LICOS Centre for Institutions and Economic Performance
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LICOS Discussion Paper Series

Discussion Paper 357/2014

Trust me, I'm a Doctor: A PhD Survival Guide

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Version: October 25, 2014

Abstract. So, you've decided to do a PhD… now what? In this essay, I provide some advice for beginning PhD students – basically sharing with you what I would tell my younger self. Doing a PhD is a transformative experience, but the process is challenging – not merely on an intellectual level but also psychologically. To overcome these challenges you will need a certain mindset and a bag of tricks. I offer some help to get you in the right mindset, and I share some of my own tricks for studying, research, and productivity in general.

Note. This essay was a present to my thesis advisor Jo Swinnen and my research group LICOS on the occasion of my graduation (June 30, 2014). In addition to Jo and all my LICOS colleagues past and present, I want to thank Saule Burkitbayeva, Maria Garrone, Seneshaw Tamru Beyene, Anna Salomons, Andrea Guariso, Lotte Ovaere, Alexander Jocqué, Jo Reynaerts, Thijs Vandemoortele, Ward Neyrinck and Angela Merritt for enthusiastic feedback on earlier drafts. Special thanks to my two external jury members, Julian Alston (UC Davis) and Steve Ziliak (Roosevelt University) for being the first jury members in the history of KU Leuven to formulate questions during a PhD defense in limerick.

Please feel free to circulate this essay to anyone who might find it useful. Comments and suggestions are welcome at koen.deconinck1@gmail.com.

Trust me, I'm a doctor: A PhD Survival Guide

If everything goes well, I should have a PhD by the time you are reading this essay. That means I have a good collection of deadlines at the moment, so I should definitely not be wasting time on writing an essay that is not strictly necessary. But the urge to procrastinate is strong, and of all the ways in which I could give in to the temptation, this seems the most acceptable one.

In this essay I want to share some autobiographical anecdotes and some insights or bits of advice. You probably couldn't care less about the autobiography part, and you will probably ignore most of the advice. Yet, I hope both can be somewhat helpful to anyone starting a PhD (or at least a PhD in economics – although some of the advice might be useful for others as well). My goal here is basically to tell you what I wish someone had told me five years ago.

But before we get started: why am I writing this survival guide? By now, I've seen several new cohorts of first-years start a PhD. These people are always highly motivated and very smart. Unfortunately, being motivated and smart is not enough to survive, and everyone in academia knows some very smart people who started, but never finished, their PhD. People drop out because they feel that they are not up to the task. The tragedy is that this perception is often wrong. They have the skills and the intelligence; what is lacking is perhaps a good understanding of what a PhD is, and a bag of tricks to overcome the obstacles along the way.

Practically everyone in the academic world – *definitely* including myself – has had serious self-doubts at some point in the PhD and has considered quitting. I don't want to scare first years, but I've felt inadequate, stupid and/or unfit for the job most of the time. It has only started to feel "natural" in the past few months. Before that I constantly had the feeling that I had no idea what I was doing.

Quitting would be a shame, however, because the PhD is a transformational experience. Five years ago, when I started, I had never been on a plane in my life, I had a ponytail, and I was so philosophically oriented that I was doubting whether to do the PhD or take another four years to study some more history and philosophy and stuff. The situation at this moment is slightly different: I am writing this paragraph on a plane, my hair is a lot scarcer (which is partly the result of a conscious decision and partly genetics), and I'm considering leaving academia for the business world to learn more about how real companies work. Apart from that, I have learned a great deal – about economics, statistics, writing, presenting, teaching, organizing, and so on.

Just like many other people, I found doing the PhD to be very hard – definitely the most challenging thing I've ever done. It's difficult, but not in the same way in which a hard mathematical problem is difficult. The biggest challenges were psychological, and I suspect that it's these psychological challenges, and not the intellectual ones, that are most stressful to PhD students and the biggest cause of people giving up. However, with the right mindset and some nifty tricks you can face these challenges, as I discovered along the way. The purpose of this essay is to tell you about those two things: the mindset and the tricks, which I'll tell you about after I discuss the challenges of a PhD.

The Challenges of Doing a PhD

Let's start with the good news: if you got hired, you are smart enough to do a PhD. Now for the bad news: being smart is only necessary, but not sufficient.

Being smart means you are normally good in finding an answer to a question. In a PhD, however, you often don't really know what the question is, let alone if there is an answer at all. This is not an easy feeling for your brain, and it's easy to feel discouraged.

Your education up to this point has probably not prepared you for a PhD at all. Studying textbooks and solving problem sets is quite easy compared to trying to figure out how the hell you can make sense of the empirical results you found, or trying to figure out how you can abstract a difficult problem into a tractable theoretical model without losing the essential aspects of the problem, or trying to find statistics on an obscure topic, and so on.

As a result, during the PhD, you will quite often feel stupid. I'm here to tell you that this is normal and even expected. The biologist Martin Schwartz wrote a short but brilliant essay called "The Importance of Stupidity in Scientific Research," which I strongly recommend. He makes the point that

> if we don't feel stupid it means we're not really trying. I'm not talking about 'relative stupidity', in which the other students in the class actually read the material, think about it and ace the exam, whereas you don't. I'm also not talking about bright people who might be working in areas that don't match their talents. Science involves confronting our 'absolute stupidity'. That kind of stupidity is an existential fact, inherent in our efforts to push our way into the unknown. (Schwartz, 2008).

I put his essay up on the door of my office and I've read it over and over again – my first concrete advice to you would be to read his essay. In fact, to make your life easier, I've attached it to this essay as an appendix, so you have absolutely no excuse.

There's an extra challenge when you start a PhD, which is that many people starting together with you feel insecure and therefore don't want to admit they are having difficulties and doubts as well. Of course, if nobody admits that they have a hard time understanding econometrics or that they have serious doubts about their capacities, you will have the impression that all the others in the classroom or in the office must be super geniuses – that everyone in your PhD program is Einstein, except for you. They are not. In fact, they are all worrying about the same things, and the best strategy here is to become friends, share your doubts and worries, encourage each other and try to solve problems together. This is true both for the coursework and for research. It makes the whole process a lot less stressful and a lot more fun.

When I was in my first year, it was clear from day one that the courses we had to take were going to be a lot more demanding than anything in the undergraduate program. But we had a great group of fellow students. I hardly knew anyone in the first week, but by the end of that week e-mail addresses and cell phone numbers had been exchanged. We often got together to work on problem sets and to drink beers (although never both at the same time). It was still an immensely challenging year, but with these brothers and sisters in arms, it was much easier to put in the extra effort.[1]

Related to this, there's an interesting psychological phenomenon known as "impostor syndrome". This is when despite your achievements you have the feeling you are actually a fraud and that all your accomplishments were just good luck and that any day now, people will find out that you're not really that smart. You would be surprised at how many PhD students experience this. In my first year, I constantly felt like I was surrounded by all these smart people and that I would fail the exams and get fired. Even after the first semester, in which I had excellent grades, I was semi-convinced that this was due to luck and that my grades would revert to their true value in the second semester. And then, after the second semester had also gone well, I told myself that taking courses was quite easy and nothing compared to doing research, at which I was clearly not good, and so on.[2]

A first step to overcoming "impostor syndrome" is just to realize that it's a well-known psychological phenomenon, a bug of the mind, like optical illusions. A second step is to tell yourself that, even if perhaps your previous accomplishments were due to luck, it is possible to grow and improve yourself to match the expectations. That's what I want to talk about next.

Your Mindset: Fixed or Growth?

There are, roughly speaking, two big "mindsets" people can have when they are trying to learn a new skill. These mindsets are usually subconscious, but they influence your motivation and how you think about failure. Importantly, they are not set in stone: you can move from one to the other, if you start paying attention to it.

The *fixed* mindset is the belief that you were born with a fixed set of skills and competencies, and that some activities or skills are just not "your thing". It corresponds to the belief that success is determined by natural talents such as your IQ. With this basic outlook on life, if you try something and fail, you will interpret it as a signal that this must not be one of the activities

[1] Since the group of students is usually quite international, there are other benefits too: I recently went to one of my classmates' wedding in Austria.
[2] And even as I'm writing this essay, part of my brain is telling me that I'm not a *real* economist and that some people in much tougher subfields of economics or much tougher universities would laugh at my pretensions, and so on, and so on.

you're naturally good at. Failure in an activity is a signal that "this is not for me". Because of this interpretation, you're not very likely to continue practicing. Why bother trying again, if you were not made for this?

The *growth* mindset, by contrast, starts from the assumption that being good at something comes down to a little bit of skill and a lot of hard work. Sure, knowing how to play an instrument depends on a bit of musical talent. But given a minimum of musical talent, you just need to practice like crazy before you can play the piano or the clarinet reasonably. With this mindset, failure is not a signal that you should stop whatever you were doing. On the contrary, it is a signal that you need more practice. With a growth mindset, you would analyse your failure in more detail to figure out what went wrong and why, so you know what to focus on in the future.

