HOW TO GET GOOD IDEAS

and how to publish them!

Giuseppe Antonio Di Luna

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CYBER INTELLIGENCE AND INFORMATION SECURITY

INTRODUCTION

- Hi! I'm Giuseppe Di Luna

- Associate Professor Sapienza University of Rome

- One Line CV: Phd Sapienza 2011-2015, University of Ottawa 2015-2017, Aix-Marseille and CNRS 2017-2018, Sapienza 2019-Now.

- Research interests:

  - Distributed Computing Theory;

  - Systems and Security research:

    - Systems: compilers

    - Security research: DNNs for binary analysis.

INTRODUCTION

How can I get
good research ideas?

How can I transform ideas
in solid research?

How can I publish my research?

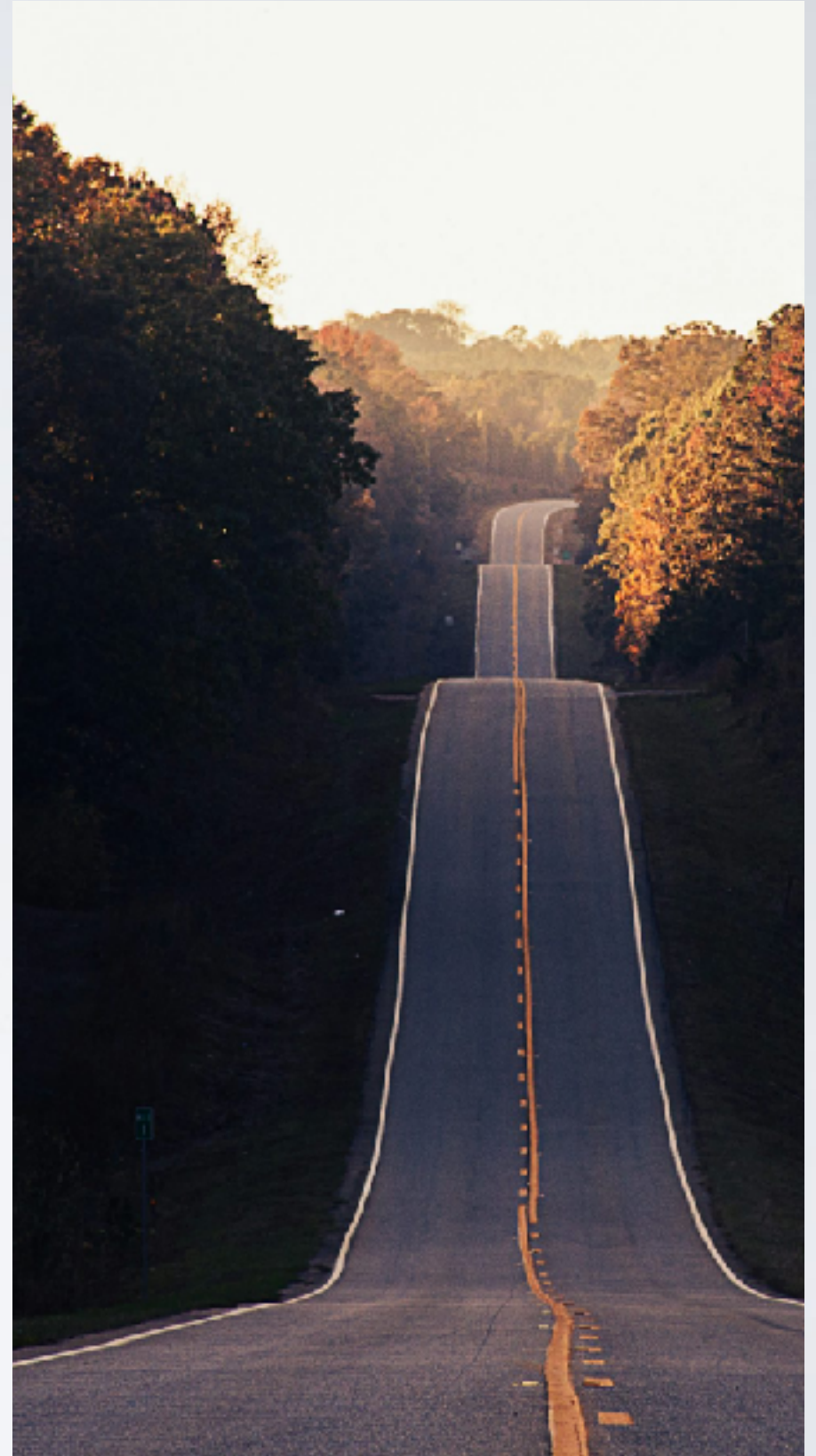Ph.D. /Master student **before** this lecture

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INTRODUCTION

Ph.D. /Master student **after** this lecture

OUTLINE

- How to get good ideas:
  - Finding good papers and books;
  - Interacting with your community.
- From an idea to a research project:
  - Get "good" results: show that your idea is useful.
  - Be ready for bad outcomes!

