Thoughts on How to Do Good Research

(Slides at: https://www.joergwidmer.org/Thoughts%20on%20how%20to%20do%20good%20research.pdf)

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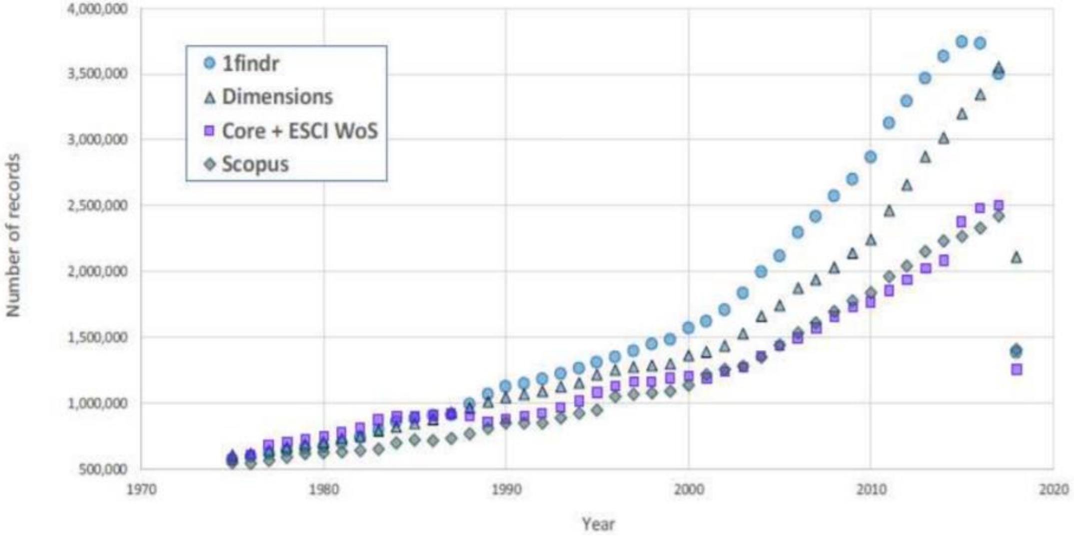
Developing the Science of Networks

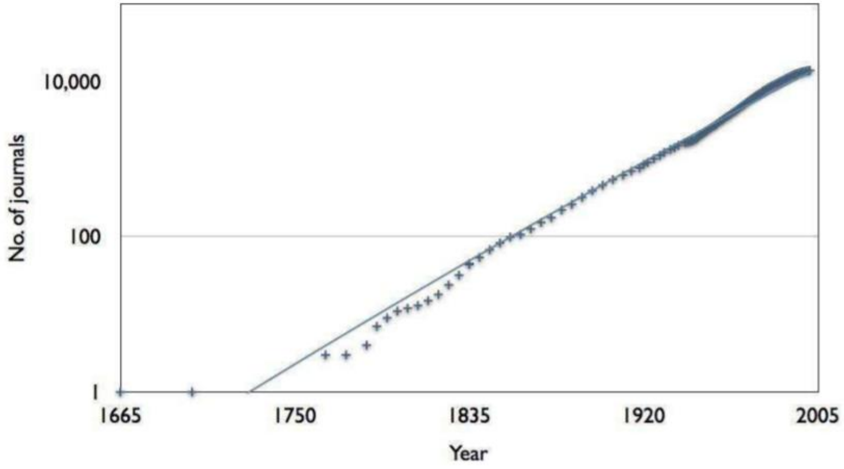
Research Trends

- Historically science was slow
  - Scientists had more time
  - Darwin took 20 years from the idea to writing “On the Origin of Species”
- Over the past few decades it has accelerated tremendously
  - Publish-or-perish is a fact
  - Less time to sit down and think
  - But also much better access to much more prior research and much more powerful tools
- Information overload: there are more papers than you could ever hope to read
- Having an impact is hard

Articles indexed from academic & scientific journals



Growth of active, peer reviewed journals



The STM Report, Fifth Edition, October 2018

Outline

"It is really important to **do the right research** as well as to **do the research right.**"