Many people, including myself, have the fixed mindset as a "default" setting. If you already have a growth mindset, all the better. If not, there are three points I want to make.

First, know that the mindset can be changed. This is perhaps the most difficult step: if you believe that success in life comes down to innate skills or talents, it seems likely you will also believe that your mindset is innate and given! But trust me on this: it is not. To take just one (possibly overdramatic) example to illustrate this point, people suffering from depression seem to have a view of the world that is similar to the fixed mindset, and which leads to a "negative explanatory style": depressed people tend to think the causes of failure are stable (fixed), global (it affects everything they do) and internal (they are the root of the problem). Therapy often focuses on tackling this explanatory style, making patients aware of these thought processes and helping them to consciously change them. Explanatory style can be changed.[3] If people with severe depression can overcome their negative explanatory style, then surely you can overcome your fixed mindset!

Second, there are good reasons to believe that in reality your innate talents matter less than your effort, so that the growth mindset is actually the correct way to think about things. Talent is never enough. The media like to portray successful people as people who just happen to have enormous innate talents. In movies, a smart person is a "genius" who on the spot has all the brilliant insights necessary to solve a problem. Movies never show the smart person as someone who spent years and years carefully studying a topic and gradually realizing how the puzzle fits together. But it's this second version which is more accurate. People who are successful in a field have usually practiced or studied or prepared *incredibly* hard to get where they are. It appears that experts in any kind of field often spent around 10,000 hours perfecting their skill.[4]

Third, switching to a growth mindset will make you happier anyway. With a fixed mindset, failure is a hard, damning, eternal judgement on your talents. You try to play the piano and fail?

[3] For more on this and other psychological phenomena, an interesting book is David G. Myers (2000) *Exploring Social Psychology*, see in particular p. 299. I'll be referring to a number of books and articles in this essay; you can find a bibliography in the back.
[4] For more on this, see Robert Greene (2012) *Mastery*, Profile Books.

Clearly, you have no musical talent whatsoever and you'll never be able to do it, in the fixed mindset view. Now, contrast this with the growth mindset. You fail on the piano? You could master it if you put in enough practice. There's nothing wrong with you, just work a bit more. Or don't, if you don't really like the piano. It's your choice.

Now, while it's possible to override your fixed mindset, it's highly likely that it will remain your "default" setting, and that these thoughts will resurface if you're not actively fighting them. I still have to fight it. For instance, it was quite a psychological barrier back when I was learning how to drive a car (which is embarrassingly recent). There was a constant struggle between the "default" of my mind telling me I'd never be good at it, and the conscious realization that if so many idiots can drive a car, surely I should be able to learn it too, if I only practiced enough. And it was true!

Even if you accept that it would take hard work to get where you want to be, you will not always have sufficient energy or motivation to actually put in the work. You may say: "Okay, I know I should study, but I simply can't find the energy; I'm simply lazy." First, you are not *simply* lazy – that would again be a form of fixed mindset thinking. Everyone feels unmotivated now and then, and there are a number of factors which typically drain motivation (e.g. a lack of deadlines; a lack of clarity about what exactly needs to be done). Moreover, don't apply ridiculous standards to yourself. Even the most disciplined people I know complain that they are terribly undisciplined; it's normal that your energy levels are not always 100%.[5]

How does all of this relate to your PhD? With a fixed mindset, you're in for a great deal of stress. If you try to figure out some statistical technique or mathematical problem but you fail, your mind will interpret this as yet another sign you're not really smart enough to be doing a PhD. You'll feel like you're not supposed to be in the PhD program (the "impostor syndrome" mentioned earlier). Since a PhD is challenging, it's almost guaranteed that you will encounter failure, so you will have lots to stress about, and perhaps you'll even convince yourself that you shouldn't be in the PhD program at all.

With a growth mindset, you realize failure for what it is: a signal that you need more practice. If you don't understand this or that technique or problem, you should realize that many other people in the field who are now experts also had difficulties understanding the issues in the beginning, and that it's just a matter of finding the right way of approaching the issue. This takes away a lot of stress. Instead of everything being a judgement on your self-worth, it's now just an indicator of what you should work on a bit more. Instead of thinking "I can't figure out how X

[5] Of course, if you have serious and persistent motivation problems, perhaps you have chosen the wrong field after all. There's nothing wrong with that; the only question would then be how you want to deal with the situation. You could continue the PhD regardless, and find another job afterwards; or you could stop before finishing the PhD. This is not a failure in any way. It's impossible to know for sure if you will enjoy a certain job or not, and this is as true for academic jobs as it is for other jobs. Again, use the growth mindset here: you've learned more about yourself, your skills and preferences, and this knowledge will come in handy as you build your post-academic life. However, don't give up too fast. Like I said earlier, almost everyone I know has thought of quitting at some point.

works, I must be too stupid for this job," you would now say "I can't figure out how X works – what would be a good strategy to crack this problem?" This is a lot more productive.

Dealing with Rejection

A growth mindset will also help you survive setbacks and criticism. And you will have your fair share of those. When I was in my first year, I applied for a scholarship from FWO (the Flemish science foundation). The rejection letter said, among other things, that I had been "good, but not excellent" as a student. The memory still makes me angry. I've always thought of myself as having a comparative advantage in intellectual matters, and quite a bit of my self-image depends on the feeling that I'm good at those things. Receiving an official letter from a government agency dissing my track record was not much fun, especially because I was still in the "impostor syndrome" phase I talked about earlier. So, I shook my fist at the skies while yelling "FWO!". Better luck next year, I thought.

Alas, no. The next year I spent a lot of time writing what I still think was a pretty cool research proposal (about using games to measure social preferences among fishermen on a lake in Benin), only to receive a letter saying that although my research proposal was very important and very creative too, unfortunately it should have been more detailed on the methodology part. (There was a limit of three pages to write the entire proposal – introduction, literature survey, research question, methodology, how the proposal fits in the research of your group, and references). Since you can only apply for FWO twice, that was game over. Fortunately, I could still apply for travel grants!

Except those got rejected, too – in one case because, as the official explanation put it, "we ranked all the requests and yours did not make it," which was very helpful feedback indeed.

Nowadays, I can joke about these rejections, although I still have revenge fantasies about that "good, but not excellent" letter. Back when I got my first rejection letter, it must have been the first time I truly failed something academic, and I was shocked. The second rejection felt equally bad, because many knowledgeable people said I had a good chance of getting the funding. When I got the news, I was at a conference with my thesis advisor, and he saw that my motivation was pretty low after this second rejection. Over a beer, he told me that the correct response in this case was to tell myself that I would prove them wrong. I agree that's the right way to think about it.

One thing which helped me deal with rejections was realizing that rejection is standard, and that many good economists have had their work criticized and rejected.[6] For instance, George Akerlof's classic "The Market for Lemons" got rejected at three journals before finally being

[6] If you think this would also have some therapeutic benefit for you, the go-to reference is Gans and Shepherd (1994), "How Are The Mighty Fallen: Rejected Classic Articles by Leading Economists," in the Journal of Economic Perspectives. More generally, tons of classic novels have been rejected by publishers, and if you Google around a bit, you'll find great examples. Apparently George Orwell's Animal Farm once got rejected because "it is impossible to sell animal stories in the USA."

accepted at the Quarterly Journal of Economics! Even top economists are routinely rejected, so you shouldn't feel too bad about it.

A friend and colleague wrote some interesting thoughts on this topic, and I don't think I can say it better than him:

> [A]s PhD students we are constantly evaluated (during presentation, conferences, academic events and sometimes - we believe - even during cheap chats with the professors) and when someone evaluates us "badly" we might fall into sadness and depression and we can start feeling insecure about everything we have done or we are currently doing… but the secret is to learn (and accept) that that's part of our job: there will always be someone that will be ready to criticize our research, as that's exactly the point of doing research. If we could only look for answers everybody would easily agree upon, what would be the point of our research? The important thing is to look for the constructive part in every criticism: maybe the person criticizing us was very right and we should abandon the idea or approach we had...or maybe he/she was wrong and we just need to insist on our idea, looking for a way of better framing or better explain it...

I love that last sentence, because this is exactly the growth mindset in action. Either the criticism was correct and we need to improve our approach, or the criticism was wrong and we need to think of a more convincing way to explain what we were doing. The growth mindset tells us that we can improve; it's just a matter of finding the right tricks.