HOW TO GET GOOD IDEAS

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This has never worked for me!

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This has never worked for me! You HAVE to: read, read and read again.

GETTING GOOD IDEAS

- The best way to get good ideas is to be exposed to a lot of good ideas.

- How to get exposed to good ideas:

  - Reading many papers in your area of interest; (but also in other areas).

  - Discussing with others (conferences, your research group, your peers,…)

- Once you have reached a critical threshold of knowledge, you can start thinking about something new.

FINDING GOOD PAPERS

- Each research community has a set of core venues in which results are published. Venues are usually ranked by the community in tiers.

- Examples:

  - For Theoretical Distributed Computing:

    - Conferences: PODC/DISC (First tier), SIROCCO/OPODIS (Second Tier), SSS (Third Tier).

    - Journals: Distributed Computing, Theoretical Computer Science, Information and Computation, IEEE TPDPS.

  - For Systems Papers:

    - ASPLOS/ EuroSys/ SOSP, OSD, NSDI (First tier); ICDCS/Middleware/SRDS (Second Tier).

  - For Security Papers:

    - S&P, CCS, NDSS, Usenix Security, EuroS&P (First Tier); DIMVA/AsiaCCS/RAID (Second Tier)

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  - For Systems Papers:

If you do not know the core venues in your community, look them up on Google (or ask your mentors)

You should always check papers published in the core venues of your community!

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DBLP   BROWSE   SEARCH   ABOUT   NFDI

For some months now, the dblp team has been receiving an exceptionally high number of support and error correction requests from the community. While we are grateful and happy to process all incoming emails, please assume that it will currently take us months, to read and address your request. Most importantly, please refrain from sending your request multiple times. This will not advance your issue and will only complicate and extend the time required to address it. Thank you for your understanding.

Joint Declaration: The freedom of science is at the heart of liberal, democratic societies. Without this freedom, it is impossible for scientific efforts to be geared toward gaining knowledge and facts. It is therefore extremely worrying that the scientific freedom increasing pressure in various regions of the world. (read more)

dblp
computer science bibliography

search dblp

46th SP 2025: San Francisco, CA, USA

> Home > Conferences and Workshops > SP

Marina Blanton, William Enck, Cristina Nita-Rotaru:
IEEE Symposium on Security and Privacy, SP 2025, San Francisco, CA, USA, May 12-15, 2025. IEEE 2025, ISBN 979-8-3315-2236-0

Yanzhong Wang, Ruigang Liang, Yilin Li, Peiwei Hu, Kai Chen, Bolun Zhang:
TypeForge: Synthesizing and Selecting Best-Fit Composite Data Types for Stripped Binaries. 1-18

Linkang Du, Xuanru Zhou, Min Chen, Chusong Zhang, Zhou Su, Peng Cheng, Jiming Chen, Zhikun Zhang:
SoK: Dataset Copyright Auditing in Machine Learning Systems. 1-19

Justin Petelka, Benjamin Berens, Carlo Sugatan, Melanie Volkamer, Florian Schaub:
Restricting the Link: Effects of Focused Attention and Time Delay on Phishing Warning Effectiveness. 1-19

Amin Abdulrahman, Felix Oberhansl, Hoang Nguyen Hien Pham, Jade Philipoom, Peter Schwabe, Tobias Stelzer, Andreas Zankl:
Towards ML-KEM & ML-DSA on OpenTitan. 1-19

Yechao Zhang, Yuxuan Zhou, Tianyu Li, Minghui Li, Shengshan Hu, Wei Luo, Leo Yu Zhang:
Secure Transfer Learning: Training Clean Model Against Backdoor in Pre-Trained Encoder and Downstream Dataset. 1-19

Tina Marjanov, Alice Hutchings:
SoK: Digging into the Digital Underworld of Stolen Data Markets. 1-18

[-] SPARQL queries

[-] Refine list

showing all 256 records

refine by search term

refine by author
Daniel Genkin (4)
Zhiyun Qian (4)
Aniket Kate (4)
Zhikun Zhang (3)
Darya Kaviani (3)
Sascha Fahl (3)
Minhui Xue (3)
Yanjie Zhao (2)
Tommaso Frie (2)
Adam J. Aviv (2)

If you do not know the core venues in your community, look it up on Google (or ask your mentors)

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HOW TO GET GOOD IDEAS... AND PUBLISH THEM

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WHAT IS A GOOD PAPER?