George Springer, Stanford University

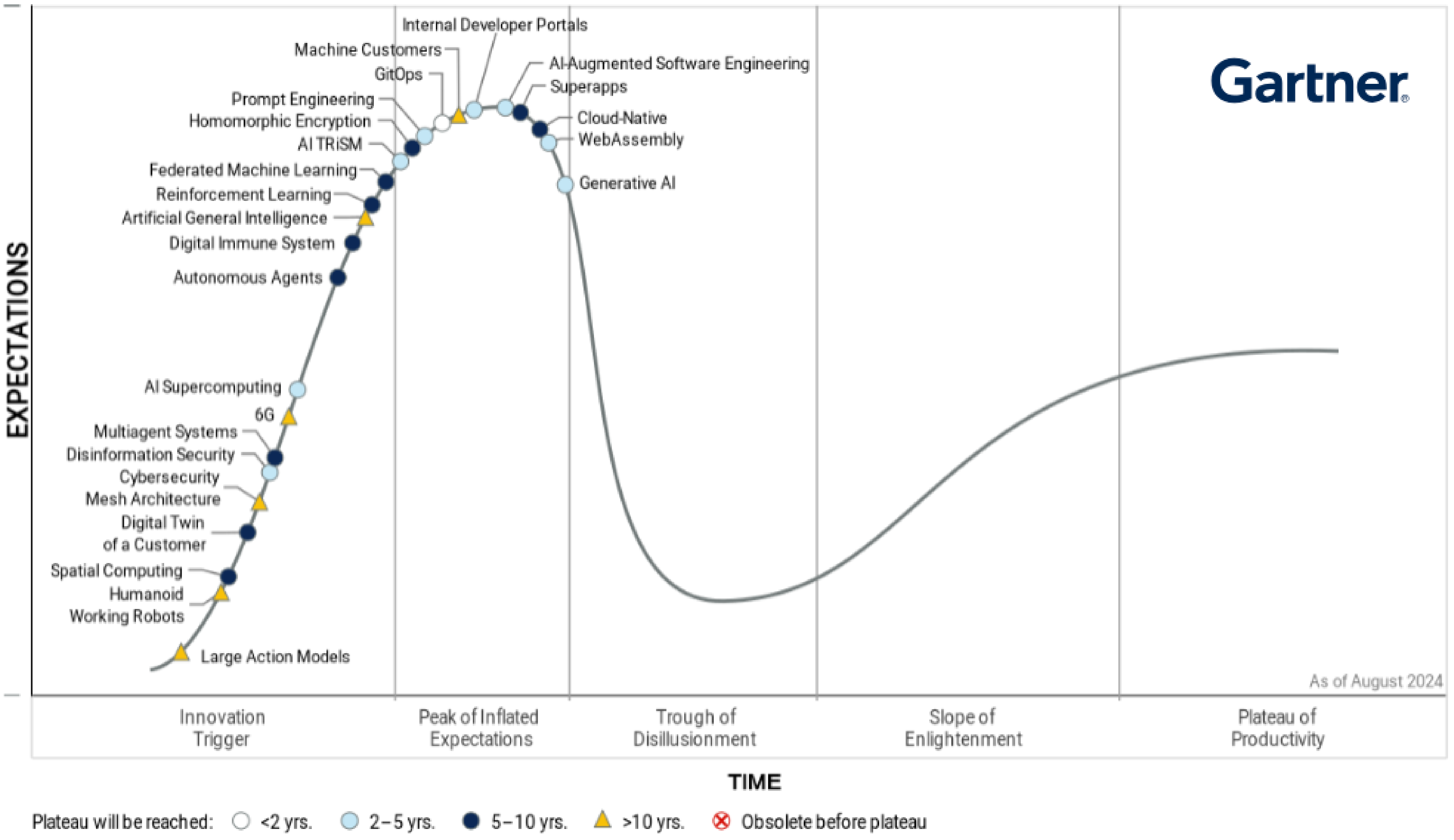
- Research
  - Choosing your research area
  - Finding the right problem
  - Coming up with a solution
  - Evaluating your idea
  - Dissemination
- You
  - Motivation
  - Time-management
  - Research ethics
  - Work live balance

CHOOSING YOUR RESEARCH AREA

Research Area

- Yes, you may already have one
  - But there's always wiggle room
  - And you will face this question many times over in your research career
- Choose something you like
  - Research should be fun (most of the time)!
- (Practical) relevance
- Look for areas that have not yet been explored thoroughly
- Pick an area close to your past experience
  - Unless you explicitly want to change fields
- Picking the right area may have a significant impact on your future career

Gartner 2024 Hype Cycle for Emerging Technologies



Check Out Research Areas

- First go for breadth, then depth
- Read survey papers in some areas of interest
- Talk to researchers in the area
  - Find the most well-known research groups
  - Look at what they are publishing and how their focus is changing
- You may have to take PhD courses anyway; pick them wisely!
  - … and use Coursera, Udemy, Udacity, Edx
- Attend as many research talks as you can
- Conferences are increasingly online (and often free!)
- **Keynote speeches** from top researchers are among the best resources out there

Focus

- Topic too broad
  - Harder to have a coherent “story” for your PhD trajectory
  - You will have a hard time focusing
  - Too much to read
  - Too much competition
- Topic too narrow
  - Danger of incremental work
  - Nobody cares
  - Difficult to find collaborators
- Topic too old
  - Can still be relevant from a practical point of view
  - But much harder to defend that your work is novel
  - Reviewers will your work just because they think this must already exist
- Topic that is still “hot” in a few years when you enter the job market

> “The most successful research topics are narrowly focused and carefully defined, but are important parts of a broad-ranging, complex problem.”
>
> Cliff Davidson and Susan Ambrose
> Carnegie Mellon University

FINDING THE RIGHT PROBLEM AND SOLUTION

Heilmeier's Catechism

You must know the answer the following questions before moving on to writing a good paper:

- What problem do you want to solve?
- Who cares about this problem and why?
- What solutions exist and why are they that inadequate?
- What is your proposed solution to this problem?
- What is new about your approach?
- How can you demonstrate that this is a good solution?
- Who will care if you succeed?
- How long will it take?
- What are the risks?

Heilmeier's Catechism: A set of questions credited to George H. Heilmeier that anyone proposing a research project or product development effort should be able to answer.

What problem do you want to solve?

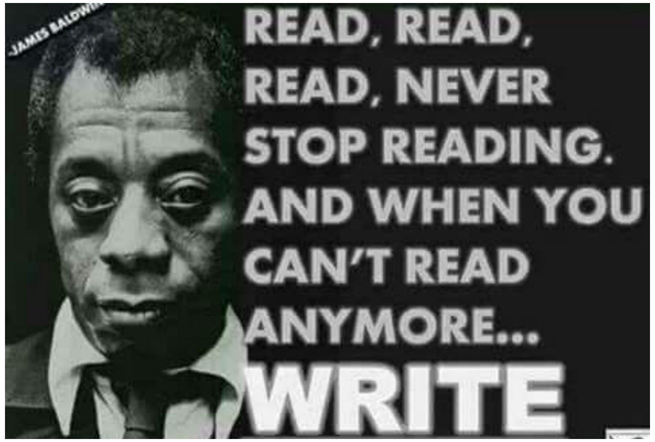
- Follow your passion
  - You're doing this ~~to~~ for yourself
- Make sure you **fully** understand the problem
  - You **have to understand the problem** before going on the solution
  - Taking time to think this through at the very beginning will save you loads of time later on
  - Resist the temptation to jump right in with a solution
- Write it down
  - Formulate a clear research question and objective

Who cares about this problem and why?

- Make sure its a problem **you** care about
  - This must be worth your own time!
  - Don't be too picky (at the beginning)
  - … but if you don't care about the problem, the research will be agonizing and you won't come up original ideas
- … and ideally not only you care about
  - Maybe you're brilliant and no one else realized the potential (but then again, maybe not)
  - But you need collaborators, funding, citations, …
  - Having impact is critical

What solutions exist and why are they inadequate?