A bag of tricks

You might think that as a PhD student it is expected of you to simply *know* how to study, how to do research, and so on. At least, nobody will tell you how to do it. And so we spend our time teaching students about obscure statistical techniques but not about the more frequent obstacles they will face, like trying to get your goddamn data in the correct format so that your statistical software can read it properly. What I discovered is that studying and researching and all other things related to a PhD – and probably to any career – depend on little tricks and techniques, and "experts" in a field are often people who also happen to know a lot of these little tricks.

Since I don't know all the tricks myself, and since the tricks you need might depend on your personal characteristics, the following is absolutely not exhaustive. So, when you get stuck on something, try to find out which trick would help you solve the issue. A good trick to find tricks would be to ask people who are very good at something – again, this usually means not that they have some magical innate skill but rather that they figured out the essential tricks through practice and experience. Talk to them and ask them. (It's a kind of meta-trick!)

Most of the tricks you need are psychological. Yes, we're supposed to be super-smart scientists here, but in the end we are just normal people, with all the normal psychological weaknesses (cognitive biases, procrastination, absent-mindedness, lack of focus, and so on). Fortunately, in recent decades there's been an explosion of work on behavioural economics and on psychological tricks to overcome these weaknesses. If you have time, I'd definitely recommend that you read a bit of behavioural economics. It's fascinating and useful, what more could you want? A good starting point is "Nudge" by Thaler and Sunstein. If that makes you hungry for more, you can check "Thinking, Fast and Slow" by Kahneman.

I almost didn't notice, but I just applied a trick here: if you want to know more about a subject, try to read a popular science book or article instead of diving straight into the academic literature. When I took a course on networks, I bought a couple of popular science books on social networks and on the science behind networks such as the internet.[7] Those books were all written by well-known researchers, and it was a great way to get familiar with the real issues and with the big questions in the field. Afterwards, studying the course material was a lot easier, because at least I knew why the author spent so much time on this or that topic. Moreover, it helped my grades. In the format of the course, every student had to teach one chapter of the book to the others and we were graded on the quality of the presentation. All the chapters were quite mathematical. But thanks to the popular science books, I could give fun examples and show unexpected similarities between, say, a hacker trying to destroy the internet and a government trying to stop a pandemic by shutting down airports. By reading the popular science books, I had fun and got better grades too.

"But Koen," you might be saying, "I don't have time to read those popular science books!" Ah, but if you can find a good popular science book about a topic you are working on, it will actually save you a lot of time down the road. You'd be more knowledgeable, it would be easier to understand arguments in the literature, and it would be easier to see to which other studies your own work can be linked. Of course, not every field or topic has good popular science books but you would be surprised at how many good popular science books are out there, even about topics such as statistics or game theory.[8] Even if you can't find a good popular science book, you might be able to find a textbook that is better than what your professor assigned. Yet another benefit of the "popular science" strategy is that you could read it in your spare time while feeling super-productive!

It's not necessary to start reading behavioural economics papers to get the necessary tricks. There are hundreds of fascinating blogs on the internet, as well as a ton of books, offering tricks for organizing your time, getting things done, learning more, reading faster, or whatever you

[7] I've listed these in the bibliography.
[8] Again, see the bibliography for some examples. In general, if you do a bit of Googling, you can easily find reading recommendations on blogs of economists or other specialists. Amazon also helpfully suggests similar books if you're looking at some title. Another good source is the "Books and Arts" section in The Economist, which has interesting book reviews.

would like to accomplish. I think the first step here is to realize that if you are struggling with something, probably somewhere among the other seven billion people on this planet there are some people with a similar problem and you might be able to learn from their experiences. Some resources I found particularly helpful:

- Josh Kaufman's website and book "The Personal MBA" as well as his list of 99 recommended business books. Check it out even if you are not interested in running a company – the book and the reading list cover topics such as 'The Human Mind', 'Working With Yourself' and 'Working With Others', all of which are useful for academics.
- David Allen's book "Getting Things Done," a classic on how to avoid being overwhelmed by to-do lists. I probably only apply about 10% of his advice but it has already made a huge difference.
- Cal Newport's blog "Study Hacks". Newport is an academic researcher himself, and he emphasizes the importance of deep focus and avoiding distractions. This conflicts with David Allen's focus on executing to-do lists (which Newport once referred to as "getting unremarkable things done"), but both have important and useful things to say.

Below I present some more tricks. Needless to say, this is not the bible; experiment with the tricks and see what works for you, and exchange tricks with others.

Tricks for Studying

Connections. I think the best strategy when you're studying anything is to focus on understanding first, and memorization later (and only if necessary). What does "understanding" mean? I have a theory that nobody really understands anything – we just recognize that some things work like other things we are already familiar with.[9] Sometimes "understanding" means finding the exact (mathematical) connection between something we don't understand and something we do understand. Other times, it's more a matter of metaphors or similarities.

The way I see it, "knowledge" or "understanding" is like a giant web of ideas in your head. If someone tells you about China's increasing influence in Africa, then you probably already knew that China's economy is growing strongly, which, come to think of it, probably means they need a lot of resources, and yes, you recall that Africa has a lot of resources, so this story makes sense. You may also know, incidentally, that having resources is not always good for a country because of the "resource curse", because just think of the Democratic Republic of the Congo with all its minerals but widespread poverty. The idea of the increasing Chinese influence in Africa immediately leads to the idea of China, and the idea of strong economic growth, and the idea of a rising demand for resources, and to the idea of Africa, and the knowledge that Africa is a

[9] Everyone understands the Pythagorean theorem, until you ask them to prove it. It's not that hard, really, but obviously we don't have the proof in our head when we use the theorem in a calculation. Yet, if you have a mathematical problem but suddenly you see how it can be translated into rectangular triangles, a little light bulb will flash and you will "understand" the problem as being a variation on the Pythagorean theorem.

resource-rich place, and to the idea that resource-rich places are often quite poor, and the example of the DRC, and possibly a host of other ideas about the DRC as well (King Leopold II and all that).

So, the statement about China's increasing influence in Africa makes your mind visit all these connections. And now, next time you think about Africa, or the resource curse, or China, there is a good chance your mind will visit the knowledge about China's increasing influence in Africa. In addition, all these different ideas are now strengthened, because you have used them recently, and you have made connections between all of them. If you find yourself having a conversation about Africa or China or resources, you'll be able to use this knowledge.

What basically happened is that you now connected the ideas of "Africa" and "China" with a link. In the future, if you learn some new piece of information about the influence of China in Africa, you will have a "hook" to put it on. Suppose someone tells you they saw a lot of Chinese migrants during their recent holidays in Africa. You'll be able to link this to the other "China and Africa" link you have, and you can form a hypothesis: perhaps this is part of the same phenomenon. Next time someone talks about China in Africa, you'll be on the lookout for information about migrants, because your mind already has a hook for that. And so you weave a web of knowledge about this issue.

The strategy of weaving a web is universally useful, whether you're studying history or mathematics or industrial organization. Take history, for instance. Recently someone told me about Indian migrants moving to Africa in the late 19th-early 20th century. I was a bit surprised to learn about this, but then I remembered that Gandhi had lived in South Africa for several years. The idea of an Indian community in South Africa now makes more sense. Next time I hear about Gandhi or about Indian migrants in South Africa, I'll remember the other fact. Both are linked, and reinforce each other.[10]

The implication for learning is this: don't try to memorize things immediately; instead, try to first create connections in your head. Here are some ways of doing that.

Exercises. If you're learning mathematics, the best way to learn a new technique is to make exercises and problem sets, instead of looking at the formula in the book over and over again. Making exercises may seem like a pain, and it is! But it's still the best way. If you've solved a bunch of mathematical problems (say, dynamic optimization problems), at some point your mind gets used to seeing this type of problem and develops an intuitive "feel". Unconsciously, all the exercises have left a trace in your head. Your brain has become used to this type of problem, and the next time it encounters a problem like this, it will more or less know in which direction to look for an answer.

[10] Scott Young has called this "holistic learning". See more here: http://www.scotthyoung.com/blog/2007/03/25/how-to-ace-your-finals-without-studying/

Likewise, in macroeconomics it's important to know the IS-LM-diagram – the graphical representation of Keynesian economics which has interest rates on the vertical axis and GDP on the horizontal axis, with a downward-sloping IS-line representing equilibrium in the goods market and the upward-sloping LM-line representing equilibrium in the money market.

I just wrote all of that off the top of my head because I had to teach it last semester, and because of a few tricks for memorizing what everything does. In the IS-LM-diagram, for instance, an increase in government spending means an increase in interest rates and GDP, so it must be the downward-sloping line moving to the right. Monetary expansion means lower interest rates and GDP, so that must be the upward-sloping line moving to the right. Since "LM" stands for "liquidity and money", and "IS" stands for "investments and savings", it's easy to remember the downward-sloping line is the IS-curve and the other one is LM.

