2014 CRA-W Graduate Cohort Workshop

Finding a Research Topic

(including Interdisciplinary)

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(with credits to Lui Sha, Carla Brodley, Lori Pollack ,Padma Raghavan, Mary Jane Irwin, …)

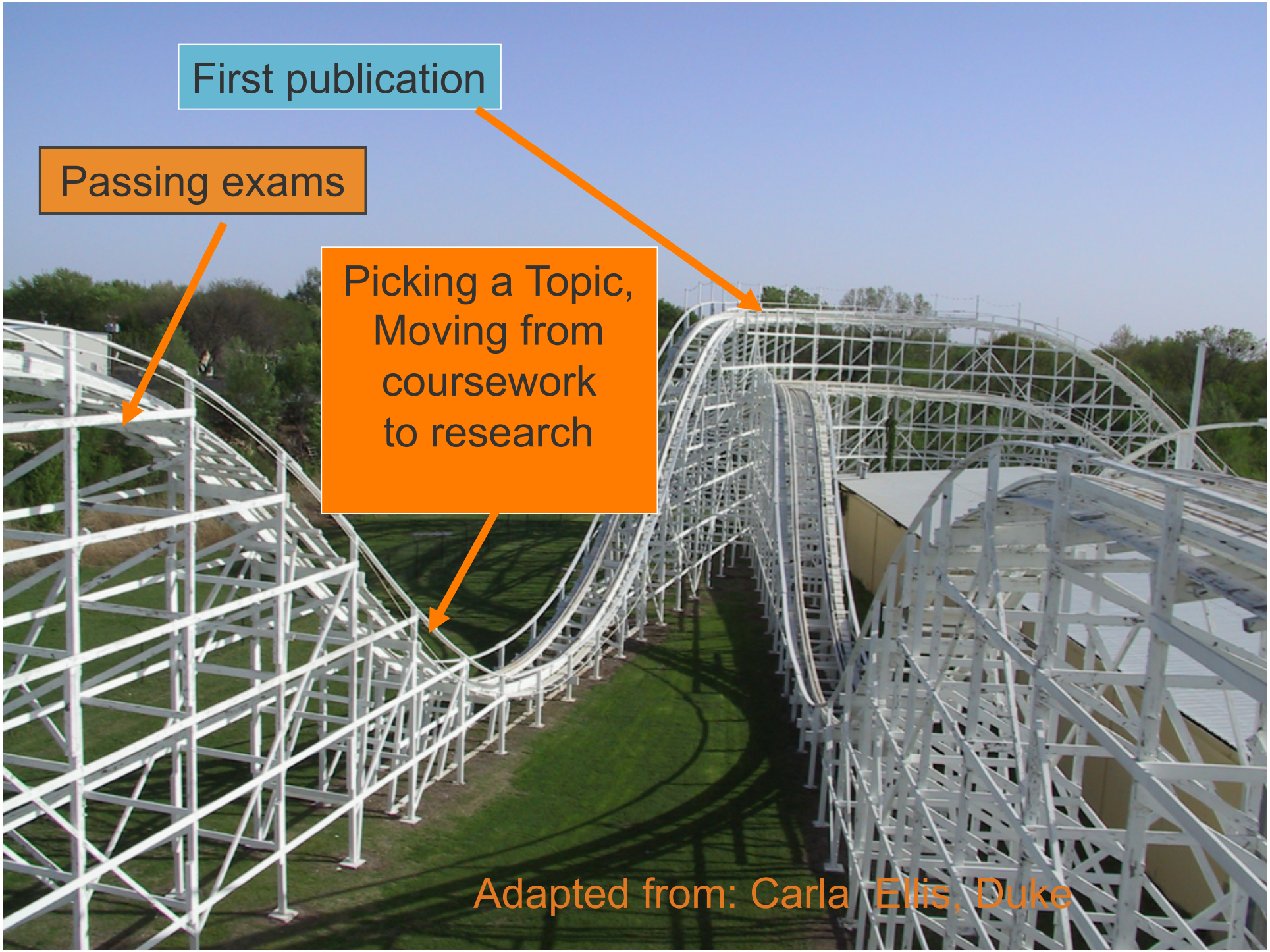
My Zigzag way

What does my story tell you, hopefully?