- Look for a problem for which there is no good (enough) solution yet
- Let your literature search guide you
  - First go for breadth, then depth
- *Critically* review the existing literature
  - It is often not as easy as it seems (assumptions, caveats, …)
  - Questions that arise while reviewing can be excellent seeds for your own research
  - Don't be afraid to contact the authors and ask questions
- The more you understand existing solutions, the better you can find alternative solutions and new questions
  - Avoid duplicate work!
- Yes, you can write a survey paper, but don't overdo it!



James Baldwin

What solutions exist and why are they inadequate?

- Good background knowledge is essential to develop new ideas
- Know and understand the relevant building blocks from those papers
  - Typical optimization approaches, algorithms
  - Protocol design components
  - Signal processing mechanisms
  - Theoretical background (information theory, …)
- Develop a core tool-set you know in detail and can apply well
- Read technology news to understand the industry, what solutions make it to the market, what are the constraints
- Continue reading while doing the research

Reviewing

- A researcher has a moral obligation to serve as reviewer
  - Peer review is a central pillar of science
  - And you get to read cutting edge work before it is published
- Very good exercise to know 1) how reviewers function and 2) what questions you should ask yourself concerning your work
- Check for correctness, novelty, impact, readability
- Fine line to decide what is a sufficient contribution
  - Too permissive and you encourage poor research
  - Too restrictive and you delay or block good research

- Write a useful review
  - Be fair: write one you would like to receive yourself, even if it's negative
- Lots of resources and papers how to review a research paper
- It's also a time killer; don't overdo it

What is your solution? What is new about it?

- It’s fine to start your PhD doing incremental improvements to the state of the art
  - You learn to write papers and sometimes this might lead to a bigger idea
  - *BUT: there are already far too many incremental papers*
- The goal of your PhD is to learn how to do original research
  - Scientific progress itself is incremental, but it must not lack innovation
  - Make sure you look for more novel ideas and bigger problems as soon as possible
  - Set the bar high: you never write an excellent paper by accident

Six Different Approaches to Research Ideas

- Flash of brilliance: don’t wait for it, this rarely happens
- Ask/wait for advisor: safe, but you don’t learn how to do it
- Build a system and the topic emerges: riskier, you better hope a topic *does* emerge
- Combining many small contributions: gratification along the way, but may be hard to make a coherent connection and the work is almost always incremental
- Use insights from other fields and try to apply them in yours: may or may not work, high risk/high reward
- Take a project course, combine the course project with your research: two birds, one stone, but can distract you if you don’t find a connection

Adapted from https://www.slideshare.net/RIZWANABBAS3/choosing-research-topic1

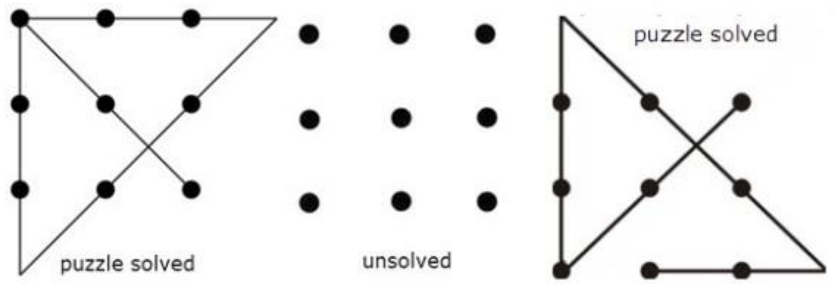
Conflict between "productive tradition" and "risky innovation"

- "Published papers that make a novel connection are rare but more highly rewarded"
- "So what accounts for scientists' disposition to pursue tradition over innovation? … Innovative research is a gamble whose payoff, on average, does not justify the risk."

Jacob Foster (UCLA), American Sociological Review, 2015

- … because of publish or perish
- Most researchers err on the side of caution, producing largely irrelevant research
- When in doubt, go for high risk high reward (but keep an eye on progress and have a fall back)

