All About Research

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The CRA-W Grad Cohort for Women Program
What is Research and Why Do It?

What?
- Investigation of a problem in scientific manner
- Discovery of a solution that advances state of knowledge in areas from theory to algorithms to prototypes to experimentation to applications

Why?
- Create, have fun, play
- Invent, be on leading edge of discovery, be a scientist
- Transfer discoveries to benefit society
- Work in interesting and rewarding careers
Beginning with the End in Mind

A PhD graduate should know how to

- select a difficult and interesting research direction (that matters to someone)
- form hypotheses that can be scientifically evaluated (and learn how to perform the evaluation)
- conduct research
- report on the results by becoming an effective oral and written communicator
- obtain funding, run a research group, be an advisor and mentor to your own students, etc. (so you can continue to do research, have an effect on others, etc.)

Plan for a research career not a research project!
Approximate Timeline

Getting started

- Choose an area
- Find research advisor
- Begin research

Year

1  2  3  4  5  6

Completing your dissertation

- Do the research
- Write the dissertation
- Develop long term career goals
- Prepare for your career

Identify thesis problem
Choose Ph.D. committee

Defining your research
Choosing a Research Area

Criteria

- Exciting and interesting area to you
- Important problems in area
- Research type is suitable to you

Ways to identify a research area

- Take courses, attend seminars and colloquia
- Talk to professors, visitors, other students
- Consider both applied and theoretical areas
- Read widely
- Learn about yourself, what you like, etc.
- Solve some research problems
How to choose a Research Advisor

Advisor-advisee relationships are forever!

- Talk to potential advisors and their advisees
  - What are their projects?
  - How much time do they spend with students?
  - Do they have group and/or individual meetings?
  - How long to return written materials?
  - How much freedom do they give students?
  - How long does it take students to finish?
  - What is the placement of past students?
  - Does the adviser publish a lot with students? What is the order of names?
  - Who presents the papers that are co-authored?
  - Do they have research assistantships?
  - Do you feel comfortable with this person as your advisor?

- “Try out” an advisor
  - Take one of their courses
  - Work with them on an independent study
Working on a Research Team (or not)

Why work on a team (in a group)?

- Part of a larger project (less common in more theoretical areas)
- Research often a collaborative, social process
- Helps you to learn to communicate ideas
- Try new ideas, practice talks, get feedback on papers, learn to advise other students, etc.

How to work on a research team

- Carve out your problem in the group
- Be generous with giving credit to others, but
- Stand up for your accomplishments
- Your role in the group will change as you progress
Identifying a Thesis Problem
You need to be an expert in your area

Read papers
- Use papers’ references to get to original papers
- Keep an annotated bibliography of papers, note
  - Main contribution
  - Open questions
  - How it relates to your interests, work

Talk to experts
- When they visit UDel
- At conferences and workshops

Attend talks, etc.
- Carry a notebook to record notes, thoughts, etc.

Question previous works’ assumptions
Identifying a Thesis Problem
You need to consider potential problems

- Consider hot topics carefully
- Thoroughly understand the problem
- Break problem into manageable pieces
- Develop methods that work for you
  - When to work deeply, broadly; when to put aside
  - Set aside blocks of time to focus on research
  - Work consistently on the problem
Persevere—You Will Find a Topic

"Every morning I would sit down before a blank sheet of paper. Throughout the day, with brief interval for lunch, I would stare at the blank sheet. Often when evening came it was still empty... It seemed quite likely that the whole of the rest of my life might be consumed in looking at that blank sheet of paper..." (Bertrand Russell, autobiography)

Went on to publish (with Whitehead) the 3-volume *Principia Mathematica*

Remember that *drive* distinguishes the great scientists (but brains help ...)
Really starting your dissertation

A major transition (often the 3\textsuperscript{rd} year)

- Classes are finally done
- Now, you have to define your own research agenda
- Self-driven schedule
Completing Your Dissertation

The research itself

✓ Plan your research, and regularly revisit, reevaluate, and revise these plans
✓ Work consistently; work very hard
✓ Take the initiative in your own research
  ■ Usually start out taking advice
    ■ Advisor sets most goals for your work
  ■ Want to end up being a colleague of your advisor
    ■ Should know your research area better than anyone else (even your advisor)
    ■ Should be able to defend your work/decisions
    ■ Should set directions for next subgoals
Completing Your Dissertation showing the world

- Communicate your results along the way
  - Identify publishable pieces of your work
  - Ten simple rules for getting published (Bourne)
  - Make appropriate contacts along the way
    - Workshops and conferences
    - Intern or visit with a research group
    - Electronic communication with researchers
  - Understand “rules” for authorship