- A good paper is any paper that teaches you something interesting for your research, and that is correct (or likely to contain only minor mistakes).

- **Something interesting for your research:**

  - Standard techniques used in your field;

  - Standard way of evaluating systems in your field;

  - Standard terminology of your field;

  - Current hot-topics of your field;

  - An innovative and cool technique that you have to know;

  - Interesting open problems (*);

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WHAT IS A GOOD PAPER?

- A good paper is any paper that teach you something interesting for your research, and that is correct (or likely to contain only minor mistakes).

- **Correct or almost correct:**

  - If the things you are learning are incorrect then you are doing more harm than good.

This is a big deal!
Many published papers contains minor or major problems!

If you base your research on a problematic technique (or a wrong lemma/theorem/...)
Then you are in for a bad time!

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- **Correct or**

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Reading papers published in good peer reviewed venues greatly reduces this probability.

This is a big deal!
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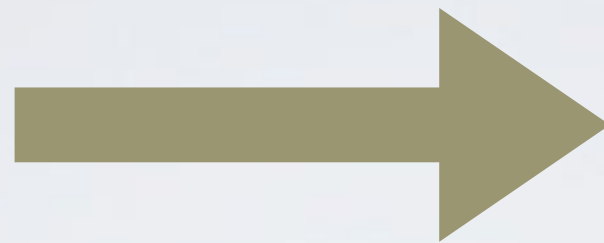Then you are in for a bad time!

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REAL LIFE EXAMPLES - DIST COMP

- At DISC 2012 Othon Michail, Ioannis Chatzigiannakis and Paul Spirakis published a Brief Announcement: Naming and Counting in Anonymous Unknown Dynamic Networks.

- In this paper they presented preliminary results on anonymous dynamic networks.

- Novelty: Studying anonymity in dynamic environments.

- Why it was a really good paper?

  - It was the first to study a model that was neglected by others.

  - It was conjecturing that some computations where impossible.

  - During the reading of the paper I had several intuitions on how to solve things in new ways.

- Starting from this paper I published several works that then composed my Ph.D. Thesis.

REAL LIFE EXAMPLES - SECURITY

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TRAIN YOURSELF TO HAVE IDEA WHILE READING

Read abstract and intro → Finish reading the paper and compare their solution with your idea

Stop for 20 minutes: think how you would have designed the system/solved the problem/ecc

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INTERACTING WITH OTHERS

- Interacting with others in your or a closely related area is also a good way to get exposed to good ideas.

- One way to know people is at conferences.

- People are not only a good source of ideas but also possible coauthors.

BRAINSTORMING

- In a brainstorming section you try to find new idea by talking with others.

- In a brainstorming session often people are afraid to critique other ideas.

- There is research showing that avoid to critique lead to less discoveries: https://onlinelibrary.wiley.com/doi/abs/10.1002/ejsp.210

REAL LIFE EXAMPLES - COMPILERS

```c
int a, b, c;

int main()
{
  {int ui1 = 5, ui2 = b;
    c =
    ui2 == 0 ?
     ui1:
     (ui1 / ui2);
    }
}
```

Clang bug 46009 at –Og.

```
(lldb) b main Breakpoint 1: where = opt`main at a.c:4:26,
address = 0x0000000000400480
(lldb) r
Process 65 launched Process 65 stopped frame #0:
0x0000000000400480 opt`main at a.c:4:26 1
 -> 4 {int ui1 = 5, ui2 = b;
(lldb) s
Process 65 stopped
frame #0: 0x0000000000400486 opt`main at a.c:8:16
-> 8 (ui1 / ui2);
```

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SYSTEMATISING RESEARCH IDEAS

	Requires	Risk	Impact
Finding a new Problem	High Creativity High Knowledge of the field	High	Mixed (could be low or high)
Solving a problem in a better way	Good Knowledge of the field Creativity Technical competence	Medium	Medium
A systematic analysis (survey, benchmark,..)	High knowledge of the field Technical competence Patience and precision	Medium/low	High (if done properly and filling a knowledge gap)

IS IT A GOOD IDEA?

