Finding and Solving Research Problems

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Once More: What is Research?

- Oxford online dictionaries: the systematic investigation into and study of materials and sources in order to establish facts and reach new conclusions.
- Wikipedia: research can be defined as the search for knowledge, or as any systematic investigation, with an open mind, to establish novel facts, usually using a scientific method.

Points to note.

- New conclusions/novel facts.
- Systematic investigation.
- Textbook problems, in contrast, are not about *new facts.*
- So, though such problems may be difficult, they are not research problems.

A Key Difficulty

- Since the conclusion at the end has to be new, when one starts, the conclusion is unknown. So,

  How does one even start?
- One guesses what may be a possible conclusion.
- A guess is based on "circumstantial evidence".
- A good guess is based on intuition and imagination.
- A strong guess can turn into a belief.

David Hilbert on being told that a student had given up mathematics and taken up the study of poetry had reportedly remarked: *"Good, he did not have enough imagination to become a mathematician."*

Guess-Then-Proceed Approach

- A guess gives a target to work towards.
- After initial attempts, it may turn out that the guess is not correct.
- But, the work may suggest new avenues.

  New targets: maybe some modifications of the initial target.

  Need to ensure that a new target is also worth working towards.

  New techniques: the known techniques are not appropriate for the work.

  Negative results: the initial target and similar other targets are not realisable.

Getting a worthwhile problem is worth a lot!

"the process of discovering new mathematics is much messier, full of the pursuit of directions which were naïve, fruitless or uninteresting."

– Terence Tao

A Psychological Experiment

- Look around the room and observe the red objects.
- How many green objects did you see?

This may be difficult for most, but, it illustrates the issue of having an open and observant mind.

- Another difference between textbook and exam problems: you do not have to solve a *specific problem; you have to solve a worthwhile problem.*

Serendipity

Serendipity refers to making fortunate discoveries while looking for something else. But, Louis Pasteur had remarked *"In the fields of observation chance favours only the prepared mind."*

Some examples:

(http://en.wikipedia.org/wiki/Serendipity)

- Gelignite by Alfred Nobel, when he accidentally mixed collodium (gun cotton) with nitroglycerin.
- Penicillin by Alexander Fleming. He failed to disinfect cultures of bacteria when leaving for his vacations, only to find them contaminated with Penicillium molds, which killed the bacteria.
- Uranus by William Herschel while looking for comets. In 1781, Uranus was the first planet to be discovered since antiquity.

Top-to-Bottom

Subject

$\downarrow$

Area

$\downarrow$

Topic

$\downarrow$

Problem

- Usual journey is from top-to-bottom.
    - The first two stages are usually evident.
    - Some difficulty in choosing the next level.
    - Getting a good problem is more than half the work.
- Sometimes the journey can be bottom-up.
    - This can happen when you are led to a problem through a connection to another problem whose top-structure is different.

Looking for Problems: Sources

- Textbooks.

  Stated research problems. These are problems others have posed and failed to solve (for a long time).

  Rethinking of basic theory.

- A paper or a group of papers.

  Do you read a paper to gather knowledge? To look for research problems? Or both?

  Getting to the core of the paper.

  Stated open problems: should be seen in a broader context.

Getting Involved

"Don't just read it; fight it! Ask your own questions, look for your own examples, discover your own proofs. Is the hypothesis necessary? Is the converse true? What happens in the classical special case? What about the degenerate cases? Where does the proof use the hypothesis?"

– Paul Halmos

Looking for Problems: Sources (contd.)

- A presentation by somebody else.

    Oral presentations sometimes gives perspectives which papers do not.

- A development in another subject.

    Produces a tool or a result which has direct implications to your subject.

    Requires being abreast of relates subjects/areas/topics.

Looking for Problems: Sources (contd.)

- Asking basic/stupid questions from a broad perspective.
  - Assimilate known knowledge.
  - Ask questions from your own viewpoint about life and society ...
  - Boundaries between subjects/areas are not necessarily crisp.

"one should be unafraid to ask "stupid" questions, challenging conventional wisdom on a subject; the answers to these questions will occasionally lead to a surprising conclusion, but more often will simply tell you why the conventional wisdom is there in the first place, which is well worth knowing." – Terence Tao

http://terrytao.wordpress.com/career-advice

What is a Worthwhile Problem?

An (admittedly inadequate) categorisation.

Important.

- Non-intuitive/counter-intuitive even to someone who knows the area.
- Something that the community had been expecting for sometime.
- A major simplification of a central part of the theory.
- A result which starts off a new subject or an area.
- A result which stops further work in an area.

What is a Worthwhile Problem? (contd.)

Useful.

- Provides a new method or improves upon known methods of doing something which has concrete/practical applications.
- Provides a new technique which can be applied to different problems.

Just about interesting.

- A new result by combining existing results.
- Non-obvious, but, once you start working there is not much difficulty.
- Implications may not be too broad or too deep.

Your Problem and You

- You have to be convinced that a problem is worthwhile before you tackle it.
  - Best effort comes when the work is worth doing.
  - The problem (and solution) has a better chance of been accepted (better saleability).
- Not all problems that one solves will be great.
- Sometimes major advancements are made through solutions to small problems.

"in many cases the mundane nuts-and-bolts of a subject turn out to actually be more important than any fancy applications."

