How to Write a PhD Thesis

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About This Talk

- Not a new topic …
- Rich body of literature
  - books, articles, web pages
- Little of it is outright controversial
- Compare with my own experience
  - as PhD student
  - as PhD advisor
- Concentrate on points I haven't found in the literature

How to Get a PhD

Everyone knows what the steps are:

- Choose a topic
- Write a proposal
- Do the research (and publish)
- Write the thesis
- Defend it
- Party

… or are they?

Choosing a Topic

What an ideal topic is like:

"The right topic will be interesting to you, complex, and compelling"

(*R. M. Reis 1999*)

"The most successful research topics are

- *narrowly focused and carefully defined*
- *but are important parts of a broad-ranging, complex problem"*

(*C. Davidson and S. Ambrose 1994*)

The PhD Topic Checklist

(R. Smith, 1984)

1) *Can it be enthusiastically pursued?*
2) *Can interest be sustained by it?*
3) *Is the problem solvable?*
4) *Is it worth doing?*
5) *Will it lead to other research problems?*
6) *Is it manageable in size?*
7) *What is the potential for making an original contribution to the literature in the field?*

The PhD Topic Checklist

(R. Smith, 1984)

8) *If the problem is solved, will the results be reviewed well by scholars in your field?*

9) *Are you, or will you become, competent to solve it?*

10) *By solving it, will you have demonstrated independent skills in your discipline?*

11) *Will the necessary research prepare you in an area of demand or promise for the future?*

However, What If ...

- Enthusiasm fades?
- The problem has an easy part,
  but the rest is more difficult than foreseen?
- The difficult part requires skills beyond your scope?
- The problem is much larger than expected?
- The problem is/becomes easier than expected?
  (e.g., someone produced a key result in the meantime)
- You develop a new method, but it is only applicable to the special case you are studying?
- The interests of the research community are shifting?

… a PhD takes at least 3 to 5 years

If It's Research, You Don't Know the Outcome

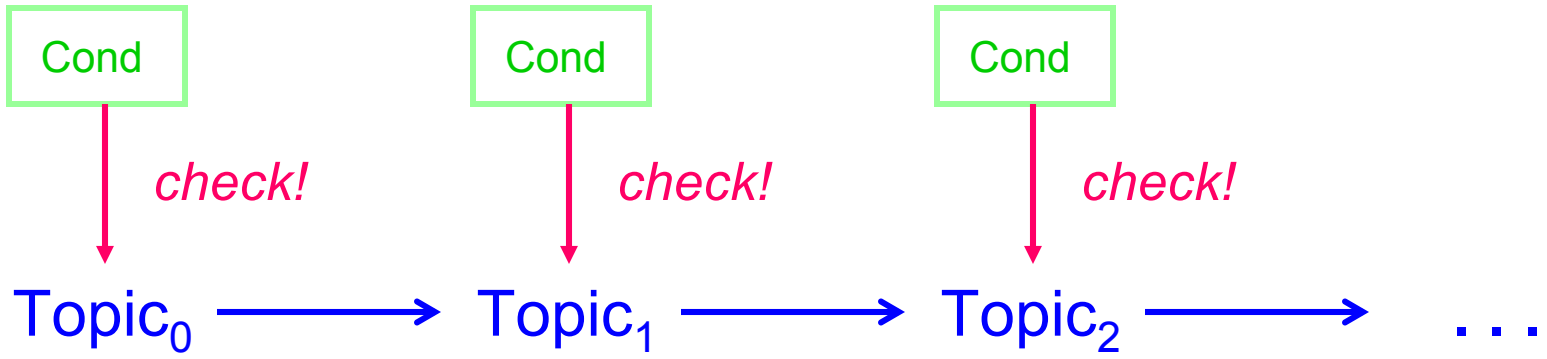
Topics evolve over time

Don't view the topic checklist

- as a static condition,

  (check at the beginning, and off you go)

- but as an invariant that needs to be maintained



Taking Corrective Action

Enthusiasm fades?

- It happens to everyone.
- Take a break.
- Work towards some short-term goal that looks achievable.

The problem has an easy part,
but the rest is more difficult than foreseen?

- The PhD is an adventure.
- Spend some time giving it a try.
- Look for help (anywhere in the world . . .).

Note: These are examples, not recipes!

Taking Corrective Action (cntd.)

The difficult part requires skills beyond your scope?

- Keep the results you already have.
- Find a related new problem.
- Redefine the topic so that it covers both your old work and the new.

The problem is much larger than expected?

- Typical situation.
- Focus on one essential aspect.
- You only need to make a *specific contribution* to a *broader field*.

How Do I Find My Topic?

- How important is the *specific topic*?
- What is the PhD about?
  - intellectual adventure
  - learning the craft of the researcher

    $\Rightarrow$ the thesis is the evidence that you know your craft
- How would an apprentice choose a workshop?

Choosing the right research environment is more important than choosing the topic!

