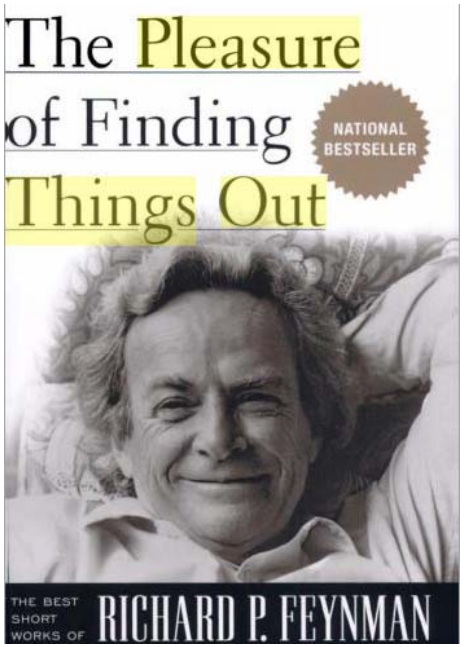
On being a professor and the pleasure of finding things out

Mihaela van der Schaar



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The path to becoming a professor does not always need to be the same: *My story*

- First-job
- PhD
- Soul searching
- Becoming a professor

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Agenda

- **Part 1: Why?**
- Part 2: Getting the job – How?
- Part 3: After getting the job
  - Your research
- Q&A

To be or not to be, this is THE question

- **My 10 "Yes!" answers:**

1. The *pleasure* of finding things out
2. The *freedom* (and duty) to "shoot for the moon" and dream to change the world
3. The *excitement* of working with the smartest people
4. The *happiness* of seeing your students grow intellectually
5. The *freedom* (and duty) to continuously learn something new

To be or not to be, this is THE question

- **My 10 "Yes!" answers (continued):**

6. *Top research?* You define it! (Good or bad news?)
7. It is all in *your* hands - Being your own boss (the goods)
8. *Your job?* Writing down your ideas and teaching the brightest students how to think.
9. *Output and social impact - guaranteed*: educate students
10. Your hobby can become your job and your job will be your best hobby

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To be or not to be, this is THE question

- **My 10 “No” answers:**

1. There is a TEST regularly, and you may fail!
2. Who cares? Does anybody read these days?
3. Am I doing all-right? Bad news: you are the final judge!
4. Frustration: you may be left empty-handed or, worse, second
5. Do you dare? It is all in your hands and nobody else to blame if you fail
6. Where are my colleagues? You have to do it alone

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To be or not to be, this is THE question

- **My 10 "No" answers:**

7. There are 100 things to do RIGHT NOW! Prioritizing may be tricky
8. It is all in *your* hands - Being your own boss (the bads… - a small start-up)
9. Funding – again and again (there is some good there as well)
10. Some just become managers – your duty as a professor (my view)

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You decided is a “Yes”, what now?

- During your PhD:
  - Do bold research – *You have to stand out!*
    - Choose a deep problem (it is also on you, not only on your adviser)
    - Think it through
    - Try to be the best in your area
  - Write high quality papers
  - Try to get the big picture (Read a lot)
  - Learn to give good talks
    - go to faculty candidates job talks at UCLA
  - Make connections and stand out (reference letters)

Interview preparation

- Where do you want to go? Do your homework
- Be familiar with the university and its research
- Standout! Sell yourself! (see previous page)
- Protocol 101: Be polite, show interest, send “thank you” notes

You have the job, what now?

- Defining a vision
- Student recruitment
- Funding (NSF vs. companies)
- NSF Career proposal – why is a good idea
- Selling your work

  "You have to learn to write clearly and well so that people will read it, you must learn to give reasonably formal talks, and you also must learn to give informal talks." [Hamming]
- The 10 "yes"-es – work on them!

My definition of high-quality research

- High quality research is ….
  - a *new* information source
  - a *new* way to process information that significantly reduce the entropy of such information sources

- Bad research is … methods that provide only *limited entropy decrease given the current state of knowledge* on *known* information sources

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My definition of high-quality research

- Ask bold questions, and work on important, significant problems
  - Classes of problems, canonical examples
  - New theories
  - New dimensions
  - New metrics
  - New formalisms

  But also,
  - Design principles
  - Structural results
  - New constructive designs and systems
  - Impossibility results

- ***Have courage!*** ***"Once you get your courage up and believe that you can do important problems, then you can"***

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Identifying and defining research topics: some examples from my own research

- Stop/limit pursuing incremental research
- New theory for communication under dynamics
- New design principles and structural results for delay-sensitive communication
- New theory for multi-user communication & networking
  - Network economics and game theory
  - Learning in games

Funding

- Never forget that funding is the means, not the goal
- Goal is high-quality research and students
- However, writing grants can be good for your research: spell out the big picture, what is your contribution, position your work, set bold milestones

On drive [by Hamming]

``Knowledge and productivity are like compound interest.'' Given two people of approximately the same ability and one person who works ten percent more than the other, the latter will more than twice outproduce the former.

The more you know, the more you learn; the more you learn, the more you can do; the more you can do, the more the opportunity - it is very much like compound interest. I don't want to give you a rate, but it is a very high rate. Given two people with exactly the same ability, the one person who manages day in and day out to get in one more hour of thinking will be tremendously more productive over a lifetime.

I took Bode's remark to heart; I spent a good deal more of my time for some years trying to work a bit harder and I found, in fact, I could get more work done. I don't like to say it in front of my wife, but I did sort of neglect her sometimes; I needed to study. You have to neglect things if you intend to get what you want done. There's no question about this.

On drive [by Hamming]

… drive, misapplied, doesn't get you anywhere. I've often wondered why so many of my good friends at Bell Labs who worked as hard or harder than I did, didn't have so much to show for it. The misapplication of effort is a very serious matter. Just hard work is not enough - it must be applied sensibly.

Long-term

- Continue to do great research
- Have courage
- Stay focused
- Work hard and focused, on important problems

- Even if you do not become a professor, try to do “significant” research
- Why does it matter?
  - Why shouldn't you do significant things in this one life, however you define significant?

- Why is academia the best place?
  - Most people in industry are obliged to work on problems they do not think are significant or important
  - Academics have the luxury to work and should work on problems they find significant

Hamming one last time ☺

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Well I now come down to the topic, ``Is the effort to be a great scientist worth it?''

To answer this, you must ask people. When you get beyond their modesty, most people will say, ``Yes, doing really first-class work, and knowing it, is as good as wine, women and song put together,'' or if it's a woman she says, ``It is as good as wine, men and song put together.''

…

It's a biased sample, but I still think it is worth the struggle. I think it is very definitely worth the struggle to try and do first-class work because the truth is, the value is in the struggle more than it is in the result. The struggle to make something of yourself seems to be worthwhile in itself. The success and fame are sort of dividends, in my opinion. “