– Terence Tao

Formulating Questions: Concepts

- Key concepts are captured in definitions.
- An area is viewed through the defined concepts.
- Changing/introducing new concepts may change the viewpoint.
    - May lead to simplifications of known methods/proofs.
    - May lead to replacement of ad-hoc analysis by formal analysis.
    - May lead to clarification of relationships between known concepts.
- Making a new definition requires an insight beyond the known definitions.

  "let us try to realize what we do know so as to make the most of it, and to separate the essential from the accidental."

  – Sherlock Holmes

  (The Adventure of the Solitary Cyclist)

Formulating Questions: Algorithms

- Starting from a known algorithm.

  Standard questions to ask are whether the algorithm is correct, whether the resources (time/space/randomness) can be improved.

  Why does the algorithm work?
- For a new computational task.

  Break down into problems which can be solved by known algorithms.

  If this works, that may itself be interesting.

  If this does not work, then you may have something very interesting.
- Having a lot of concrete examples help in thinking about algorithms.

Formulating Questions: Theorems

- Starting from an existing theorem.
  - Why does the theorem hold? This is different from verifying the correctness of the proof.
  - How essential are each of the conditions of the theorem?
  - Is there a simpler set of conditions which will give the same conclusions?
  - Is the converse true? If not, can this be shown by a counter-example?
  - Can the conclusion be strengthened?
  - Are there other interesting conclusions that can be derived from the conditions of the theorem or some variant of the conditions?
- Visualising a new theorem.
  - Unification of scattered partial results.
  - Relationships among the known concepts are not completely known.
  - You have defined a new concept and need to establish its relation to the known concepts.

Importance of Examples

- In many cases, it is extremely useful to have concrete examples.
- Help in building intuition.
- A set of examples may reveal patterns which are not explained by known results.

  Lead to a new conjecture.
- Examples are also useful for checking conjectures before attempting a proof.
- A typical example may illustrate a large portion of the theory.

"Data! data! data! I can't make bricks without clay!"

– Sherlock Holmes

(The Adventure of the Copper Beeches)

Analytical and Synthetic Approaches

- Analytical.
  - Thinking from the end to the beginning.
  - Ability to take apart and understand.
  - You read a result and then figure out the components used to obtain it.
  - Determine the relative importance of each piece.
- Synthetic.
  - Moving from the front to the end.
  - Ability to put pieces together and go forward.
  - Requires the ability for strategic thinking and the ability to see the broad picture.

Tackling Hard Open Problems

Disclaimer: I have not tackled any such problem.

Neither have most other people.

- These are problems which have deep implications.
    - As a consequence, they also have deep connections to different concepts.
    - They are hard and open because nobody knows how to tackle them.
- Hard problems get solved only because people tackle them.
    - You need to have an idea of how to approach the problem.
- Should you invest the time when the likely outcome is negative?
    - A difficult personal decision.
    - There may be partial/side results which may serve your immediate requirement of showing something.

A Climbing Analogy

"It's not about being smart or even fast. It's like climbing a cliff; if you're very strong and quick and have a lot of rope, it helps, but you need to devise a good route to get up there. Doing calculations quickly and knowing a lot of facts are like a rock climber with strength, quickness and good tools; you still need a plan – that's the hard part – and you have to see the bigger picture."

– Terence Tao

How to Solve It?

George Pólya (1887–1985): 4-step procedure.

1. Understand the problem.

   Restate the problem as if its your own.

   Visualise by drawing figures.

   Determine whether you need something more.

2. Make a plan. Some strategies

   guess and check; make an orderly list; eliminate possibilities; use symmetry; consider special cases; look for patterns.

3. Execute the plan.

4. Look back and reflect.

   Investment for the future.

   Reflection helps in assimilating and understanding the strategy.

Heuristic Reasoning

- Analogy: Can you find a problem analogous to your problem and solve that?
- Generalization: Can you find a problem more general than your problem?
- Induction: Can you solve your problem by deriving a generalization from some examples?
- Variation of the Problem: Can you vary or change your problem to create a new problem (or set of problems) whose solution(s) will help you solve your original problem?
- Auxiliary Problem: Can you find a subproblem or side problem whose solution will help you solve your problem?

Heuristic Reasoning (contd.)

- Use Previous Solutions: Can you find a problem related to yours that has already been solved and use that to solve your problem?
- Specialization: Can you find a problem more specialized?
- Decomposing and Recombining: Can you decompose the problem and “recombine its elements in some new manner”?
- Working backward: Can you start with the goal and work backwards to something you already know?
- Visualisation: Can you visualise the problem geometrically and identify structure and pattern?
- Auxiliary Elements: Can you add some new element to your problem to get closer to a solution?

God Rewards Fools

“Ralph, like us, was willing to be a fool, and the way to get to the top of the heap in terms of developing original research is to be a fool, because only fools keep trying. You have idea number 1, you get excited and it flops. then you have idea number 2, you get excited and it flops. Then you have idea number 99, you get excited and it flops. Only a fool would be excited by the 100th idea, but it might take 100 ideas before one really pays off. Unless you’re foolish enough to be continually excited, you won’t have the motivation, you wont have the energy to carry it through. God rewards fools.”
– Martin E. Hellman (Ralph here is Ralph Merkle)

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