Creativity



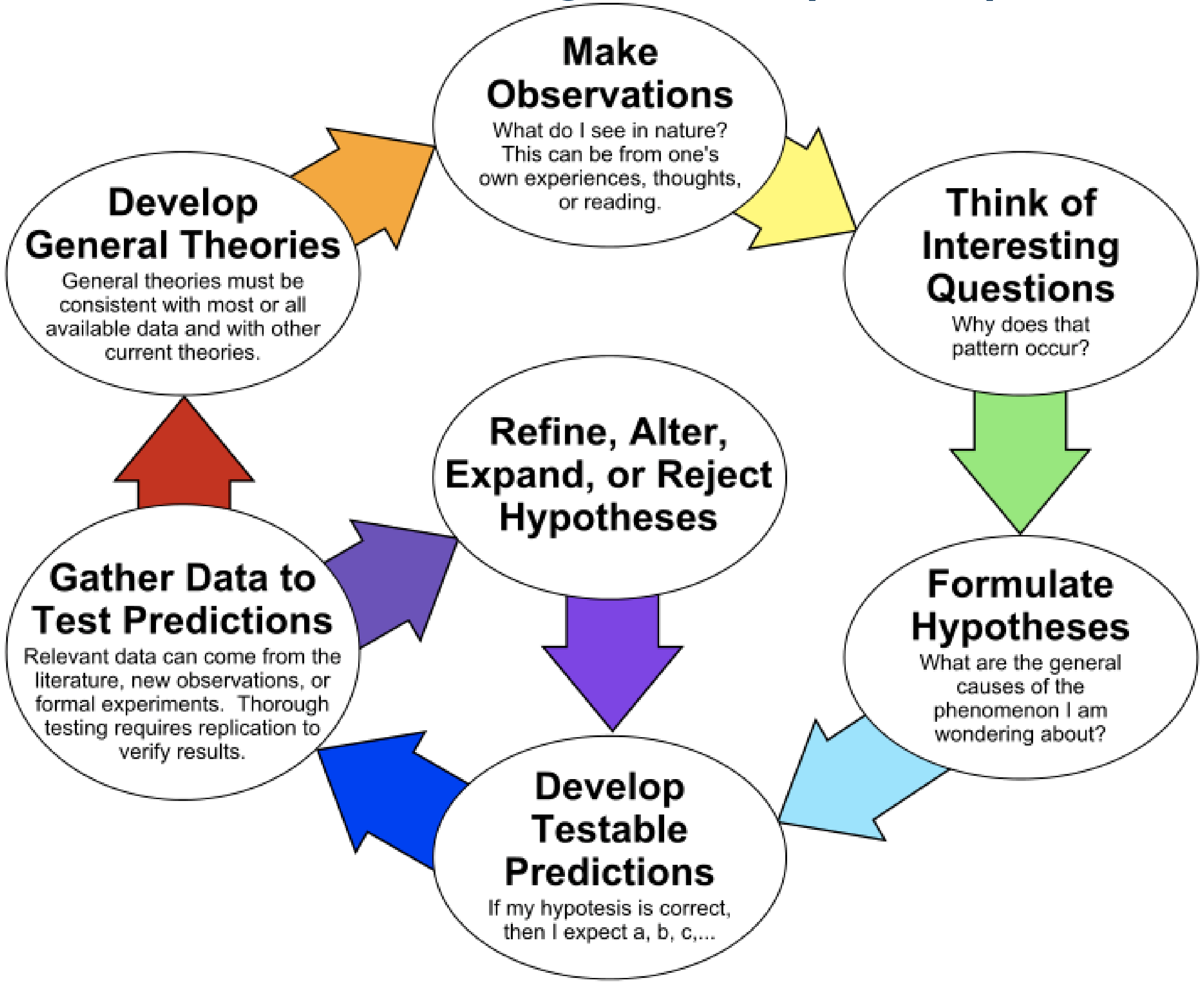
- "Thinking Outside the Box": creativity does not work at the push of a button
  - It comes when you're having fun, are relaxed, "in the flow", exchange ideas with other people, …
- Make sure you know which box you and others operate in
- Beware of unquestioned truths (invisible boxes)

- Beware of tunnel vision
  - Stop the research and read some more
  - Take a step back and look at the bigger picture
  - Work on something else for a bit
  - Explain the problem to someone else
  - Think in a different place/time (before sleeping, in the shower, while exercising, …)

> "We *cannot* think outside boxes. We can, though, choose our boxes."
> Harvard Business Review, June 2015

How can you demonstrate that this is a good solution?

- The Scientific Method: set of general principles for any research



How can you demonstrate that this is a good solution?

- Remember it's an iterative process → iterate!
- Theories, formal proofs, algorithm verification, …
- Apply your idea to some (realistic) examples
  - Simulation studies
  - Prototypes and testbeds
  - Repeatability (with same setup), reproducibility (with different setup)
  - Precision (agreement between measured quantities)
  - Accuracy (agreement between measured quantity and the true value)

Critical Thinking

- Constantly question your solution and results
  - Attack your solution from all possible angles to find the holes (or others will)
  - Questioning things is what research is all about
  - More often than not, it's been done before. If not, there may be good reasons why people take a different approach.
- Beware of confirmation bias
  - CS research is often (too) hands on; we can learn a lot from other disciplines
  - Think about what the results mean
  - Is this what you expect? Why?
- It's hard to be creative and critical at the same time → switch between the two
  - Good research needs both!

You’re Not an Island

- Know how your research fits into the greater body of work in your research area
  - Discuss advantages and disadvantages of your approach
- Do a systematic comparison with *the best* state-of-the-art
  - Except for the rare case where you’re the first to ever solve the problem
  - Make a fair comparison (resist only picking the sweet spot where your algorithm works better)
  - If you don’t, how can a reviewer trust you?

Dissemination

- The best results are worthless if others don’t see them
  - Prioritize quality over quantity
  - Avoid salami paper writing
- Learn how to communicate scientific results
  - How to write a good research paper is addressed below
- Open science: collaborative, reproducible and reusable research
  - Common in other areas, and critical for CS/EE to advance more rapidly
  - Open-access publishing, open-source software, open data, open hardware
  - Common testbeds

Paper Writing

- Abstract: concisely state the problem, your approach and solution, and the main contributions results of the paper
- Introduction: (Stanford InfoLab's patented five-point structure)
  1. *What is the problem?*
  2. *Why is it interesting and important?*
  3. *Why is it hard?* (E.g., why do naive approaches fail?)
  4. *Why hasn't it been solved before?* (Or, what's wrong with previous proposed solutions? How does mine differ?)
  5. *What are the key components of my approach and results?* Also include any specific limitations.
- Don’t overclaim, don’t over-criticize others

From: Tips for Writing Technical Papers
Jennifer Widom, January 2006

Paper Writing

- Body: the paper should tell a *coherent* story
  - Tell the story the results should evoke in the mind of the reader (*not* the story of how you arrived at your results)
  - Use a "top-down" description: readers should be able to see where the story is going and how components fit (be able to skip ahead and still get the idea)
  - Readers may skip the math and details: make sure the intuition and important contributions are clear without them
  - Justify your design choices: why not use alternative solutions or techniques
  - Rule of thumb: clear new important technical contribution by page 3
  - When possible, use a running example throughout the paper
  - Clearly delineate material that is not original but is needed for the paper

From: Tips for Writing Technical Papers
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Paper Writing

- Evaluation: all the relevant details to fully understand what you did
  - It is easy to do meaningless experiments (many papers do)
  - It is easy to craft experiments to show your work in its best light (most papers do)
  - Recall the points on critical thinking and comparing against state-of-the-art
  - Go from simple to complex, adhere to the story
  - Make sure each contribution/claim is translated to appropriate research question(s) → no unsubstantiated claims
  - Make sure each question is answered with help of appropriate metrics
- Conclusions: short summary, do not repeat the abstract or introduction

From: Tips for Writing Technical Papers
Jennifer Widom, January 2006

Paper Writing

- Read "The Elements of Style" by Strunk and White (and the many other available resources on writing)
- Learn good *technical* writing
- Be aware of what happens in the mind of your reader
- Keep it simple and omit needless words
- Use technical terms and names consistently
- Run a spelling checker on your final paper, no excuses!
- Create nice, readable figures, uniform design, decent font size
- Use LaTex!
- Carefully check your final bibliography
- If you are not meticulous with the presentation, reviewers assume you are not meticulous with the research either!

