Finding a Research Topic & Interdisciplinary Research

CRA-WP IDEALS Workshop

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San Diego

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University of New Mexico

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Melanie in One Slide

- From Annapolis, MD
- BS Symbolic Systems, Stanford
- PhD Biology, UNM
- Professor UNM CS & External Faculty Santa Fe Institute

- Research:
  Bio-inspired robotics & computational immunology
- Non-Research: Family & Paddleboarding!

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CJ in One Slide

- Faculty, CIS Department, GRASP Lab, University of Pennsylvania
- PhD Yale, A.B. Harvard, postdoc U.C. Berkeley
- Born and raised in Jamaica
- Research Areas: Robotics and Computer Vision
- Personal: Avid Squash player, very bad chess player

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Quick Poll: Where are you now?

- Identifying a research topic:
  - I know what I want to research and am ready for my proposal
  - I have a vague direction, general interests I want to pursue
  - I have no idea what I will research
- Making it happen:
  - I have a research advisor appropriate for my topic
  - I have funding or a plan for funding
  - I have support in place or a plan to get it:

    mentors, courses, a community of scholars and peers

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What Research Looks Like

"creative and systematic investigation to uncover new knowledge"

Knowledge in a broad range of topics, for example:

- large scale evolution of the universe
- modeling and predicting spread of infectious diseases
- understanding the impact of climate change on endangered species

Broad range of methods of **investigation**, for example:

- theoretical: develop abstract models
- experimental: conduct laboratory studies
- observational: gather data and analyze (apply data science and ML)
- humans: pay special attention to ethical concerns

What CS Research Looks Like

"creative and systematic **investigation** to uncover new **knowledge** in computing"

Knowledge in a broad range of topics, often:

- within traditional branches of computer science (theory, systems, AI, interfaces, etc.)
- also within many emerging areas sometimes at the intersections of branches
  - (e.g. cross-cutting research within CS or vertical research that spans the system stack)
- or with other fields entirely
  - Research that connect CS with biology, medicine, etc.
  - Can enable bi-directional growth and exploration

interdisciplinary

Broad range of methods of **investigation**, may include:

- theoretical: develop abstract models/prove theorems
- experimental systems: build and evaluate large system
- users: humans are part of the system
- data at scale: uncover truths hidden in the data

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What Your CS PhD Research Looks Like

*"creative and systematic investigation to uncover new knowledge in computing done by **you** and your collaborators"*

Topic chosen by you and your advisor (focus of this talk)

- research topic != research interest
- can be interdisciplinary but needs right opportunities

Methods of **investigation** need to be appropriate for the topic, expertise, and resources

There are opportunities to innovate in both topic and method!

You gain new **knowledge** by asking and answering questions that may:

- Uncover fundamental knowledge about computation
- Allow us to more effectively design, program, and use computers
- Enable computers to solve new problems in other fields

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Understanding Your Context

“Your research story began long before you realized it!”
Contributing factors are both personal and environmental.

Intrinsic: your interests, experiences, expertise, aspirations

Local: degree program, department faculty, advisors and collaborators, funding, resource access

Broader: research community interests, past research, societal interests, economic/political climate, timeliness

Interdisciplinary work compounds all these factors!

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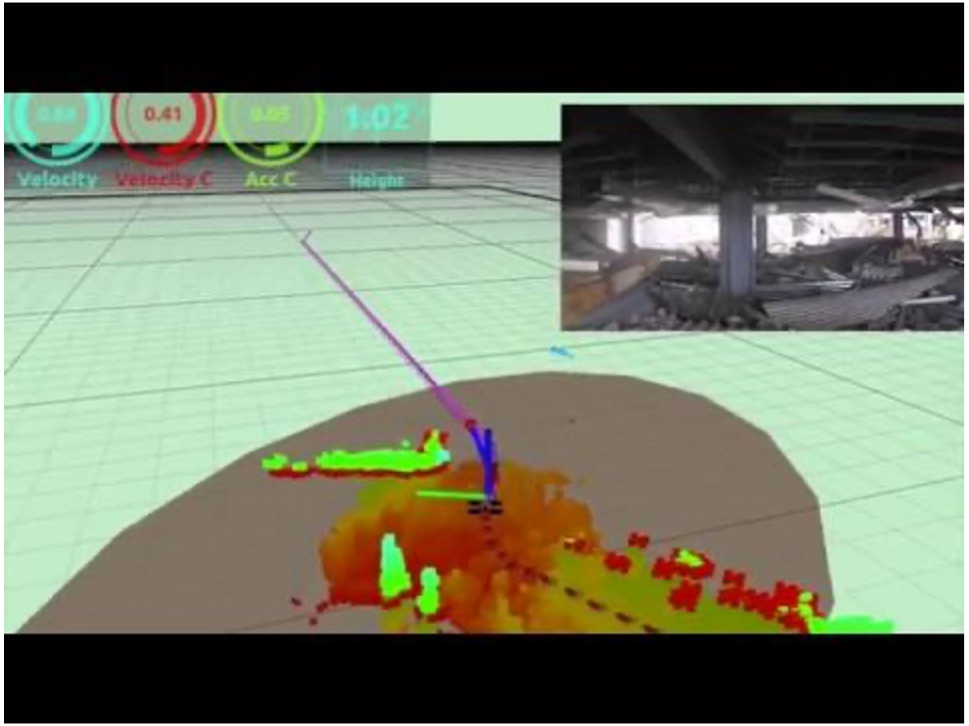
Intellectual Flexibility