This is of course putting everything on its head. Normally you should first study the entire thing, remember which line is which, and then carefully derive that if you increase the money supply, the LM-curve shifts to the right, leading to higher GDP and lower interest rates. But this is not how I remember. As an undergrad student I made plenty of exercises on the effect of monetary or fiscal policy in the IS-LM-framework and so I more or less remember how the curves move, and I can reason backward to figure out what the exact definitions were.

There's an extension of the IS-LM-framework known as IS-LM-BP which takes into account international trade and exchange rates (BP stands for "balance of payments"). But I never made that many exercises on it, and so I don't really "see" intuitively how the BP-curve looks like, when it shifts, and how everything works together. If I wanted to know, I would make a bunch of exercises until I more or less remembered how different policy measures work out in that framework.

Likewise, I have absolutely no clue about linear algebra. I studied it in my undergrad years, I used some of it at some point (in econometrics), but it's not fixed in my mind. Every time I read something about eigenvalues, I again think "Damn, I should know this stuff by now." But I simply don't have enough connections (or, to put it differently, I never studied this stuff the right way; I never made enough exercises).

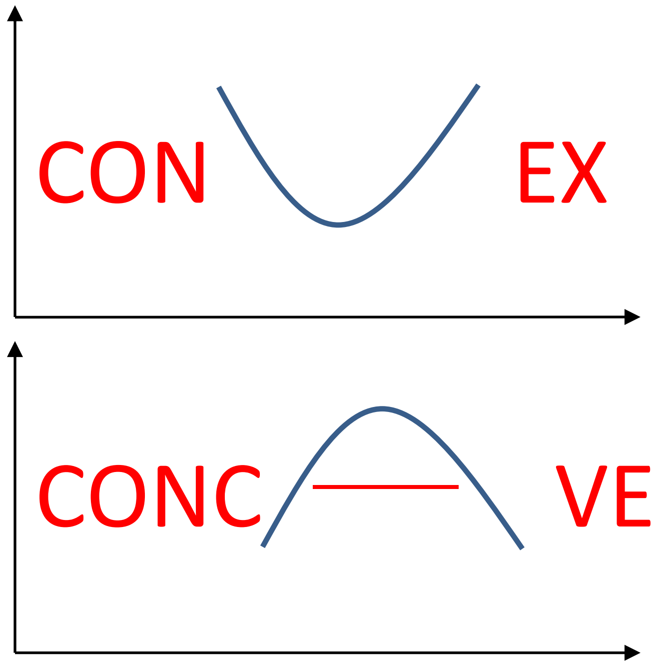
Find a story. The connections don't necessarily need to be "logical" or "correct" or even in the same chronological order in which you learned the material. For instance, I remember or understand how statistical significance, t-statistics, p-values and standard errors work because I read Ziliak & McCloskey's critique of this methodology. When I was trying to understand their criticism, I went back and forth between their book and my statistics notes. Chronologically, you would expect people to first really understand how statistical significance works and *then* understand the critique. In reality, I learned about both by reading the critique, and they reinforce each other: I understand the critique because I understand statistical significance, and I understand statistical significance because I understand the critique.

In my mind, the whole thing has become a story. Humans love stories (ever noticed how popular soaps are?), and this is also true of PhD students. It's much more fun to read your statistics textbook thinking "oh, Ziliak and McCloskey would *hate* this!" Because there is a debate on statistical methods, the dry and boring text suddenly becomes a battlefield of ideas and arguments. "They say A; the others would counter with argument B. Now, I read about a different issue C; how would the other side react to this?"

You can do this for many subfields in economics, for instance in macroeconomics, where there is a lively debate between neoclassicals, new Keynesians and what have you. It's also the case in development economics, where you have people believing in foreign aid (Jeffrey Sachs), people who are very sceptical (William Easterly), and people who emphasize we need to use good evaluations to see what works and what does not (Duflo & Banerjee). Some economists have emphasized institutions (Acemoglu), but others say that this is not the full story (Duflo & Banerjee again). And so on! It becomes a lot more fun to study methodological issues, historical facts, theoretical concepts etc. if you see how it forms part of a broader story.

This is part of the reason why I recommend reading popular science books – the authors are writing for a broad audience and so they are usually minimizing the technical aspects to bring out the story first and foremost. Once you get the broader story, you can delve into the technical aspects and you'll know why it matters.

Crazy connections. Don't be afraid to come up with crazy or weird connections to remember or understand things. For instance, how does a convex function look like? It looks a bit like a "V", for conVex. And a concave function looks a bit like an "A", as in concAve. Stupid? Yes. Efficient? Definitely.



Also, what is the second derivative of such a function? Well, that's easy: the second derivative of a convex function is positive because the convex function looks a bit like a happy smiley face, which is positive; and the concave function looks like a sad smiley, so it's negative.

Of course, that's nonsense, and you're smart enough to look at the curve and figure out how the derivative goes – but little tricks like this can sometimes save you time, they can help you remember, and they make everything a lot more fun. I can never see a convex function without seeing a smiley face.

Now, you may be wondering: is this really how Koen thinks about concave functions? Yes, it is. Don't be too proud to admit you're using a ridiculous trick to remember something. This again relates to the "impostor syndrome" I was talking about earlier. You may feel like you are supposed to be a math wizard who just *sees* the world in terms of vectors, upper-hemi-continuity and Kakutami's fixed point theorem. You may feel like you are supposed to remember the entire IS-LM-thing from first principles. So, you may be a bit reluctant to admit you're using silly tricks to remember stuff. Don't fall into this trap. If a trick works, then use it.

I want to make two important caveats here. First, the silly tricks are not *substitutes* but *complements* of the more serious work. If you ask me why the convex function corresponds to a positive second derivative, I can show you – but it takes slightly more time to work out the math than it does to see a smiley face. However, you need to know the correct reasoning. At some point, you'll be facing an exercise or an exam question or even a research problem where convexity and concavity play an important role and you need a little bit more insight than drawing smiley faces. But the silly trick can help you remember what you're looking for exactly, and can help you to memorize or understand the actual proof.

The second caveat is that if you are using a silly trick, make sure you're not using a *confusing* silly trick. An American friend of mine once travelled through the Netherlands and completely messed up her schedule because she avoided the *sneltrein* (fast train) – she had been learning a few Dutch words by associating them with English words and she thought she knew the sneltrein was the *slow* train because she associated it with a snail. The same can happen if you are using ambiguous silly tricks. Keep this in mind if you're trying to find silly tricks.

Explain it to somebody. This is a classic trick, but it works really well. If you want to understand something for yourself, try to explain it to yourself or – even better – to somebody else in the simplest possible terms. It's remarkably easy to fool yourself into thinking you know something. But if you're explaining it to somebody who has no background on the topic, you are forced to avoid complicated words and concepts, and so you are forced to go back to basics, to explain everything in tiny baby steps.

Use extreme examples. This one may seem related to the "silly tricks" but there's a difference, because it's not really silly, just strange. Suppose you are explaining to a friend who is not an economist why monetary expansion can lead to inflation. Are you going to throw $MV = PQ$ at him, or explain about expectations-augmented Phillips curves? Hopefully not. What you can do, is tell a story about what would happen if tomorrow we printed billions and billions of euros, then took our helicopter and threw the bills down. Suddenly, everyone feels richer and rushes to the stores to buy stuff. But the amount of stuff has not increased. So, we have more and more euros chasing the same amount of goods, and prices would increase. You can use the example of Zimbabwe or Hungary here.[11] Of course, in reality such hyperinflations are extremely rare, but it helps to make clear the link between the money supply and the price level.

Extreme examples should not always be realistic. You can imagine a demand curve which is perfectly inelastic or a demand curve which is perfectly elastic. If you're thinking about intertemporal maximization, you can imagine a person who is infinitely patient or a person who is extremely impatient. You can imagine an agent who is infinitely rational or an agent who is extremely naïve. Using these extreme cases can help you to understand how an economic model or an economic phenomenon really works.

Find allies. One of the magic tricks which pulled me through my year of doctoral training was the fact that I could rely on a wonderful group of people who were in the trenches with me. As I explained above, I knew nobody in the program, but from day one I had the good fortune of meeting wonderful people with whom I spent hours studying in the library, discussing problem sets, and e-mailing about concepts we did not quite understand, or bits of Stata code we could not figure out. You may not have as much luck as I had in finding such exceptionally collaborative people, but you should try to get allies whenever you can. There's little value in

[11] In 1946, after astronomical hyperinflation, Hungary decided to replace its old currency (the pengö) with a new one (the forint), at a rate of 1 new forint = 400 000 000 000 000 000 000 000 000 000 pengö. On the internet you can find wonderful pictures of people sweeping worthless banknotes in the streets.

staring at a problem for a day because you misunderstand a concept which a friend could have explained to you in ten minutes.