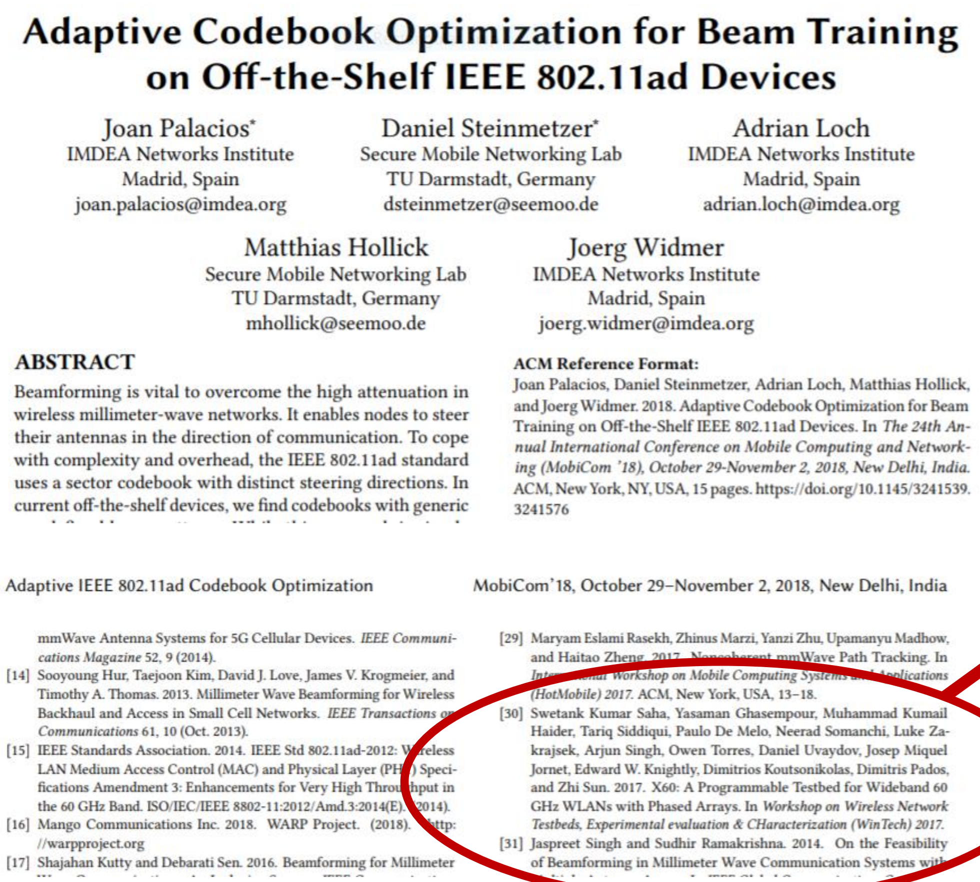
From: Tips for Writing Technical Papers
Jennifer Widom, January 2006

How long will it take, and other questions

- Can you cover the topic in the time you have?
- Do you have the right skills?
- Do you have the right resources (funding, equipment, access to data, colleagues, …)?
- What are the chances of success/failure?

SOME TOOLS

Related Work + Google Scholar



Adaptive Codebook Optimization for Beam Training on Off-the-Shelf IEEE 802.11ad Devices

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ABSTRACT

Beamforming is vital to overcome the high attenuation in wireless millimeter-wave networks. It enables nodes to steer their antennas in the direction of communication. To cope with complexity and overhead, the IEEE 802.11ad standard uses a sector codebook with distinct steering directions. In current off-the-shelf devices, we find codebooks with generic

ACM Reference Format:
Joan Palacios, Daniel Steinmetzer, Adrian Loch, Matthias Hollick, and Joerg Widmer. 2018. Adaptive Codebook Optimization for Beam Training on Off-the-Shelf IEEE 802.11ad Devices. In *The 24th Annual International Conference on Mobile Computing and Networking (MobiCom '18), October 29-November 2, 2018, New Delhi, India.* ACM, New York, NY, USA, 15 pages. https://doi.org/10.1145/3241539.3241576