- Graduate school is quite different from your undergraduate experience. You may go into it with one idea of what you are going to do but that may change as you learn more, as you talk to more people or as the state of the field changes.
- Stay curious and be willing to learn. Those traits will serve you well beyond graduate school.

Fun with flying robots



Heilmeier Catechism - DARPA

- What are you trying to do? Articulate your objectives using absolutely no jargon.
- How is it done today, and what are the limits of current practice?
- What is new in your approach and why do you think it will be successful?
- Who cares? If you are successful, what difference will it make?
- What are the risks?
- How much will it cost?
- How long will it take?
- What are the mid-term and final “exams” to check for success?

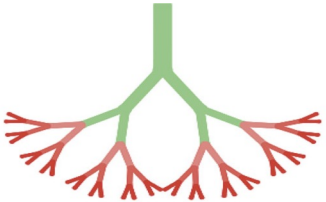
My own winding, branching path

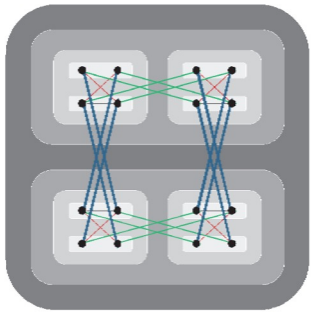
(This is not what you're *supposed* to do!)

- My story of identifying a research topic is full of twists turns, false starts, new beginnings and crossing paths
- A long, inefficient, but ultimately rewarding way to navigate through a research career
- I followed my curiosity without much regard for other factors

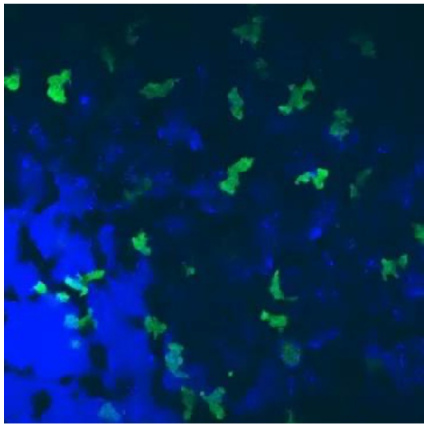
Where did that lead me?

An interdisciplinary journey through computation, ecology, complex systems theory and engineering: scaling theory, immunology, ant foraging and robotics