- **The path to find a research topic will be a zigzag road**
  - **Don't expect to find it in just one shot**
- **Often your research topic changes along your career**
  - **So no need to feel that you will be stuck with your Ph.D topic for the rest of your life**
- **OK to span two fields**
  - **Many breakthroughs are made this way**

Selecting a Topic

- Moving from coursework to picking a topic is often a low point
  - Even for the most successful students
- Why?
  - Going from what you know-coursework with answers, to something new-research that no one knows the answer and there can be many answers


First publication
Passing exams
Picking a Topic, Moving from coursework to research
Adapted from: Carla Ellis, Duke

The Thesis Equation

Topic + Advisor = Dissertation

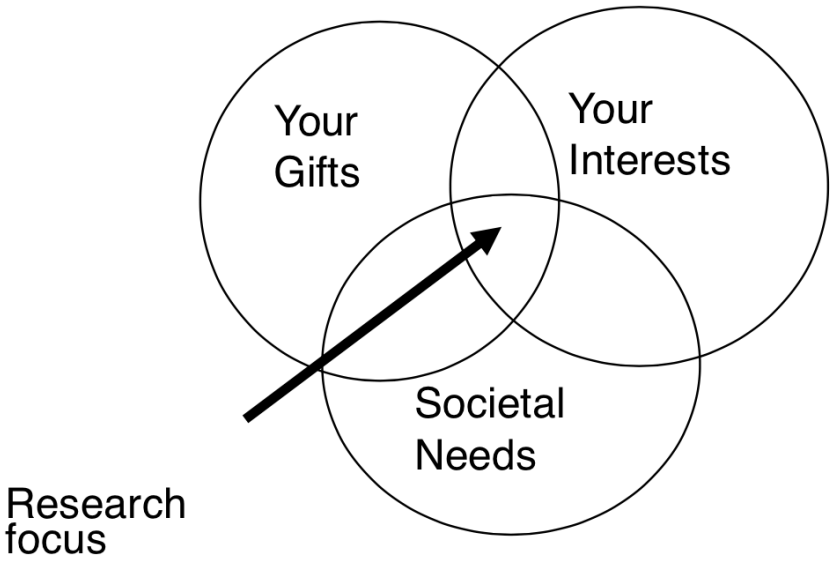
Adviser vs. Research Areas

- What if you like an adviser but not passionate about his/her subfield, or vice versa?

- My personal opinion: Picking a good, matching adviser is more important!
  - An adviser is for life
  - He/she can teach/mentor you in many things, not just research
  - You will be less stressed out
  - You can expand to adjacent subfield, with his/her help

Now the harder part: Find a research topic

The path to success consists of three simple elements. Find what interests you that you can do well, and is needed by the people.



Find your own strength

Understand others is intelligence.
Understand yourself is wisdom.
--- Lao Tze

- What is easier for you?
  - Writing and modifying a complex software and debugging it?
  - Prove theorem?
  - Analyzing data?
- How to find it if you don't know?
  - Try various projects/classes

Your interests?

- What make you excited?
- Imagine yourself attend a talk about such topic
  - Do you falll asleep after 5min?
  - Or you will be awake for the whole talk, and keep discussing with your peers after the talk?
- What if you are not interested in anything?
  - Have you attended enough talks and are exposed to enough fields/areas?
- What if you are interested in everything?
  - Good! Consider the other factors
  - Pick one----Ph.D is only the beginning of your career, and you still have 20-30 years to work on the others!

Find Social Needs

Creating an Exciting Application Scenario

"as a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by the ideas coming from 'reality', it is beset with very grave dangers.

… that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities."

John Von Neumann, "The Mathematician" , 1957

Exciting application scenarios will

- motivate you,
- expose the limitations of existing solutions,
- help you to focus your efforts.

Think Out of the Box

Great advancements in science and engineering often are the repudiation of generally accepted beliefs.

Anonymous

Most researches are constrained by models and generally accepted assumptions of the real world. But our knowledge of the nature is never perfect, and the underlining technologies are rapidly changing…

- *Velocity of light is constant. … embrace it as a law of physics and we have the theory of relativity.*
- *Clients request and server computes … Why not send some of the code to client instead? … and we have JAVA & mobile code.*

- *Is TCP appropriate for wireless communication?*
- *Is fairness a good metric for real time computing?*
- *Is load balance is always a good idea?*

More Detailed Considerations

- Whose interest besides yours may also be important?
  - Your advisor
  - Your research community
    - E.g. architecture and OS fields' interests may not be the same
- Love your topic!
  - Sets the course for your next 2-3 years
  - Determines, in part, opportunities offered to you upon graduation
  - May work in same/related area for years

More Things to Consider

- What drives you? bores you?
  - Technology, puzzles, applications, interdisciplinary
- Do you (i.e., your advisor) have funding for you to work in the area?
  - Working as a TA
  - Working as an RA
  - Having university/college, government, industry, etc… fellowship/scholarship/grant
- Don't chase hot topics unless you are truly interested
  - Hot topics can change by the time you graduate and are in the job market