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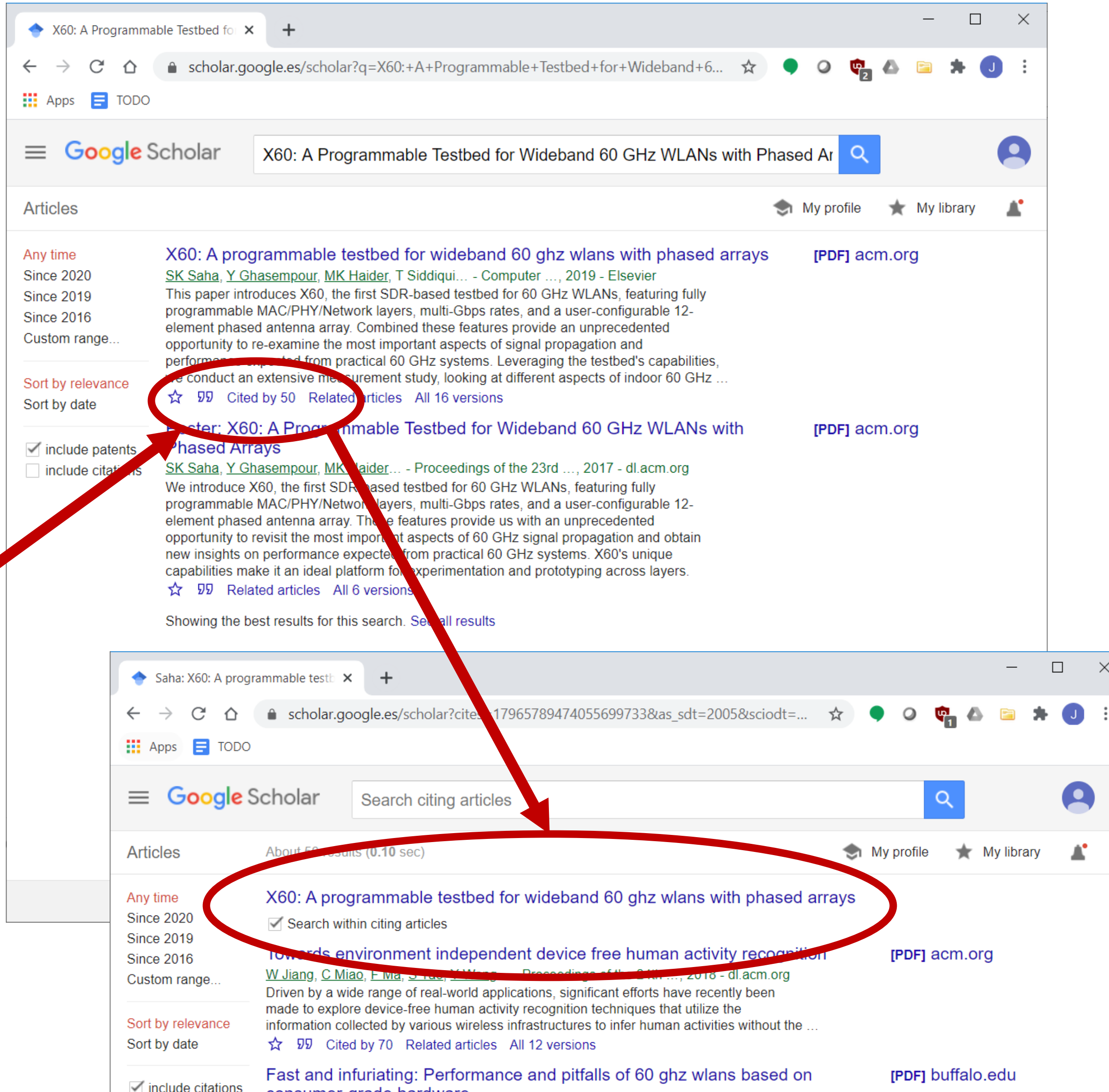
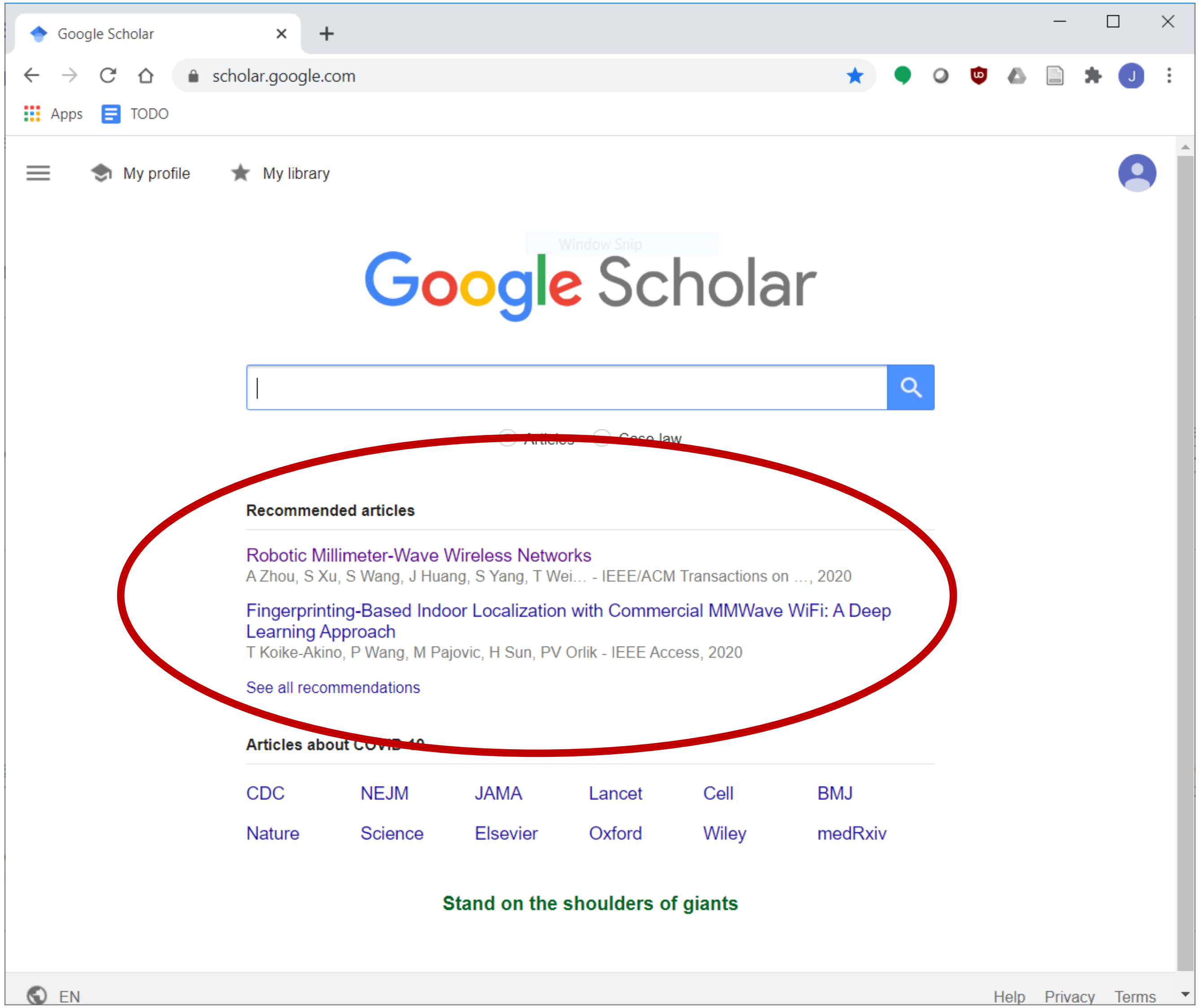
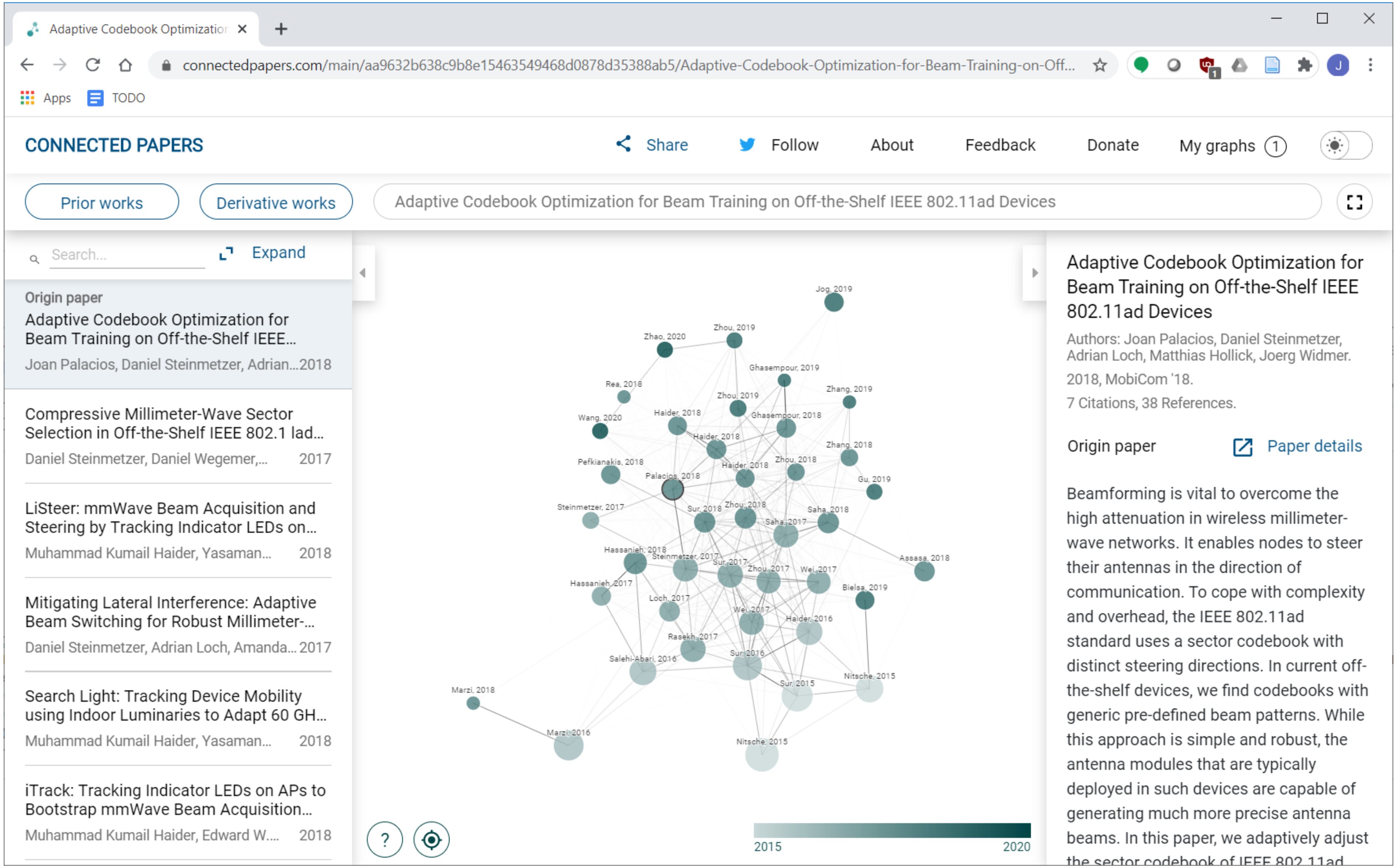