- A, not so serious, quantitative approach. For a good idea it should usually hold:

$$P(\text{Having Idea}|Knowledge) > P(\text{Having Idea})$$

- while

$$P(\text{Understanding Idea}) \approx P(\text{Understanding Idea}|Knowledge)$$

HOW DO YOU KNOW IF AN IDEA IS A GOOD IDEA?

- Many bad or mediocre idea initially seems good:

  - Fase 1:

    - Wait a few days and see if the idea still seems good to you.

    - Discuss your idea with your peers (possible co-authors), be open to accepting and welcoming even harsh feedback (an harsh but fair and honest feedback now will save you a lot of time later).

  - Examples of ideas that I will immediately shoot down:

<div style="background-color:#d99a1a; color:white; text-align:center; padding:20px;">

I want to detect malware with an LLM

</div>

This is not an idea; it's a vague proposal in a field (malware detection) that is saturated with ML methods. What is the novelty? What do you mean by LLM? How would you prove that your system works against the 100 other methods?

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  - Examples of ideas that I will immediately shoot down:

I want to show that the upper bound for matrix multiplication is n^2.34….

This is not an idea is a vague proposal
on a problem that has been studied for decades, and for which in the last 10 years the progress has been always done by a restricted set of scientists.

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  - Examples of ideas that I will immediately shoot down:

  > I read paper Z that is solving T using method M; I think that method M can be improved by using K. There is paper X that shows that K is SoTA at doing this function that is used in M.

  This is an idea that i would really like. It is not vague, it has inside a rough plan of what has to be done, and it carries an intuition on why it should work.

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  - Examples of ideas that I will immediately shoot down:

  I read paper Z that is proposing an algorithm A to solve problem P on ring graphs. I would like to solve P on grids using some ideas from A.

  This is an idea that I like. It is less clear than the previous one, at this point there is no intuition that what has been proposed is possible, but for certain problems this extension could indeed by viable.

HOW DO YOU KNOW IF AN IDEA IS A GOOD IDEA?

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    - Discuss your idea with your peers (possible co-authors), be open to accepting and welcoming even harsh feedback (an harsh but fair and honest feedback now will save you a lot of time later).
  - Examples of ideas that I will immediately shoot down:

I read many papers on topic T, and I saw that no one is studying problem P. P is a reasonable problem because of X. I would like to study P.

This is an idea that I like. Spending few weeks to study P could be rewarding.

HOW DO YOU KNOW IF AN IDEA IS A GOOD IDEA?

- Many of your good ideas have been already thought of by someone before.
  - Fase 2:
    - Read, read, read the literature to see if your idea is really new.
    - Spend at least a week searching everything that is related to your idea on Google Scholar.
    - A week now is better than discovering a week before the submission that your idea is not so novel as you expected.
    - If you are not an expert of the domain of the idea, or you are not working on it with experts, it could be good to look for a trusted one that could vet the novelty of your idea.

A REAL LIFE DEBACLE

- When I was a Ph.D. student I thought about studying how *to triangulate noise origin in a graph.*

A REAL LIFE DEBACLE

- When I was a Ph.D. student I thought about studying how *to triangulate noise origin in a graph.*

- I spent a few months doing research on it.

- I sent a paper to a conference.

- I received this review: "This paper studies a problem that the authors call "minimum observer". To me it seems to me that minimum observer is a variant of a classical graph problem, "metric dimension", yet the paper does not mention the connection to metric dimension, or cite any prior work related to it. […]"

- It turned out that my idea was related to the metric dimension problem studied by Slater in 1975.

- Upset, I gave up and the paper is still in one of my drawers unpublished.

WHAT TO DO IF YOUR IDEA WAS DISCOVERED BEFORE

Keep calm

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Related prior work is an indication that, at least, your idea was indeed good.

WHAT TO DO IF YOUR IDEA WAS DISCOVERED BEFORE

<div style="background: gray;">Keep calm</div>

- If your idea has enough technical complexity the probability that what you found is **exactly** your idea is low.

- The best course of action is to carefully assess all the differences between your idea and the related literature (do not immediately give up).

- There could be still enough novelty for publication if:

  - The differences are meaningful enough (e.g.; someone is using your exact neural network architecture on your exact same problem but the training strategy is different)

  - The setting is different and yours is especially important (someone uses your exact technique to solve a problem in a setting that is irrelevant, and you show that the technique applies to a relevant setting that brings improvement on a real-life problem).

  - The setting is the same the technique looks similar but you have better results (e.g. +5 points of Recall, a better running time,…).