I'm not saying you should always sit together to study. Sometimes it's better to isolate yourself from the world and work without distraction. But having a support group which can help you is an amazing asset to have.

Tricks for Research

I don't think I'm a good researcher at all, but I do believe I've improved a lot compared to five years ago (thank God). As I said earlier, research is incredibly hard. If you're making a difficult exam, at least the question is more or less clear and, since the question is being asked on the exam, there is probably an answer. In research, often the question is not really clear, and it's absolutely not clear if there is an answer at all. Moreover, the question could turn out to be the wrong question completely. You could fail to find an answer, or you could come up with an answer to a different question, and you might not even realize it. Suffice it to say, research can be a bit complicated. Here are some ideas which will hopefully be useful to you.

You are a detective, not a statistics robot. After finishing my exams in the doctoral program, my first assignment was to write a book chapter about beer consumption in Russia. Basically, my advisor told me beer consumption had increased dramatically in Russia over the past fifteen years and he told me to figure out what was going on.

Now, since I had just finished the doctoral program, my math and econometrics skills were better than ever. But none of it prepared me for that paper. My advisor had told me to look up beer consumption on the FAO website, which was easy enough to do. But how do you go about finding "the cause" of the rapid increase in beer consumption? What data are you looking for? Where can you find it? I got stuck pretty quickly.

The problem is that most of the doctoral program (at our university and elsewhere) seems built on the assumption that research is almost a routine job: pick hypothesis, build model, collect data, test model; repeat. We spend most of the time on building the model or testing the model, but almost zero time on picking the hypothesis or collecting the data.

When you're picking a hypothesis, which one should you pick? Out of the bazillion possible hypotheses of what caused beer consumption in Russia, which one is most plausible or most interesting to look into? It's a little known fact of science that there is an infinite number of hypotheses out there, and you could spend a lifetime checking all of them if you were blindly following the "routine" model (and almost none of them would give you any interesting results, let alone a publishable paper). I needed a way to help me select a decent hypothesis. With a good hypothesis in mind, it becomes easier to identify what kind of data, what kind of sources and what kind of tests I could use to study the question.

Collecting the data is another step which is usually glossed over. Surprisingly, not all the information in the world is already available in an Excel file for you to import into Stata. Even more surprisingly, a lot of data is not quantitative, or even if it is, it would not be quantitative enough to do a regression on it. I had to become a lot more flexible in thinking about "data".

In the Russian case, I realized I needed to stop thinking like an econometrics robot and start thinking like a detective, finding information wherever I could. Moreover, I realized that not all the important information comes in the form of a clear, "knock-down" argument. For instance, I wrote in the chapter that beer in the Soviet era was apparently pretty awful. I based this assessment on newspaper reports and on the fact that when foreign investors entered the Russian market they spend millions to upgrade the breweries to produce higher quality beer. It's a judgement call, because you could of course argue that I don't have enough data to make this statement; if you were truly hard-core, you could insist I should find objective sources on beer quality around the world during the Soviet era (good luck with that). However, people familiar with the situation confirmed that quality was poor, and available information points in the same direction, so we can be more or less confident that Soviet beer was not exactly a pleasure for the taste buds. This is relevant information if you're trying to find out why beer consumption changed, but it's not the kind of information I was originally trained to look for.

Historians need to make such judgment calls all the time, and a large part of historical debates centre around the question of whether someone's judgment calls can be justified or not, and whether they are consistent with other pieces of evidence. If you want to know whether there was a change in the social standing of merchants in England in the centuries leading up to the Industrial Revolution, you cannot really go and consult Gallup poll data. Instead, you need to rely on indirect evidence – the way merchants are portrayed in art and literature for instance. But even the more quantitative approaches to economic history are full of judgment calls. Thanks to the pioneering work of people like Angus Maddison, we have historical statistics on GDP and population for the past two-thousand years.[12] Of course, there were no statistical offices in 1500 calculating GDP statistics, so economic historians need to use all kinds of indirect techniques. They use estimates on populations of cities, tax data on the number of breweries, records of shipments of wheat, and so on, to come up with more or less plausible estimates of what GDP per capita must have been in a certain historical period. For some regions this works better than for others (Spanish GDP estimates for the 16th century vary wildly, for instance), but it's better than nothing.

The bottom line is that "data" is not necessarily the hard numbers you've been assuming throughout your education, numbers which seem to fall from the sky as objective representations of the truth. Instead, think like a detective, or like a historian. Perhaps you don't have data on the thing you're studying, but you have indirect information which can be used as a proxy. (For

[12] Which is seriously cool! See http://www.ggdc.net/maddison/maddison-project/home.htm. If you want to see how economic historians think about the numbers, it's interesting to read Bolt and van Zanden (2013), who discuss how the original Maddison estimates are being updated using new research.

instance, we don't always have good income data for developing countries, but we can use ownership of assets such as cars, houses, bikes or TVs to construct a "wealth" proxy).

Keep it simple. Again, much of our education focuses on fancy techniques (Generalized Method of Moments! Infinite-horizon dynamic stochastic programming!), but it's more important to first get the story straight. And the story can usually be told without integral signs.

Suppose you are looking at beer consumption in Russia, again, and your hypothesis is that beer prices changed, leading to the increase. That would be a plausible hypothesis. But, being a good economist, you know that prices are not really exogenous. So, you start thinking of ingenious instruments to study the effect of beer prices on beer consumption.

But hold on a second – your data on beer prices does not really show any big changes, while beer consumption quintupled. So, first of all: since the change in beer consumption is really large, we would need a very big change in beer prices or a very price-sensitive demand for beer for this story to work. And second, your beer prices don't seem to have changed all that much. Perhaps they contributed a little bit, but do we really believe that a small decline in beer prices led to a quintupling of beer consumption over the span of ten years or so? That doesn't seem plausible at all. So, forget about your search for an instrumental variable for beer prices: it's probably not worth the trouble.

You can see the problem here. If you start off with the technique, you can easily spend months perfecting and implementing the technique only to discover that there's nothing going on. And quite often, you could have known up-front that you wouldn't find anything.

I actually fell into this trap with the Russia paper. At some point I learned that there is a large panel dataset on Russian individuals, which also has data on beer consumption (a dummy variable indicating whether the individual drank beer in the past thirty days or not). I was thinking about all the complicated issues I had here – a binary outcome variable, a panel, missing values, and on top of all that I was thinking about estimating peer effects. At some point I convinced myself that what I really needed was a complicated technique using Simulated Maximum Likelihood which, unfortunately, was not yet coded as a Stata command. I spent an entire month coding the algorithm before I realized that I should probably check whether it was necessary to do all this. The answer was no.

It turned out that simple linear techniques (OLS, fixed effects, and so on) actually already give a good idea of what's going on, even if the dependent variable is binary and even with missing values. Instead of spending an entire month programming a fancy technique, I could have spent two hours running some simple regressions and I would have seen that the issue I was worried about was probably not that important.

If you are getting started on an empirical paper, and your hypothesis is that A causes B, try a simple scatterplot first. Or make a graph in Excel to see whether A and B move together over time. Then, do a simple regression with a few extra variables to see if there's anything going on.

This could save you months of hard work and frustration. If your hypothesis is that A causes B, but you can't find anything in a scatterplot or a regression, then perhaps there's nothing going on and you can save yourself the trouble of using more advanced techniques to study the question in more detail.

Moreover, some of the most impressive papers in economics use little more than OLS or instrumental variables to make their point. Fancy techniques are often not necessary to tell a compelling story.[13]

Discover the wisdom of György Polya. The Hungarian-born mathematician György Polya (1887-1985) is considered a pioneer in the field of "heuristics". A heuristic is a tool or a trick to find a solution to a problem. Polya wrote a little book called "How To Solve It" (1945) with advice on how to tackle mathematical problems – but many of the tricks can be used for other problems as well. On one of the first pages of the book, Polya put an overview of the approach, which I've copied and put up in my office and to which I turn in times of trouble. And I've added it as an appendix to this essay too, so again you have no excuse.

Polya offers a strategy in four steps:

(1) Understand the problem
(2) Devise a plan
(3) Carry out the plan
(4) Look back on your work.

Those four steps are of course a bit trivial, but for each of the four steps he offers some extra advice.

1. *Understand the problem*. You would be surprised at how often I have sat staring at exam questions or exercises, or theoretical problems for a paper, before I suddenly realized that I did not really know what I was looking for. So, the first question is always to clarify what, exactly, you're expected to do. Polya lists some questions to help you get clarity on this. For instance, do you know what all the words or concepts in the problem statement mean? If not, go back to definitions. Try to draw a picture. Try to restate the problem in different words.
2. *Devise a plan*. Here, Polya offers a long list of ideas for how to tackle a problem. If you're stuck on a specific problem, try generalizing the problem. If you're stuck on a general problem, try solving a more specific version or a simpler version of a problem. If

[13] See, for instance, the papers by Nathan Nunn on the long-run effects of the slave trade on African development (listed in the bibliography).

there are only a few possibilities, try all of them, one by one. If you have an idea of what the solution should look like, try to write that down and work your way backwards.