Google Scholar Recommendations



https://www.connectedpapers.com/



LLMs Use Cases for Research

- Literature Review & Summarization:
  - Prompt Suggestion: "Summarize the following research papers, highlighting their key contributions, limitations, and connections to each other: [Paste paper text/abstracts here]"
  - Advanced: Chain prompts – summarize, then compare/contrast.
- Code Generation & Debugging: done by one of you
- Writing & Editing Research Papers:
  - Prompt Suggestion: "Improve the clarity and conciseness of the following paragraph"
  - Advanced: Generate related work sections, introductions/conclusions (always verify!)
  - *Careful with research ethics; need to disclose AI use*
- Idea Generation & Exploration:
  - Prompt Suggestion: "I'm developing a new image segmentation algorithm. What are some factors (dataset characteristics, algorithm parameters, evaluation metrics) that might significantly impact its performance? Suggest some specific hypotheses I could test."
- Others
  - Data Analysis & Interpretation (patterns in unstructured data, sentiment analysis, topic modeling)
  - Translation

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YOU

Motivation

- Tenacity and hard work are the primary things required to do good research
  - Being smart helps, but it is nothing without work
  - Don’t wait for inspiration; work for a bit every day and you will make some progress, which in turn will inspire you to work some more
- Pushback may be a sign that you hit a sweet spot
  - Your insight may not have been raised before because of other people's fear of the pushback
- Collaboration is essential to stay motivated
  - Join a productive group where more senior students mentor new students
  - Find a mentor
- Selecting your own problem is more motivating than having a problem handed to you

Motivation

- ***Motivation in research and learning is often neglected***
  - Or seen as a first step, to be motivated to start something
- Adding motivators to research/learning activities
  - Action: active participation
  - Fun: enjoyable activities
  - Choice: variety of resources, different ways to advance
  - Social interaction: ensure opportunities for interaction
  - Error tolerance: “safe” environment
  - Measurement: positive “scorekeeping” system
  - Feedback: timely and constructive feedback
  - Challenge: setting challenging (self-set) goals
  - Recognition: positive reinforcement