LOW HANGING IDEAS ARE (KIND OF) DANGEROUS

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HOW TO GO FROM AN IDEA TO A RESEARCH PROJECT

FROM IDEA TO USEFUL RESULTS

Idea → Results

Hard Work

Refinement

- Once a promising idea has been identified, there is the need to validate it to obtain positive (or sometimes negative) results.

- Negative outcomes are also outcomes, and you can publish them if they tell an interesting story. Examples of important negative outcomes:

  - You show that your algorithmic idea to solve problem P does not work because solving P is impossible (in your setting or for a certain family of algorithms).

  - You show that a cool neural architecture, for example a transformer, is not good for your setting because of certain non-obvious peculiarities.

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REACHING A MINIMAL WORKING EXAMPLE

- You should try to reach, somehow fast, a small example that could also be a really restricted toy setting, on which your idea shows promising results. This toy setting helps you to develop the methodology and the tools needed to obtain publishable results.

Example 1

IDEA: You want to solve the problem of counting the number of processes in a network. Where processes may experience a memory corruption (self-stabilizing)

MWE: An algorithm that counts on a line graphs when only few processes may have failure.

Example 2

IDEA: You want to identify debug information bug in a compiler.

MWE: a technique that finds a specific kind of bug (wrong variable value) on a single compiler (gcc) on a really small dataset (100 programs).
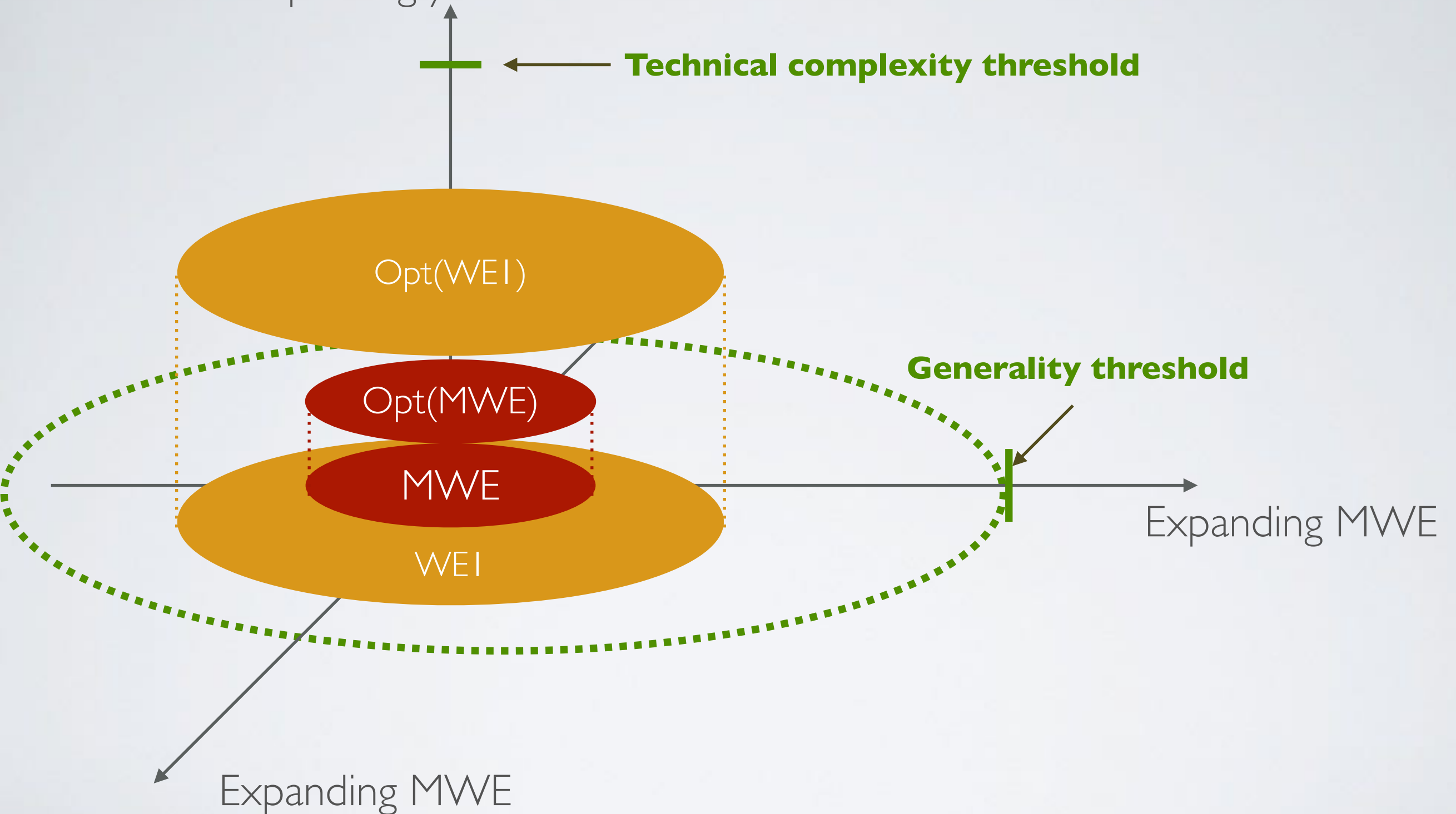
MWE	Restricted enough to be easier to be solved fast
	General enough to not be trivial

