Dear reader,

This is a short essay I wrote for my students here at Lund University in Sweden. My aim was not to give them more advice on “methods” or “theories” — they get too much of that as it is — but instead to try to say something about what I take to be the very core of the research process — the importance of asking good questions. An academic paper should be like a detective story, it has a dead body in a library already in the first chapter. The essay is intended for under-grads but I presume PhD students could find some inspiration here too.

If you have any comments or questions, email me at erik@ringmar.net

Good research!

Erik

Lund, Sweden, November 2015.

People write many different kinds of things. What you write, and for which audience, will vary and the advice given to writers varies accordingly. The pieces of advice that follow below refer to the kind of writing we do at universities. What concerns us is how to write essays, theses and academic papers. Or put slightly more pretentiously: it is concerned with the question of how to do research. A paper in a freshman or a sophomore class is just the beginning, and much of the advice below can for that reason appear slightly overblown. Come to think of it, that's probably the case. At the same time, it's good to understand the process since the process is the same for all kinds of research from the freshman's term-paper to PhD dissertations and on to those seminal tomes which a scientist produces in his or her 50s.

To write a thesis at a university is to enter a new and at first rather intimidating world. Previously, if you had a question, you simply looked up the answer on-line or searched for it in some old book. On-line or in the book you would find knowledge that someone else had made available to you. At university, however, you learn not only to be a consumer of knowledge but also a producer. Now it's no longer enough to look up answers other's have provided, instead you must start providing the answers yourself. You are your own authority; you are your own source of information. To be an authority and a source of information is a great responsibility. It is no doubt a pretentious and perhaps slightly scary position to be in, but it is also exciting. If you have problems presenting yourself as an authority, you will have to begin by pretending. After a while you will start to believe in your own hype and your own pretensions. Before long you'll
be able to hold forth with authority on most issues. Welcome to our world!

There are of course many kinds of research and also many kinds of academic papers you can write. Researchers have different goals. Sometimes they primarily want to describe something. To describe something sounds like a fairly easy enterprise, but that depends of course on what it is we are trying to describe. When Galileo Galilei observed hitherto unobserved heavenly bodies through his telescope, the first and most obvious questions was what he had seen and how to describe it to others. But it is also possible to describe the already well-known in new ways and thereby to convince others that they should look at the world in a new fashion. This is for example what Adam Smith did in his *Wealth of Nations*, 1776, where he presented the economic market as an independent system governed by its own laws. No one had previously described the economy that way.

But most research concerns not descriptions as much as explanations. Research is about understanding things that have not been understood before; to find answer to why things are the way they are and work the way they do. Really good, really fascinating, research is organized like a detective story. In a book by Agatha Christie there is always a person found dead in the first chapter — often a young woman on the floor or a library. The question is obvious: who killed her and why? The rest of the book is concerned with finding an answer to that seemingly trivial question — to find the murderer and the motive. Nothing else matters. Since we always know what detective stories are about they are easy to read. This is also why they are easy to write. You put the puzzle together, and then there is nothing more to do. Detective stories are also quick to write — Agatha Christie used to write two or three per year.

A good academic paper is like a detective story. In the first paragraph there should be a body in a library. The reader should be introduced to something strange, something that isn't the way it should be, something that needs to be explained. The rest of the paper provides the explanation. Nothing more, nothing less. All the while


both the author and the reader are perfectly aware of what the paper is about, and the paper is finished when the question has been satisfactorily answered. In the conclusion, at the end, the researcher gathers all the facts pertaining to her investigation for a final encounter in the library, where he or she goes through the case again, and identifies, to everyone’s amazement, the most important causal variables.

There is thus a great difference between a research topic and a research question. The world is full of interesting topics and as human beings there are always a lot of things that make us excited. But you cannot do research about a topic. Topics are too big; they never really start and they never really end either. There is always something that can be added, a background that can be given, or some consequence that can be better brought out. All topics relate to other topics and before long we end up with an enormous topic-web where everything is dependent on everything else in some fashion or another. If you start to untangle this web you will never finish. There are pathetic examples of scholars who haven’t understood this point and who write five volumes about a topic before they realize that their lives aren’t long enough to finish what they have started. A question doesn’t work that way. A question you pose and then you answer it. When the question is answered, the research is done. This is why it is important to formulate the question well. A well-formulated question is a question that can be answered. A badly formulated question is a question that only pretends to be a question but which actually only points to a topic. If you are a PhD student this provides a great opportunity to save years of work. The better the question, the sooner you can finish and get your life back.