Dean R. Spitzer

Motivation

- There's no easy way: doing research/a PhD is hard and at times you will get frustrated along the way
- Take away the pressure
  - Remind yourself that you're doing this because it's fun and you're curious!
- Breaking down a concept into its core elements, understanding these elements and then use them for your own ideas
  - This a challenge, but drives the ideas that fundamentally progress science
  - While you are formulating questions, you are already moving towards the answers

Motivation

- You’re not alone, get inspiration from others, e.g., “Surely, you're joking Mr. Feynman”
  - Feynman's way of working reflected his love for solving riddles, breaking locks, and he was fueled by curiosity (when lecturing in Brazil, he ended up playing in a samba band)
  - When “getting stuck“, go back to the basics (e.g., he wrote down the equations of motion, just to working on something and managed to get his thoughts and research back on track)
  - At Princeton Institute for Advanced Study “they have every opportunity to do something, and they’re not getting any ideas. I believe that in a situation like this a kind of guilt or depression worms inside of you, and you begin to worry about not getting any ideas. And nothing happens. Still no ideas come. Nothing happens because there’s not enough real activity and challenge: You’re not in contact with the experimental guys. You don’t have to think how to answer questions from the students. Nothing!”
- And there are many more: Tesla, Hamming, …

Time management

- There is plenty of material out there; use it
- Work in intervals (say 30-90 minutes, followed a break)
  - You cannot sustain mental productivity for 8 hours a day
- But make time for deep work
  - Dedicate continuous interruption-free time to unraveling one small subquestion of your research question
- There is no such thing as multitasking
  - Remove time-wasters; keep Whatsapp, Facebook, mail/news notifications, games, etc. separate from your work time
- Learn to say “No”, learn to delegate

Stephen Covey's Time Management Matrix

	Urgent	Not urgent
Important	1	2
Not important	3	4

S. Covey, "The Seven Habits of Highly Effective People"

Randy Pausch Lecture on Time Management

- Make a plan
  - Failing to plan is planning to fail: Why am I doing this? What is the goal?
  - You can change a plan, but only if you have one
- Doing things right vs. doing the right things
  - Remember the 80/20 Rule
- Break things down into small steps and do the ugliest thing first
- Avoiding Procrastination
  - Doing things at the last minute is much more expensive than just before the last minute
  - Deadlines are really important: establish them yourself!

https://www.youtube.com/watch?v=oTugjssqOT0

Write It Down!

- Keep a research journal, try to write in it every day
  - Keep track of what you have done, why you have done it, and what is/is not working
  - Easy to forget the details of what you worked on two weeks ago, let along half a year ago (you'd be surprised how fast you forget things)
  - Revisiting thoughts prevents you from reinventing the wheel
- Write down your research problem and ideas
  - Great exercise to organize your thoughts
  - Great practice for paper writing
  - … and it helps when you're stuck
- Weekly/monthly progress monitoring
  - Most important results and insights from last week/month
  - What deviations were there and why
  - Goals for next week/month

Tools

- Whatever works for you, but some are a must
  - Calendar
  - Ordered todo list
- Organize the research papers you read
  - Annotated bibliography to (Mendeley, Zotero, txt, …)
  - Google Scholar, IEEE Xplore, …
- Email, Skype, Slack, Sharepoint, Trello, Github, mind maps, …

Research Ethics

- Always maintain scientific integrity:
  Trust is the basis of scientific relationships!
  – Nothing hurts you research more than losing the trust of others
  – Full session on this on Thursday!
- This not only refers to plain fraud (falsifying results or omitting what doesn’t suit you), but just as much trying not to kid yourself (confirmation bias)
  – Careful, this starts small; fight it every step along the way!
- Be very careful with (self-)plagiarism: never copy, use proper citation
- Fortunately our area is also changing: stronger focus on repeatability, verification, open access to data, …

Survival

- Prepare for rough times
  - Doing a PhD/research isn’t easy (see presentation tomorrow)
  - … and it doesn’t get easier afterwards, you just get more used to it
- Imposter syndrome
  - Happens to many/most of us
  - Especially researchers: failure is part of the job
- Don't take it personally
  - There will be many rejections (and acceptances)
  - A sense of self-worth should come from more important things

Work life balance

- Eat, sleep, and exercise above all else
  - Exercise is a great way to keep your brain fresh and stress-free
  - Huge difference in mental clarity and focus
  - Helps avoid tunnel vision

Summary

- Plan your work
- Know what you're doing and why
- Read, write!
- Find out what works for you
  - One size does not fit all
- Have fun!

Thank you

Resources

- Eva Lantsoght, "The A-Z of the PhD Trajectory: A Practical Guide for a Successful Journey"
- Ken Blanchard and Spencer Johnson, "The one minute manager"
- Stephen Covey, "The Seven Habits of Highly Effective People"
- How to do good research
  - https://www.site.uottawa.ca/~bochmann/Projects/how-to-do-good-research/index.html
  - https://terrytao.wordpress.com/career-advice/
  - https://www.cs.cmu.edu/~mleone/how-to.html
  - https://dspace.mit.edu/handle/1721.1/41487
  - http://www.cs.cmu.edu/~mblum/research/pdf/grad.html
  - http://www.cs.utexas.edu/~EWD/transcriptions/EWD06xx/EWD637.html
  - http://www.paulgraham.com/hamming.html

Resources

- How to choose a research topic?
  - https://isrl.byu.edu/wp-content/uploads/2015/05/How-to-Choose-a-Research-Topic.pdf
  - https://www.chronicle.com/article/Choosing-a-Research-Topic/45641
- How to Write Research Papers
  - https://users.cs.northwestern.edu/~kch670/useful/writepapers
  - https://www.cs.tufts.edu/~nr/pubs/two.pdf
  - And countless more