FROM MWE TO ENOUGH RESULTS

Improving your solution

Technical complexity threshold

Opt(WE1)

Generality threshold

Opt(MWE)

MWE

WE1

Expanding MWE

Expanding MWE

HOW TO PROGRESS (THEORY FIELD)

- Brainstorming.

- Adversarial collaboration: https://en.wikipedia.org/wiki/Adversarial_collaboration

  - Researcher 1: Try to show that an algorithm can be built with a certain running time.

  - Researcher 2: Try to show that what Researcher 1 is doing is impossible.

- By discussing you have a nice dynamic.

- Design fixation (https://www.sciencedirect.com/science/article/abs/pii/0142694X9190003F):

  - a blind adherence to a set of ideas or concepts limiting the output of conceptual design.

HOW TO PROGRESS (EXPERIMENTAL FIELD)

- Brainstorming.

- Adversarial collaboration: https://en.wikipedia.org/wiki/Adversarial_collaboration

  - Researcher 1: Propose a design or a system.

  - Researcher 2: Try to demolish the design and system.

- This saves times.

- Design fixation (https://www.sciencedirect.com/science/article/abs/pii/0142694X9190003F):

  - a blind adherence to a set of ideas or concepts limiting the output of conceptual design.

DOING RESEARCH IS AN HARD WORK

- Do not be lazy, be precise keep everything logged.

- For experimental research:

  - Make a monthly research plan where you program the research activities of the month. The plan should have a rough estimate of the time you will allocate to each activity.

  - Keep a diary of your experiments, log meticulously: setting, experiment, data on which the experiment was performed, outcome.

  - Always reason on the outcome of your experiments and on your code.

    - Thinking will save you a lot of time and pain.

- Fatherly advice: DO BACKUPS, DO BACKUPS DO BACKUPS!

DOING RESEARCH IS AN HARD WORK

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- Backups horror stories 1:

  - Our research group had a powerful physical sever that was running several virtual machines.

  - We were using one of that VMs to keep the coding environment of one of our research project.

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  - My students were minimally impacted since they were doing weekly backups.

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- Backups horror stories 2:

  - One of my Ph.D. student was working on the main paper of his dissertation. He had backup of the code on the cloud and GitHub, but he left the data used to generate some figures on our DGX A100.

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    - We had to scramble by borrowing hardware around (is not easy to find 8 A100 GPUs) to finish our experiments and redo the figures.

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  - We had to scramble by borrowing hardware around (is not easy to find 8 A100 GPUs) to finish our experiments and redo the figure.

  - We made it for the deadline just in time!

  - **YOUR PH.D. COULD BE LITERALLY SAVED BY BACKUPS.**

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YOU HAVE TO KEEP A RESEARCH/LIFE BALANCE

- If you burn yourself out you will likely publish less than if you keep things under control.
- (Sleep inspires insight) https://www.nature.com/articles/nature02223

HOW TO PUBLISH YOUR RESEARCH

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QUICK TIPS

- Decide an appropriate venue.

- Write things clearly.

- Write things for your venue.

- Rushing is never good.

- Be prepared to see also good paper rejected (it happens to everyone).

- Be resilient to rejections, but be open to receive feedback (even harsh ones).

- Other resources:

  - Lamport: https://lamport.azurewebsites.net/pubs/lamport-how-to-write.pdf

CONCLUSION

- Good ideas do not grow on trees. They are usually the fruit of an extensive prior hard work.

- You will likely never have a good idea without a solid prior: start reading.

- Once an idea is identified try see if you like the structured approach that I suggested.

- That is it.

- Good research to you!

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CONCLUSION

- Good research to you!