- Develop excellent presentation skills (oral and written)
  - Get feedback for your presentations
  - Practice, practice, practice
    - To your research group
    - In your “SIG” group
    - At conferences
Persevere
You Will Find that Elusive Result
(hopefully …)

“Every morning I would sit down before a blank sheet of paper. Throughout the day, with brief interval for lunch, I would stare at the blank sheet. Often when evening came it was still empty... It seemed quite likely that the whole of the rest of my life might be consumed in looking at that blank sheet of paper...” (Bertrand Russell, autobiography)

There are no guarantees - that is what makes it research!

And, you’ll often have difficult/stressful times

But remember - drive distinguishes the great scientists
Some Ways to Find a Topic

There is no “one size fits all”

But, here are six common ways to find a topic
1) A Flash of Brilliance

- You wake up one day with a new insight/idea
- New approach to solve an important open problem
  - interdisciplinary

- Warnings:
  - This rarely happens if at all
  - Even if it does, you may not be able to find an advisor who agrees
2) The Term Project

- You take a project course that gives you a new perspective
  - E.g., theory for systems and *vice versa*
- The project/paper combines your research project with the course project

- **Warnings:**
  - This may be too incremental
3) Re-do & Re-invent

- You work on some projects
  - Re-implement or re-do
  - Identify an improvement, algorithm, proof
- You have now discovered a topic

- **Warnings:**
  - You may be without “a topic” for a long time
  - It may not be a topic worthy of a PhD thesis
4) The Apprentice

- Your advisor has a list of topics
- Suggests one (or more!) that you can work on
- Can save you a lot of time/anxiety

Warnings:
- Don’t work on something you find boring, badly-motivated,…
- Several students may be working on the same/related problem
5) 5 papers = Thesis

- You work on a number of small topics that turn into a series of conference papers
  - E.g., you figure out how to apply a technique (e.g., branch and bound) to optimize performance tradeoffs

- **Warnings:**
  - May be hard to tie into a thesis
  - May not have enough impact
6) Idea From A → B

- You read some papers from other subfields/fields
- Apply this insight to your (sub)field to your own
  - E.g., graph partitioning to compiler optimizations
  - Interdisciplinary

**Warnings:**
- You can read a lot of papers and not find a connection
- Or realize someone has done it already!
Four golden lessons

Steven Weinberg

When I received my undergraduate degree — about a hundred years ago — the physics literature seemed to me a vast, unexplored ocean, every part of which I had to chart before beginning any research of my own. How could I do anything without knowing everything that had already been done? Fortunately, in my first year of graduate school, I had the good luck to fall into the hands of senior physicists who insisted, over my anxious objections, that I must start doing research, and pick up what I needed to know as I went along. It was sink or swim. To my surprise, I found that this works. I managed to get a quick PhD — though when I got it I knew almost nothing about physics. But I did learn one big thing: that no one knows everything, and you don’t have to.

The work of many theoretical and experimental physicists has been able to sort it out, and put everything (well, almost everything) together in a beautiful theory known as the standard model. My advice is to go for the messes — that’s where the action is.

My third piece of advice is probably the hardest to take. It is to forgive yourself for wasting time. Students are only asked to solve problems that their professors (unless unusually cruel) know to be solvable. In addition, it doesn’t matter if the problems are scientifically important — they have to be solved to pass the course. But in the real world, it’s very hard to know which problems are important, and you never know whether at a given moment in history a problem is solvable. At the beginning of the twentieth century, several leading physicists, including Lorentz and Abraham, were trying to work out a theory of the electron. This was partly in order to understand why all attempts to

to spending most of your time not being creative, to being becalmed on the ocean of scientific knowledge.

Finally, learn something about the history of science, or at a minimum the history of your own branch of science. The least important reason for this is that the history may actually be of some use to you in your own scientific work. For instance, now and then scientists are hampered by believing one of the over-simplified models of science that have been proposed by philosophers from Francis Bacon to Thomas Kuhn and Karl Popper. The best antidote to the philosophy of science is a knowledge of the history of science.

More importantly, the history of science can make your work seem more worthwhile to you. As a scientist, you’re probably not
Four golden lessons from S. Weinberg

- Swim or sink. Start doing research, and pick up what you need to know as you go along.
- Aim for tough water.
- Forgive yourself for “wasting time” on seemingly unsolvable problems.
- Learn something about the history of science, or at a minimum the history of your own branch of science.