Scaling properties of networks in CS and Biology



Decentralized search in ant colonies & immune systems

NASA Swarmathon

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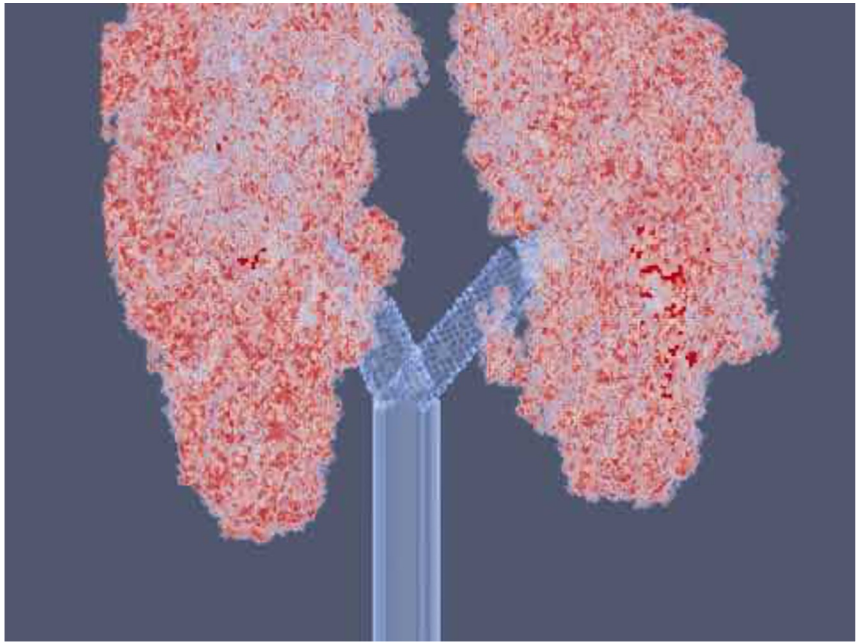
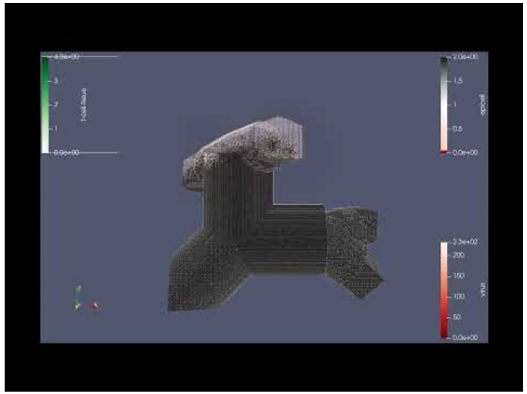
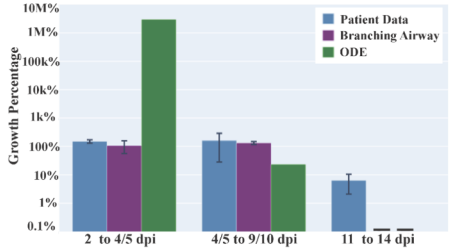
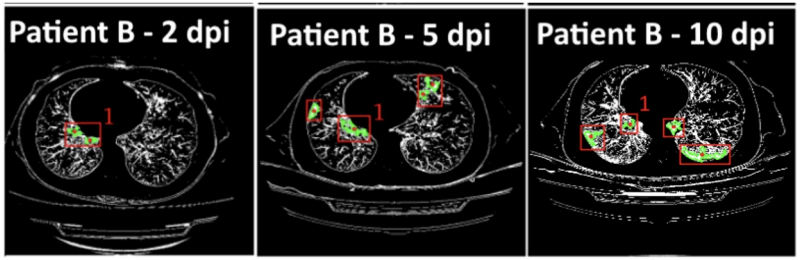
Where did that lead me?

An interdisciplinary journey → modeling COVID spread in the lung









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Other kinds of research journeys

- Some PhD students have a long standing goal that they methodically pursue

- Most students will face uncertainty, setbacks, & confusion as they navigate toward a research topic or topics

- Often your research will lead to multiple interesting ways forward, and you have to choose where to focus your time

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Ways to explore

- Think big, but focus
- **Always connect to what you think is important** - be intentional about what you choose (or agree) to do
- **Take advantage of serendipitous opportunities** –
  - Speakers you hear or meet with
  - Synergy or a spark from a paper you read
  - Discussions with your lab mates, advisor, course projects
  - Make your own luck: the more you explore & connect the more opportunities you have for new insights
- Take time and space (but not too much)
  - Research is a marathon, not a sprint

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Finding a Research Topic

- What are you curious and passionate about?
- What skills, talents, training do you have?
- What kind of impact do you strive for?
- What are your post PhD goals: academic research, teaching, industry?
- What do you want to be the expert in?

- What resources do you have?
- What can your advisor support? Do you need to find another advisor?
- What funding can you get? What can you apply for now?

- What new things are you willing to learn?
- What are you willing to sacrifice, postpone or sideline?

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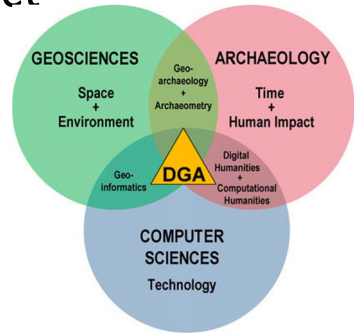
Identifying an Interdisciplinary Research Topic

- Many (perhaps most?) interesting, challenging and high-impact research programs are at the boundaries between disciplines
  - Previous researchers have been

    untrained in skills to take on these research problems &

    unprepared to identify the right questions/problems
- Look for areas of intersection & opportunities for cross-pollination
- Read, attend conferences, talk to speakers in different research areas that you are excited about