Join the Right Research Group!

A group

- working on a reasonably wide subject

  (some application problem, if possible)

- whose work attracts you
- that is well-integrated into the research community
- has a cooperative work style
- has resources

Probably the most important choice for your PhD!

Once You Are There ...

- Find out what the others are working on and identify
  - missing principles
  - possible extensions or generalisations
  - ways to do things differently
- Ask your advisor for ideas — but be skeptical!
- An application within your reach can give you a leading edge:
  - problem generator
  - testbed

Find your problem!

If Your Current Group Is Not Like That ...

- Join another group

  (*usually difficult)*
- Look for a co-advisor
- Go to another place as a visitor
- Start a cooperation with another group
  - e.g., within an EU project

"Research propagates by osmosis"

$\Rightarrow$ *Make sure you work in a conducive environment*

What is a (Good) Research Topic?

- Research is about finding out
  ⇒ *Research should be guided by questions!*

- Research should be relevant
  ⇒ *The answers should be useful for some purpose!*

- Research should be explorative
  ⇒ *The question should not anticipate the solution!*

- Research results need to be concrete
  ⇒ *The question should allow for specialisation!*

Examples (Real)

- "How can one answer XPath queries over sources that contain answers to other XPath queries?"

  Relevant question (2-5 years ago), some result is guaranteed

  \+ Broad ranging and complex $\Rightarrow$ allows for specialisations

  \+ Permits theoretical and empirical approaches

  +/– Mainstream $\Rightarrow$ Competitive!

- "How can one pose queries over XML data without knowing the schema, and still get sensible answers?"

  Interesting question (3 years ago)

  +/– Allows for many approaches, will never be solved completely

  \+ Results would be strengthened by empirical tests

Examples (Fake)

- "How can one extend a description logic by Petri nets and compute subsumption in that logic?"

  Prototypical example for "Combine X with Y!"

  – Relevance not visible

  – No criteria for success

- "Can one use theorem prover ABC to efficiently draw inferences in description logics?"

  Prototypical example for "Can X be solved by Y?"

  – Concentration on ABC too narrow

  – Failure if answer is "no"

  – Failure if answer is "yes", but encoding is simple

The Proposal

Marks transition from preparatory to "full steam" phase

Purpose:

- Clarification of the goal
- Identification of possible steps *("divide and conquer")*
- Yardstick to measure own achievements
- Laying claim to one's own terrain
- Seed text for the thesis

Proposal Structure

- Definition of the topic

  (= broad problem + special focus)

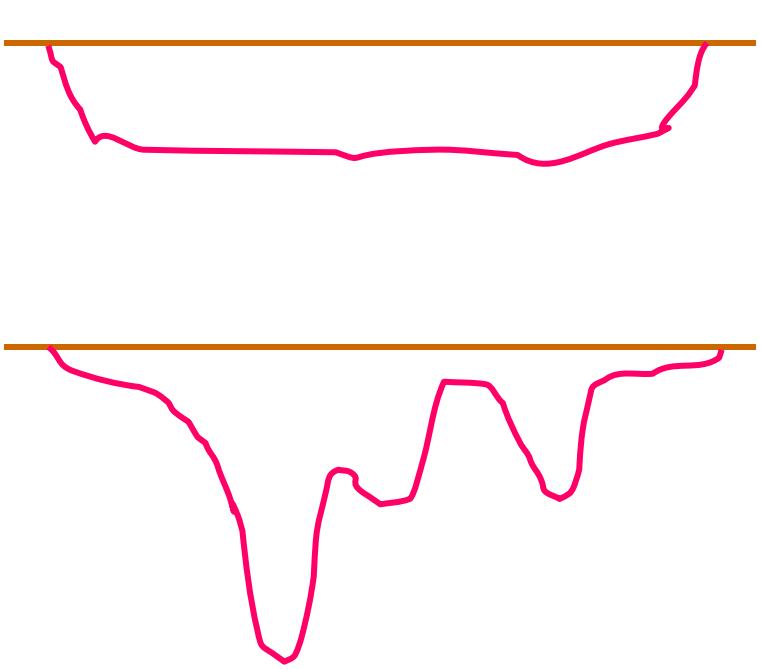
- Survey of related work

  (= What is known, what may be useful?)

- First insights and results
- What will a solution look like?
- Plan of action
- Thesis outline

(cf. H.C. Lauer 1975)

The Survey of Related Work: How Deep Should I Dig?



?

?