3. *Carry out the plan*. Try one strategy at a time. Take the plan you came up with in step 2 and try it out.
4. *Look back on your work*. Either your approach solved the problem, or it didn't. In both cases, go back over your calculations in step 3 to see what worked (or what went wrong). If you solved the problem, perhaps the strategy you used could come in handy in the future and it's good to check again how you did it. If you didn't solve the problem, perhaps you can find the exact point where something goes wrong – some number turns out to be a constant instead of a variable, or the other way around – and this may give you inspiration for a new plan.

You can find more information here: http://en.wikipedia.org/wiki/How_to_Solve_It

My favourite suggestions of Polya are to make a picture, to look at a special case, and to go back to definitions.

Make a picture. For one of my papers, I got stuck until I decided to turn to the eternal wisdom of Polya and make a drawing. It actually took me an entire evening to figure out how to draw the situation I was thinking about. But once I had found a way to draw it, the entire story fell into place. I could suddenly see how different cases in my paper looked like graphically, and I could add a few extra situations which were obvious once I made a picture but which I hadn't thought about while staring at the equations.

Look at a special case. This advice is a bit similar to the suggestion to "use extreme examples" when you're studying. In my case, I was writing a paper where all kinds of things were happening at the same time – moral hazard, risk aversion, double marginalization. I kept getting stuck in a mathematical jungle until I used Polya's trick: look at a special case. By solving a case without risk aversion, and then a case without moral hazard, and then a case without double marginalization, I could see patterns emerge. If I then went back to the general problem I could see how the different pieces fit together. Every time you have a model with different effects working together, try to "turn off" one of them and see what the solution becomes and why. Do this for all the different pieces and it will be much easier to see the solution to the general case.

Now, that is specific advice for theoretical work, but you can see the same tricks emerging over and over again in this essay: visualize (by drawing a picture; by making a scatterplot) and simplify (by looking at special cases; by doing simple OLS first before you turn to more complicated techniques).

Go back to definitions. If you have been staring at a problem for some time, maybe the problem is that you don't really understand what you are looking at. For instance, if you are studying cooperative game theory and you don't know how to demonstrate that some solution concept satisfies "individual monotonicity," maybe you should start by writing down the definition of that solution concept, and of that axiom. This is the point where you need to move beyond the silly tricks and the intuitive 'feel' and get down to the technical details – quite often, just writing down the definition will give you a hint of where to look for the solution.

In Douglas Adams' *Hitchhikers' Guide to the Galaxy* an enormous supercomputer is built to compute the answer to the great question of Life, the Universe, and Everything. After seven million years of calculating, the computer comes up with the answer: 42. The people who receive this answer are obviously a bit confused, and ask the computer whether this answer can be correct:

> "I checked it very thoroughly," said the computer, "and that quite definitely is the answer. I think the problem, to be quite honest with you, is that you've never actually known what the question is."
> "But it was the Great Question! The Ultimate Question of Life, the Universe and Everything," howled Loonquawl.
> "Yes," said Deep Thought with the air of one who suffers fools gladly, "but what actually *is* it?"
> A slow stupefied silence crept over the men as they stared at the computer and then at each other.
> "Well, you know, it's just Everything ... Everything ..." offered Phouchg weakly.
> "Exactly!" said Deep Thought. "So once you know what the question actually is, you'll know what the answer means."

Whenever you get stuck, keep in mind that maybe you don't really know what the question is – and when that happens, go back to definitions to clarify the problem.

Always ask "How much?". I'm a big fan of Deirdre McCloskey. Her works (in particular The Rhetoric of Economics and Economical Writing) should be compulsory reading for any PhD student in Economics.

In one brilliant essay, The Secret Sins of Economics, McCloskey explains that a good science should do two things: it should think and it should look. It should make theories about how the world works, and it should observe how the world functions to see if the theories are any good.

McCloskey's claim in the essay is that economics all too often *pretends* to do both, but in reality does neither. That is, she makes the claim that economics "engages in two activities, *qualitative theorems* and *statistical significance*, which *look* like theorizing and observing, and have (apparently) the same tough math and tough statistics that actual theorizing and actual observing would have. *But neither of them is what it claims to be*."

A lot of theoretical work in economics consists of "existence" theorems, which show that starting from a set of assumptions we can reach a certain conclusion. The problem, says McCloskey, is that we can always come up with a slightly different set of assumptions which would lead to drastically different conclusions. And that is exactly what economists have been doing in much of their theoretical work: changing some assumptions to arrive at a different conclusion. The problem here is that there are infinitely many possible assumptions you can make (on convexity, rationality, information sets, and so on), and so you can prove practically any conclusion you want.

McCloskey argues that theorists are too often satisfied when they have demonstrated this "existence" conclusion – that an equilibrium exists or does not exist; that something is Pareto efficient or not – without trying to assess the "how much" question. For instance, you could write a nice theory paper to show that in this or that situation, the market outcome is inefficient. Interesting result – but how problematic is it? Has the pie become 2% smaller or 60% smaller? Surely, that makes a difference, and we would like to know. McCloskey argues that in physics, which is not afraid of using a little bit of math now and then, theory papers are much more concerned with trying to figure out how big the theoretical effect would be.

The second secret sin is that much empirical work in economics is also not really asking "how much" but instead, like the theoretical work, seems to focus on *existence*, through a sad tradition known as "significance testing".

By now, you know how that works: you run a regression or some other statistical test, you look at the t-statistic or the p-value or the number of stars your statistical software puts next to the coefficient, and then you decide whether an effect matters or not. If the t-statistic is larger than 1.96, or equivalently if the p-value is smaller than 0.05, we confidently say that variable X is statistically significant at the 5% level, usually awarded two stars.

Economists sometimes forget that there is actually a coefficient to be interpreted too – instead, they would look at the statistical significance and conclude that some effect exists or does not exist, while hardly talking about how big the effect is. There are so many problems with statistical significance I can't even begin to discuss them all – but I'm listing some resources in the bibliography. For now, let's focus on this strange phenomenon that we sometimes calculate coefficients without really thinking about *how big* they are. Try to pay attention to it next time you are attending a seminar or reading a paper, and you'll see this happens more often than you would think.

Whether you are looking at theory or data, asking "how much" is what economists should do. To take one fascinating example, let's look at the economics of migration. It's not that easy for people to migrate from poor countries to rich countries, mostly because of regulation. Preventing the mobility of labor leads to efficiency losses similar to those with trade distortions. So, if we would open all the borders and allow free migration, there would be an efficiency gain.

So much for theory; how big would those effects be? In a thought-provoking paper, Michael Clemens (2011) summarizes studies on the efficiency effects of removing all barriers to trade, capital flows, and migration, respectively. If the world moved to free trade or completely free international capital markets, the gains in terms of global GDP would be somewhere between zero and four per cent of GDP. That's not a growth rate; that would be a one-time increase in global GDP. By contrast, if the world moved to a policy of complete freedom of migration, the increase in global GDP would be astonishing: somewhere between 67% and 147% of global GDP, according to the estimates. Isn't that surprising? But as Clemens goes on to show with some "back of the envelope" calculations, it makes sense, so these numbers are probably a good approximation. The point, of course, is that it does not really matter whether the actual number is 67% or 147% increase in GDP; the point is that the efficiency gains would clearly be much, much bigger than those of trade liberalization or liberalization of capital markets.

This is why economists should ask "how much" questions rather than "existence" questions. Of course we can write a theory model to show that trade liberalization would improve efficiency, and we can make a similar model for migration. In empirical terms, we could find that both free trade and free migration have a "significant" effect on efficiency. But what we really want to know is: how much?[14]

Clemens' paper shows that migration is potentially much more important than trade liberalization (although economists have studied trade much more intensely than they have studied migration). Now, migration is typically a sensitive issue for all kinds of reasons. But no economist could seriously look at those numbers – 147% increase in global GDP! – and shrug that migration is "too difficult". The potential benefits for humanity are so large (and mostly concentrated among the poor) that we should at least think about the policy implications and what could be done to minimize the drawbacks of open borders. Policy makers, voters, and the global poor ask "how much", and so should you.