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Identifying an Interdisciplinary Research Topic

- Many important areas in CS research began with interdisciplinary innovations: e.g. human robot interaction, computer security, neural networks, social network analysis
- Almost all natural and social sciences need new computational approaches
- Opportunities abound for YOU to be THE EXPERT in
  - Computational linguistics
  - Computational immunology
  - Computational X

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Opportunities and Challenges for Interdisciplinary Research

- Have you found a problem that matters?
  - Are you passionate about the problem?
  - Is anyone else, or can you convince them to be?
- Is there funding?
- Do you have the needed skills, tools & training?
  - Have you leveraged your unique skills, training, interests and perspective in identifying a problem and approach?
- Is there a community of researchers ready to accept the research?
- Are you building from established successe,

  or are you too far out on a limb?

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Case Study finding an Interdisciplinary Research Project

- "Joe" came to my lab with a MS degree in Computer Engineering
- Joe was interested in complex systems and biology.
- His first 2 years he worked on a project on disease modeling. Joe never really liked disease modeling.
- Joe overheard me say that our ant foraging models could be effective robot foraging algorithms
- Joe, who had just gotten his first Arduino, said he wanted to build robots to implement ant algorithms
- I advised Joe that would be hard, & it would delay his graduation
- Joe designed robots, implemented the algorithm, published 20 papers, mentored 6 undergrads, helped get $2.8 Million in NASA funding, ran the Swarmathon, and now works as a senior roboticist

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Finding advisors for interdisciplinary research

- One, two or more advisors and mentors?
- Do they appropriately balance breadth vs depth of research?
- Do they have a core identity that supports or overlaps with yours?
- Are they open-minded and enthusiastic about learning from other fields?
- Can they fund interdisciplinary research?
- Will they help you find a community of researchers that support your work?

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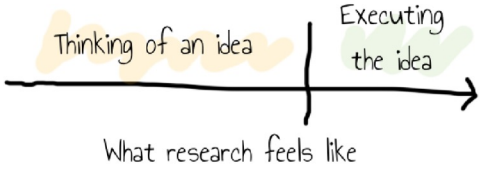
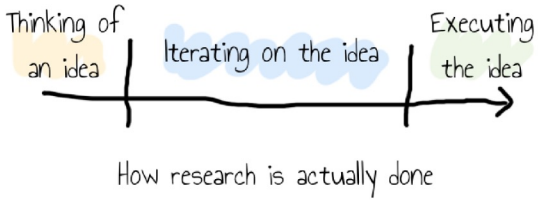
Pitfall: Early Fixation

Often people get fixated on an early idea...work on it for long time and then expect to present a complete idea to the advisor and expect them to accept/reject it as is...

Best Practice: In many successful projects there are many iterations of an idea after the initial conception. This is part of the process and is actually where lots of knowledge is acquired!





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Pitfall: Flightiness

Flightiness is the other extreme where one churns quickly through iterations/ideas without properly evaluating them

Best Practice: Do not move on to the next idea/iteration until you have learned why the current one doesn't work. Keep focused!

Pitfall: Too *

Pitfall: “Is my topic too …?”: too broad or too niche? too hard or too easy? too risky or too straightforward? …

Best practice: Be intentional in your research pursuits and explicitly evaluate potential ideas:

- Generate lots of ideas
- Evaluate Impact, Effort, Risk (low, med, hi)
  - Balance higher impact against effort and risk
  - If risk is high, can you fail fast?
- Is it SMART? Specific, Measurable, Attainable, Relevant, Time-bound

Pitfall: Self Deprecation

Pitfall: “How on earth am I going to do something that has never been done before?”

Best practice:
CS research is the acquisition of new CS-related:
problem + questions + methods + application

Only one of these things has to be new!

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Pitfalls and Best Practices

- **Early fixation**
- **Flightiness**
- **Too broad, deep, hard…**
- **Self Deprecation**
- Forgetting the purpose/end goal
- Getting scooped
- Waiting for inspiration
- Not having the necessary resources
- Advisor contention
- Moving goalposts
- Too personal
- Going at it alone

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Recap: Finding a Research Topic

- What are you curious, passionate about? What do you want to be known as an expert in? What kind of impact do you strive for?

  What is the new approach, knowledge, or problem you will solve?

  (Remember Heilmeier Catechism - no jargon!)

- What skills, talents, training, funding do you have or need?
- What are your post PhD goals: research, teaching, industry?

- What new things will you need to learn?
- What are you willing to sacrifice, postpone or sideline?
- **What pitfalls do *you* need to look out for?**

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