In the end, you want to find out something new yourself

⇒ understand the details

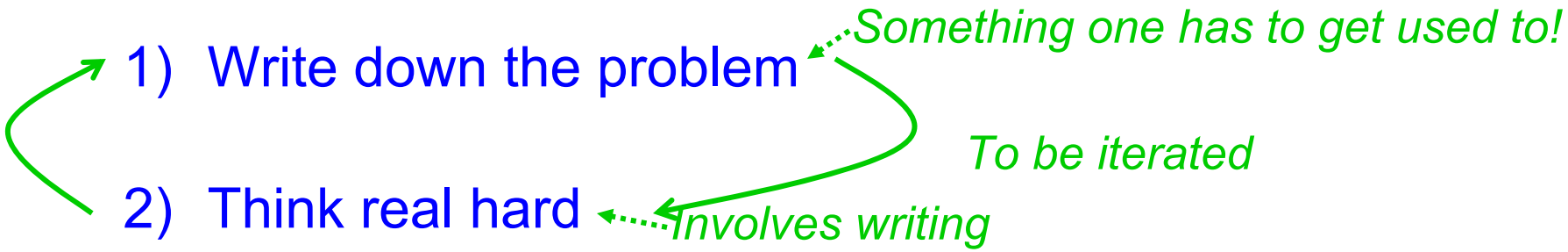
⇒ identify shortcomings

⇒ solve open questions

A small concrete problem can be an excellent guide to the literature

Research is Writing

The Feynman Problem Solving Algorithm



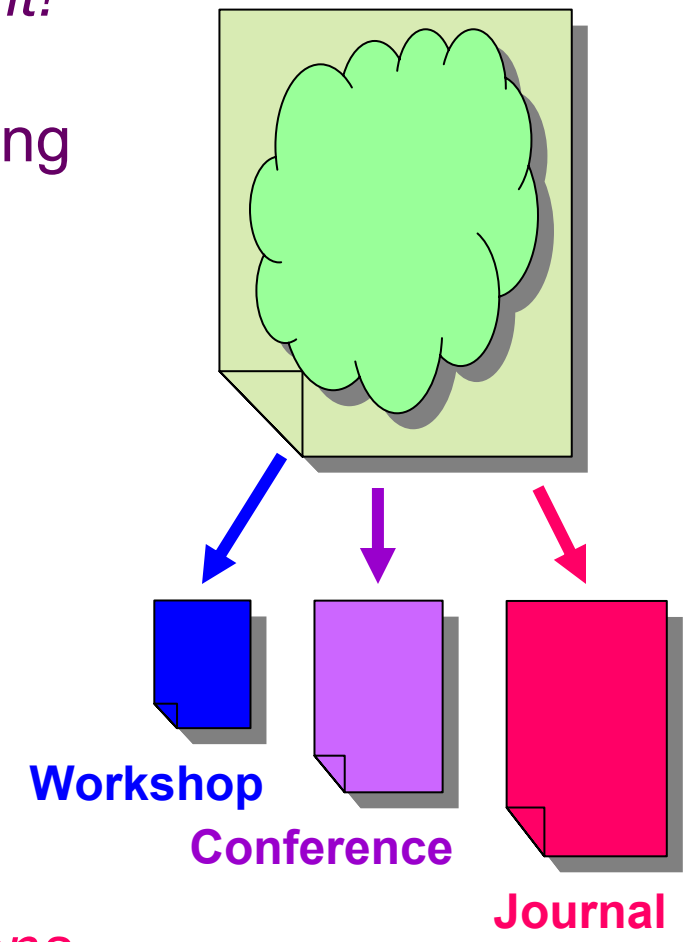
M. Gell-Mann

Not only a joke!

Paper Writing: (Ideal) Strategy

Doing research = writing a document!

- Theory: Outcome is always in writing
  - Framework
  - Theorems and proofs
- System: Create a paper trail!
  - Goals
  - Design, including alternatives
  - Algorithms



Results in a rough large report

⇒ *quarry for publications*

When to Stop

Requirement: Make a significant contribution to research …

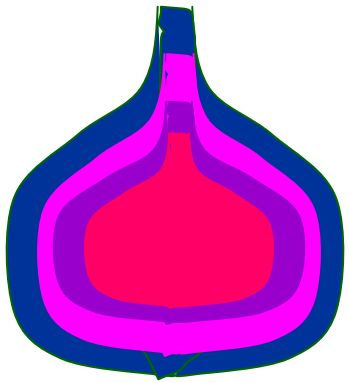
Constraint: ... within a time of 3, 4 years

"Dissertations are not finished; they are abandoned."

(Fred Brooks)

⇒ "Ognion" approach:

- Start with a core result
- Add layers, as time permits



Writing It All Up

- The subjects of Computer Science are immaterial
  (Concepts, formalisms, languages, algorithms, ...)
- "Writing Up" is only feasible if the previous research has produced results in writing
- ... otherwise "Writing up" turns out to be the research
- Idea: maintain the thesis outline as a living document
  - check your progress
  - change it when necessary

Note: Writing up involves new research

Conclusion

The PhD is not simply about producing a document, but

- acquiring the ability to
  - define research problems
  - and solve them
- communicating one's work
- becoming part of a community
- pursuing a changing goal in a changing landscape

The thesis provides evidence that you were successful in this