Feedback: get it early, get it often, get a lot of it. I think this subtitle is self-explanatory, but anyway: your research group is full of smart people with more experience than you have, and you should make us of that opportunity. You don't want to spend days or weeks working on some problem only to discover that your entire approach is wrong, or that someone else has done exactly the same thing already, or that you're working on the wrong problem. More experienced people can point this out quite early. It may not always be nice to get criticism on your work, but it can save you lots of time down the road, and I sure wish I had done this more often. Talk to colleagues, send drafts around, and give informal seminars where you can get some "friendly fire".

You might have a romantic notion of the lonely scientist working in the attic on the Big Solution to the Important Question, but that's not how actual research works. Or even if it was, it would

[14] I'm guilty of this myself. Three of the papers in my PhD offer a theoretical model without providing an answer to the "how much" question. For instance, one of my papers is about "planting rights", a tool used in the European Union to restrict wine production. My model explains how it works and what the theoretical effects are but I have not calculated elasticities or demonstrated how big or how important the efficiency losses are. Originally, that was the plan, but finding data proved to be a daunting task.

not be the most efficient way to do research. As Keynes once said: "It is astonishing what foolish things one can temporarily believe if one thinks too long alone, particularly in economics."[15]

Write. Writing and research are not two separate activities. While you're writing, you'll discover that some of your arguments were not as solid as they seemed in your head; at the same time, putting thoughts down on paper will stimulate new thoughts. So, writing is an essential research skill.

Of course, writing is also important because at some point you need to communicate your findings to others. Knowing how to write well is useful at that point. Again, writing well is a skill that can be learned – for instance by reading and using McCloskey's "Economical Writing".

Research questions. Now we come to a topic about which I know even less than I do about all the other stuff in this essay: how to find a good research question. It's a question I've struggled with for several years, and I don't think I've found a good answer yet, but I hope these thoughts can help nonetheless.

A good research question should satisfy a few criteria.

First, there should be a **market** for the paper you want to write. If you start from an existing literature and you've found an original way to contribute to a debate, that's great. By contrast, you might have a nice idea but no clear sense of who would be interested in those results. That's not good. Journals want to publish papers that will get cited. And, from a more philosophical point of view, you probably want to work on a topic which is important enough to attract attention of others. So, the first question to ask is: is there a demand for this paper? I've made this mistake a few times, because I found an interesting topic (taxes on beer in the 16th-17th century Low Countries, say) and started to write about it, without having a clear idea whether anybody else would actually care about the results. It can save you a lot of stress if you know that you're working on a topic other people care about.

Another way of putting this is that you ideally want your paper to be part of the "flow" of a conversation among scientists, where people pick up on each other's arguments.[16] In a natural conversation, every contribution relates to what has been said earlier, and acknowledges what other people said; in turn, your conversation partners will be interested in what you have to say if it's relevant to the discussion. You don't want to be the awkward kid who just blurts out something, then retreats into the shadows – your contribution would not really "link" to any conversation and would not be of interest to many people.

[15] I got this quote from Paul Krugman's essay "How I Work", which is excellent reading material in itself: http://web.mit.edu/krugman/www/howiwork.html

[16] This metaphor is due to Arjo Klamer (2007).

This does not mean that you should never come up with original ideas of course; but if you do, make sure you explain how they relate to previous discussions or why they are a worthy topic of conversation.

Second, you should have the **capabilities** of actually writing the paper. You can set yourself the goal of writing The Paper That Explains Everything (for which there would probably be a market), but that doesn't mean it's realistic. I once embarked on a project to mix endogenous economic growth, trade, and network theory in one model. I had a lot of fun for about three or four weeks until I realized there was no way I would be able to pull this off. In terms of capabilities, what matters is not what you can do right now, but what you can learn over time. If you're at the start of the PhD, everything may seem overwhelming; but if you pick a topic and focus on it, you can master the literature and the techniques, and gain the capabilities. So, assessing this aspect can be a bit tricky. Some questions to ask are:

- What kind of capabilities would you need to write this paper? Don't try to list *every* capability you would need ("literacy") but focus on the most important ones. Would you need to know more about some statistical technique? Would you need to read up on an immense literature? Would you need to get your hands on a fancy dataset?
- Now that you made a list of the key capabilities, let's see what kind of capabilities you already have. Are you good at statistics? Do you already know the relevant papers? Do you already have a fancy dataset that would suit the purpose?
- For the capabilities you don't have, let's look around and see how easy it would be to obtain them. For instance, perhaps you don't know the relevant literature because you just started the PhD, but your research group is filled with people who know the field inside out. In that case, it would be relatively easy to learn from your colleagues what the key papers are. It could be that you need a difficult statistical technique which the girl in the office next door happens to master; it might make sense to collaborate on the project. Or perhaps you can take some specialized training.

Third, you want to make sure the **competition** is not already doing exactly the same thing. You don't want to waste months writing a paper, only to discover that someone else already had the idea and beat you to it. A first check here, of course, is to do a quick literature search to see if someone else has a journal article or working paper on the topic you're interested in.

Fourth, you ideally want your paper to have **potential for follow-up work** for yourself. This could mean different things. One possibility would be that your paper raises a bunch of questions which you can explore further. For instance, Nathan Nunn's original paper on the negative impact of the slave trades on economic development in Africa (Nunn, 2008) led to a paper investigating the impact of slave trades on mistrust (Nunn and Wantchekon, 2011) and a paper on how 'rugged' landscapes affect this link (Nunn and Puga, 2012). Another possibility is that your paper will teach you a lot about some analytical techniques (RCTs, ArcGIS, numerical methods, …) which you can use for other papers.

Ironically, I came up with this way of thinking about research while doing job interviews for consulting positions. The checklist here is quite similar to a method sometimes used by companies when they think about launching a new product or enter a new market. I recently used a version of this checklist to decide not to pursue an interesting idea for a paper (on the health effects of the switch from vodka to beer in Russia) with a colleague. There's a market for that paper, and my colleague speaks Russian (which helps), but we don't really know the health economics literature; there's a guy with a PhD from Berkeley who is Russian, works at a Russian university, and has written several papers on very similar topics; and there's not much scope for follow-up work later.

Of course, I realize that it's much easier to write down a list of criteria than to actually find and delineate a research question. I can't offer more guidance at this point, mainly because I don't feel like I've mastered this area myself. Keep looking for tricks to find good research questions, and talk to people who seem to have an instinct for identifying good problems.

Get out of the comfort zone. During my PhD, I was fortunate enough to visit UC Davis. Other colleagues who have had the opportunity to spend some time in other institutes agree with me that this is a wonderful experience. You can meet new people, discover new ideas, get feedback on your own work, and see first-hand how other environments are organized. If possible, try to get an experience like this, whether it is doing fieldwork in developing countries (something I've unfortunately never been able to do), an internship in an international organization, or a research visit to another university.[17]

Tricks for Productivity

Finally, here are some tricks which could be useful either while studying or while doing research.

Mind your energy levels. You are not a robot, and as a consequence you are not capable of working at full capacity for several hours a day and several days on end. In fact, most robots aren't capable of doing that either; they need some maintenance now and then. The problem is even worse with humans. Your mind is a part of your body, and just like the rest of the body it needs proper nutrition and proper rest.

Academia often fosters a macho culture where it looks cool to pull all-nighters fuelled by coffee or energy drinks and ignoring weekends. In reality, there's no way that's efficient. Your mind cannot function without sleep, without proper food, and without exercise. At the risk of sounding like your mother, make sure you get enough rest, make sure you eat your fruits and veggies, and go for a walk now and then. You'll be ten times more productive afterwards. Who knew your mother's advice would actually come in handy during the PhD?

[17] Plus, it looks good on your resume!

One particular thing I discovered is that my energy levels can fluctuate a lot if I don't eat regularly. For instance, if I eat lunch at 1 PM, I can feel my energy go down around 3 or 4 PM. One strategy would be to drink coffee, but that only works for about ten minutes. The best strategy is to eat small things at regular intervals (a banana, say). This may sound totally ridiculous or trivial, but I've become a lot more productive since I discovered this. If I don't do this, I am usually completely exhausted (mentally and physically) by 6 PM. Try to figure out for yourself how your energy levels usually vary during the day, and experiment to see how you can optimize it.

Also, take proper breaks. When I was an undergrad student, I would feel guilty about not studying, and so I would not allow myself to take a real break. Instead, I would sit at my desk staring at my notes for an hour. What a terrible waste of time! You're not studying, and you're not relaxing either. Try to be disciplined about this. Relaxing is serious business; don't do it in a half-assed way. Go do something totally unrelated (take a walk in the park, go for drinks with friends, go watch a movie). You'll be more efficient afterwards, so there's no need to feel guilty, as long as you strike a good balance.