Focusing from Area to Topic

- Area = subfield
  - architecture, theory, AI, high performance computing, or interdisciplinary
  - Is it important? Timely? Jobs in the area?
- Topic = specific open problems in subfield
  - **Theory**: provably better algorithm
  - **AI**: Improving a machine learning algorithm
  - **Architecture**: reliability, approximate computing
  - **HPC**: parallel algorithm, scheduling scheme
  - **Systems**: Reliability, Big data,
  - **Interdisciplinary**: deep learning, big data…

Topic Scale and Scope

- Scale
  - Should be big enough to have more than one open problem, or solving one should lead to another
- Scope
  - Too narrow, e.g., just analysis no experiment, many not leave enough room
  - Too broad, e.g., data mining, for what? why? too open ended

Interdisciplinary Research Topic

- These days, many top faculty candidates have inter-disciplinary thesis topics
  - Examples: AI + Systems, HCI + Software engineering, AI + Biology/Medicine, HCI + Psychology, database + architecture, HCI+ education,…
- Benefits
  - May leverage your interest/strength in the other areas
  - You can find jobs in other areas/departments
  - You can easily find co-advisers and collaborators
  - It might be easier to bring “fresh air” to an old area or problem
  - **There are so much to learn, so you won’t get bored** ☺
  - …

7 Ways to Identify a Good Research Problem

1) Flash of Briliance

- You wake up one day with a new insight/idea
- New approach to solve an important open problem
- Warnings:
  - This *rarely* happens if at all (please don't rely on it)
  - Even if it does, you may not be able to find an advisor who agrees

2) 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, fruitless, badly-motivated,…
  - Several students may be working on the same/related problem

3) The Extended Course Project

- You take a project course that gives you a new perspective
- The project/paper combines your research project with the course project
  - One (and ½) project does double duty
- Warnings:
  - You may need to check with your adviser first
  - May also be a distraction if the scope and scale are too small

4) Redo … Reinvent

- You work on some projects
  - Re-implement or re-do; Evaluate
  - 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 doctoral thesis

5) Analyze Data (my favorite)

- You participate in more senior student's evaluation study or spend 6 months collaborating with industry:
  - Help with data collection and analysis
  - Identify open challenges
- You have now discovered a topic
- Warnings:
  - You will have to agree on who works on identified open challenges
  - If collaborating with industry, make sure that they allow you publish!

6) The Stapler

- You work on a number of small topics that turn into a series of conference papers
- You figure out *somehow* how to tie it all together
- Warning:
  - May be hard/impossible to find the tie

7) The Synthesis Model

- You read papers from other subfields in computer science or a related field
- Look for places to apply insight from another (sub)field to your own
  - E.g., machine learning to compiler optimizations

- Warnings:
  - You can read a lot of papers and not find a connection
  - Please do NOT first start with the solution. Start with the problem, and then find what solution is the best! (don't look for nails for your hammer)

Tips and Suggestions

- Topic + advisor are both important
- Follow your interests and passion
  - Key driver for success and impact
    - Are you eager to get to work, continue working?
- If not really interested, adapt
  - Tedium or actual lack of interest and motivation?

When you're stuck at the start

- Read/present papers regularly to find open research issues
  - Practice summarizing, synthesizing & comparing sets of papers
  - Write your own slides for presentations
  - Don't 100% believe what a paper says ☺
- Work with a senior PhD student on their research
- Get feedback and ideas from others: conferences, research internships, advisor's idea
- Sometimes you need to take a leap of faith!
  - Be open to trial-and-error

When you're still stuck…

- Do internships in industry
  - They have many problems but have no time to solve them
- Attend PhD oral exams, thesis defenses, faculty candidate talks
  - Understand how to formulate problems
  - Understand what constitutes a problem solution
- Assess your progress, with your advisor
  - Set goals per semester - Have you ruled out an area? converged on an area? Chosen a topic for an exploratory research project?

When you're really really stuck

- Change research topics?
  - May move you out of your advisor's comfort zone of expertise
  - Starting from "scratch" (e.g., need to learn the related work in a new area)
- Change research advisor?
  - May go through 'shakedown' period again
  - May or may not be better off
- Sometimes take a few months break can relax you and fresh up your mind!

Identify a research topic and get started!

Great relevant article in *ACM Crossroads*, "How to Succeed in Graduate School: A Guide for Students and Advisors", (part I, Dec 1994; part II, Feb 1995), available in ACM Digital Library