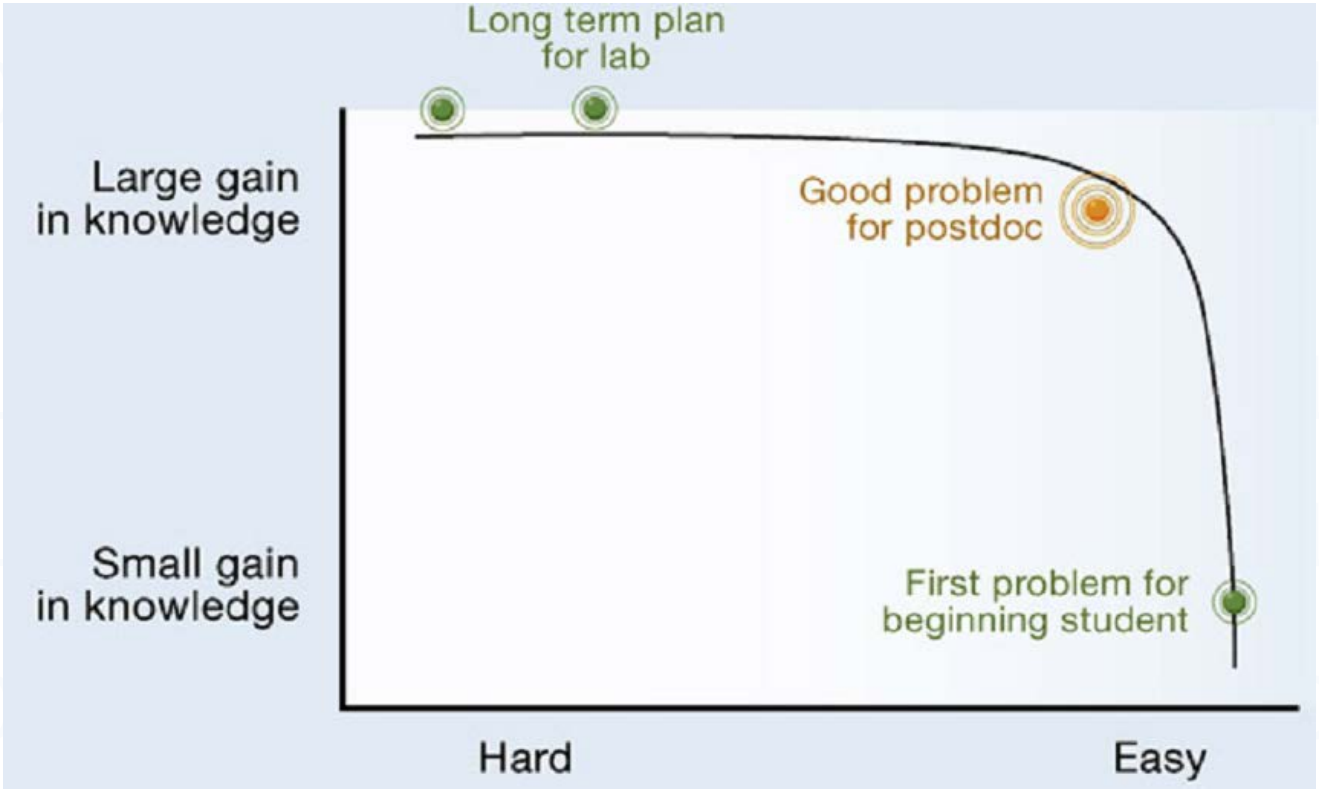
Feasibility *vs* Interest



Interest

Feasibility

Uri Alon, 2009

The Perils

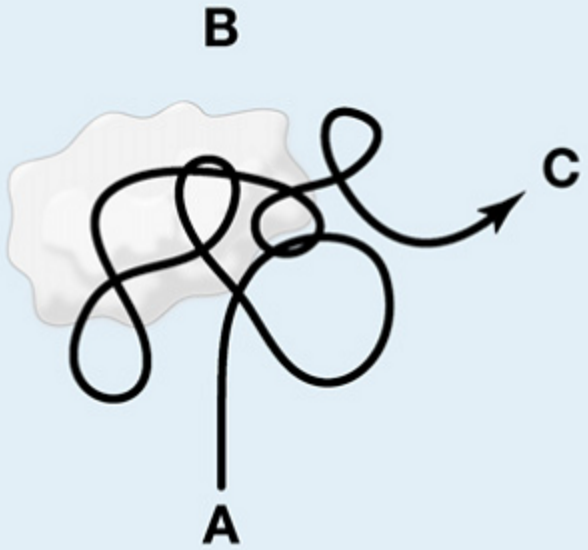
- Working on a good research problem may involve venturing into unchartered territories

- **Risk** of **not** being able to:
  - Achieve your designed objectives in time
  - Get something a collaborator promised
  - Spend too much time with your family & friends

Researching in *The Cloud*

http://www.youtube.com/watch?v=RVoz_pEeV8I&t=3m27s

The Cloud Model

The objective schema can lead to frustration when the project goes off track	The nurturing schema gives support and opens new directions
	

The Joys – Potential Rewards

- Graduate with your thesis – have MS or PhD after your name ☺

- Make new discoveries, develop new approaches
- Scale new heights

- Build in confidence
- Sense of fulfillment & pride

Key Ingredients

- Based on these theories and my experience, here are some of the key ingredients of a good research problem:
  - Involves a team
  - Safe topic
  - Narrow and deep
  - Sufficiently feasible
  - Sufficient personal interest
  - Sufficient wider interest

Take Your Time

Conclusions

- Research problem is a problem (question) in your field that **needs** to be addressed (answered)
- Choosing a good research problem is probably **the most important decision** in your PG study

- Assess a research problem against some of the key ingredients
- Take your time to decide

Acknowledgements

- Dr Tamer Elsayed for initiating this thread
- Several colleagues (students and collaborators) with whom I have had discussions on this topic

- Uri Alon, “How to Choose a Good Scientific Problem,” 2009
- Peter Eades, “How to Get a PhD in IT,” 2010

Thank you