Avoid the Cognitive Switching Penalty. Whenever your mind switches from one task to another, there is a switching cost. Your mind needs to figure out the context and all the relevant information for the new task, and this takes a bit of mental energy and time. As a consequence, if you're constantly switching from task A to task B and back, you are basically wasting energy and time – a phenomenon known as the "cognitive switching penalty". Try to avoid this at all cost. If you're studying, turn off your cell phone and your e-mails. If you don't strictly need your computer for studying, shut it down. Find a way to study without distractions or interruptions, and focus on one thing at a time. The cognitive switching penalty is also true for different topics inside one course. It's okay to go through your course notes quickly to get a first overview of the contents and structure, but once you start studying, focus on one topic exclusively and don't go switching back and forth between topics unless you absolutely have to for some reason.

Don't sweat the small stuff. Think for a moment about all the tasks you have. Some will be urgent, others will be important, but those two groups of tasks do not necessarily coincide. In fact, you can arrange your tasks in a matrix, putting "unimportant – important" on one axis and "not urgent – urgent" on another axis.

Clearly, if a task is both important and urgent, you should tackle it. And if a task is not urgent and also not important, you can ignore it. But the other two boxes are trickier. If you're not paying attention, it's easy to work on projects or tasks that are urgent but not important.

One problem is that psychologically speaking it feels satisfying to check things off a to-do list. Most of the time, however, the important projects are vague and don't really fit well on a to-do list. This makes it tempting to spend your time on less important tasks.

Moreover, once you're in the mindset of doing all kinds of urgent and small tasks, it can be difficult to calm down, concentrate and focus on big, possibly fuzzy, important projects.

One trick I've found useful is to start working on an important thing first thing in the morning. For instance, when I need to figure out a theoretical model, I will first work on the model for some time before turning on my computer. The absolute worst thing I can do, is check my e-mails in the morning; that would destroy my concentration completely, so I try to avoid that.

Some Closing Thoughts

My original idea was to write a few short pages, and I'm a bit shocked to discover this essay has turned out to be pretty long. I hope the length and the content don't scare you away! While the PhD can be very challenging, I have learned a great deal and met so many interesting people that the experience was definitely worth the trouble. In the words of Ariel Rubinstein (2013),

> Remember that you are one of the most privileged people on earth. Society has given you a wonderful opportunity. You are supposed to do whatever you want, to think about new ideas, to express your views freely, to do things in the way that you choose and on top you will be rewarded nicely. These privileges should not be taken for granted. We are extremely lucky -- we owe something in return.

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Instead of just listing everything in alphabetical order, I've grouped the references together by theme, listing first some works on the psychology of doing a PhD, some works offering useful tips and tricks, and some books offering a more philosophical look at research in economics. I then list some popular science books by theme, and finally some other works mentioned in the essay.

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The importance of stupidity in scientific research

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Accepted 9 April 2008
Journal of Cell Science 121, 1771 Published by The Company of Biologists 2008
doi:10.1242/jcs.033340

I recently saw an old friend for the first time in many years. We had been Ph.D. students at the same time, both studying science, although in different areas. She later dropped out of graduate school, went to Harvard Law School and is now a senior lawyer for a major environmental organization. At some point, the conversation turned to why she had left graduate school. To my utter astonishment, she said it was because it made her feel stupid. After a couple of years of feeling stupid every day, she was ready to do something else.

I had thought of her as one of the brightest people I knew and her subsequent career supports that view. What she said bothered me. I kept thinking about it; sometime the next day, it hit me. Science makes me feel stupid too. It's just that I've gotten used to it. So used to it, in fact, that I actively seek out new opportunities to feel stupid. I wouldn't know what to do without that feeling. I even think it's supposed to be this way. Let me explain.

For almost all of us, one of the reasons that we liked science in high school and college is that we were good at it. That can't be the only reason – fascination with understanding the physical world and an emotional need to discover new things has to enter into it too. But high-school and college science means taking courses, and doing well in courses means getting the right answers on tests. If you know those answers, you do well and get to feel smart.

A Ph.D., in which you have to do a research project, is a whole different thing. For me, it was a daunting task. How could I possibly frame the questions that would lead to significant discoveries; design and interpret an experiment so that the conclusions were absolutely convincing; foresee difficulties and see ways around them, or, failing that, solve them when they occurred? My Ph.D. project was somewhat interdisciplinary and, for a while, whenever I ran into a problem, I pestered the faculty in my department who were experts in the various disciplines that I needed. I remember the day when Henry Taube (who won the Nobel Prize two years later) told me he didn't know how to solve the problem I was having in his area. I was a third-year graduate student and I figured that Taube knew about 1000 times more than I did (conservative estimate). If he didn't have the answer, nobody did.

That's when it hit me: nobody did. That's why it was a research problem. And being *my* research problem, it was up to me to solve. Once I faced that fact, I solved the problem in a couple of days. (It wasn't really very hard; I just had to try a few things.) The crucial lesson was that the scope of things I didn't know wasn't merely vast; it was, for all practical purposes, infinite. That realization, instead of being discouraging, was liberating. If our ignorance is infinite, the only possible course of action is to muddle through as best we can.

I'd like to suggest that our Ph.D. programs often do students a disservice in two ways. First, I don't think students are made to understand how hard it is to do research. And how very, very hard it is to do important research. It's a lot harder than taking even very demanding courses. What makes it difficult is that research is immersion in the unknown. We just don't know what we're doing. We can't be sure whether we're asking the right question or doing the right experiment until we get the answer or the result. Admittedly, science is made harder by competition for grants and space in top journals. But apart from all of that, doing significant research is intrinsically hard and changing departmental, institutional or national policies will not succeed in lessening its intrinsic difficulty.

Second, we don't do a good enough job of teaching our students how to be productively stupid – that is, if we don't feel stupid it means we're not really trying. I'm not talking about 'relative stupidity', in which the other students in the class actually read the material, think about it and ace the exam, whereas you don't. I'm also not talking about bright people who might be working in areas that don't match their talents. Science involves confronting our 'absolute stupidity'. That kind of stupidity is an existential fact, inherent in our efforts to push our way into the unknown. Preliminary and thesis exams have the right idea when the faculty committee pushes until the student starts getting the answers wrong or gives up and says, 'I don't know'. The point of the exam isn't to see if the student gets all the answers right. If they do, it's the faculty who failed the exam. The point is to identify the student's weaknesses, partly to see where they need to invest some effort and partly to see whether the student's knowledge fails at a sufficiently high level that they are ready to take on a research project.

Productive stupidity means being ignorant by choice. Focusing on important questions puts us in the awkward position of being ignorant. One of the beautiful things about science is that it allows us to bumble along, getting it wrong time after time, and feel perfectly fine as long as we learn something each time. No doubt, this can be difficult for students who are accustomed to getting the answers right. No doubt, reasonable levels of confidence and emotional resilience help, but I think scientific education might do more to ease what is a very big transition: from learning what other people once discovered to making your own discoveries. The more comfortable we become with being stupid, the deeper we will wade into the unknown and the more likely we are to make big discoveries.

Journal of Cell Science

HOW TO SOLVE IT

UNDERSTANDING THE PROBLEM

First.

You have to *understand* the problem.

What is the unknown? What are the data? What is the condition?

Is it possible to satisfy the condition? Is the condition sufficient to determine the unknown? Or is it insufficient? Or redundant? Or contradictory?

Draw a figure. Introduce suitable notation.

Separate the various parts of the condition. Can you write them down?

DEVISING A PLAN

Second.

Find the connection between the data and the unknown. You may be obliged to consider auxiliary problems if an immediate connection cannot be found. You should obtain eventually a *plan* of the solution.

Have you seen it before? Or have you seen the same problem in a slightly different form?

Do you know a related problem? Do you know a theorem that could be useful?

Look at the unknown! And try to think of a familiar problem having the same or a similar unknown.

Here is a problem related to yours and solved before. Could you use it? Could you use its result? Could you use its method? Should you introduce some auxiliary element in order to make its use possible?

Could you restate the problem? Could you restate it still differently? Go back to definitions.

If you cannot solve the proposed problem try to solve first some related problem. Could you imagine a more accessible related problem? A more general problem? A more special problem? An analogous problem? Could you solve a part of the problem? Keep only a part of the condition, drop the other part; how far is the unknown then determined, how can it vary? Could you derive something useful from the data? Could you think of other data appropriate to determine the unknown? Could you change the unknown or the data, or both if necessary, so that the new unknown and the new data are nearer to each other?

Did you use all the data? Did you use the whole condition? Have you taken into account all essential notions involved in the problem?

How To Solve It

CARRYING OUT THE PLAN

Third.

Carry out your plan.

Carrying out your plan of the solution, *check each step*. Can you see clearly that the step is correct? Can you prove that it is correct?

LOOKING BACK

Fourth.

Examine the solution obtained.

Can you *check the result?* Can you check the argument?

Can you derive the result differently? Can you see it at a glance?

Can you use the result, or the method, for some other problem?