Unfortunately it is very difficult to formulate a good research question. In many ways this is the most difficult part of the work — representing, perhaps, something like 50 percent of the overall effort. I have written books where it has taken me several years to understand what I wanted to know. When the question finally comes to you, it is profoundly embarrassing to acknowledge how lost you have been, but it is also
wonderful to know that the right path is straight ahead. After that the work goes quickly. It is easy to write once you know what you are doing. It is just like talking in front of an audience. If you have something to say, an hour can be short and it is easy and fun to talk. But if you have nothing to say, two minutes can be an absolute eternity. And just as in the case of a speaker, it is crucial to win and to keep your audience. Compare the working conditions for a stand-up comic who doesn’t have many seconds before the rotten tomatoes start to fly. Once you’ve lost your audience, it is next to impossible to win it back.

This is not least the case in relation to your supervisor or examiner. Supervisors and examiners are human beings too — well, most of them anyway — and this means that they easily lose interest. They often have a whole pile of papers in front of them on their desks and only a couple of hours to get through the whole thing. Often they make up their minds very quickly regarding what sort of a paper they are reading, often only after having read a few pages. Of course research should not be judged on such a flimsy basis, and maybe you are lucky and will find a supervisor or examiner who has more patience. But it is a mistake to trust this to be the case. So why not make it easy for the reader? Make it clear already in the introduction to the paper that you are worth listening to. Don’t start with a “historical introduction” or something equally boring which you think you have to say before you start saying what you really have in mind. Put the best points first and use the rest of the paper to explain, deepen and clarify your argument. Don’t worry if the paper seems shorter this way and that you don’t reach the required word length. Good papers are never too short but always exactly the right length. They finish when they have done what they set out to do. The end when they have answered the question they set out to answer.

Also don’t take the metaphor of a “supervisor” or “advisor” literally. Although the idea of being “supervised” can sound slightly threatening, at least a supervisor is someone who cares about you and what you write. And “advisors” are obviously friendly
people since their job is to offer advice. But don't bet on it. Even if you are lucky and your supervisor/advisor takes his or her work seriously, it will not take long before you are more of an expert on the topic at hand than they are. The only thing a supervisor/adviser can do at this stage is to come up with suggestions for how research questions can be formulated and results judged. That is, you have to do all of the hard work yourself. It is lonely to write things and it really helps if you like your own company. Not everyone is suited to be a research, and that's probably a good thing.

What do you then do to find a good research question? The task here is to learn how to construct problems; that is, how to problematize the world around you. In order to problematize something you must wonder about what you see, worry about things that happen, and hopefully to wonder and worry about things that no one else worries and wonders about. One of my teachers, a very famous American political scientist, used to say: “The problem with students these days is that they read too much. They must think!” What he had in mind, I think, was that reading can turn into a pretext for not doing what research really should be about — posing questions and answering them. To read is hardly better than other things that researchers do when they don't want to work — download pdfs off the web or gossip about students. A great advantage of problematizing as a research activity is that it can be carried out whenever you have a few moments to yourself — when you are on the bus, say, about to fall asleep or suffer through a particularly boring lecture. Turn off the phone and just think!

It might seem obvious, but the point is easily missed — don't write about a question you don't want to find the answer to, or about something you don't really care about. If you don't genuinely want to find an answer, or if you don't genuinely care what the answer might be, chances are no one else will care either. The best research questions are simple and obvious and they often sound naïve, at least at first. If your question is unclear, ambiguous, or contain more than two qualifying subclauses, it is probably the wrong question. Change to something you can formulate in a more
straightforward fashion. In a way the best questions are the questions children ask. Children are good at noticing that emperors have no clothes and they ask the questions grownups miss. That is to say, many questions are not “obvious” until the formulation of the question has made them so. The best research questions are consequently a sort of discovery. They require a new way of looking, and a new way of looking often requires new perspectives. Children ask good research questions since they have their own, not the grownup, view of the world. Don't be afraid to be childish!

I once traveled between Singapore and Laos together with my children. Despite the fact that the two countries are located in the same part of the world and share some historical and cultural influences, they are worlds apart. Singapore is one of the richest countries in the world and Laos one of the poorest. After having walked around Vientiane, the Laotian capital, one afternoon, my oldest daughter asked: “Dad, why are people so poor here?” That is of course a great research question. Why indeed are people so poor in Laos and so rich in Singapore? There must be a reason? What could that reason be and how would we ever find out? The Chicago economist and Nobel Prize laureate, Robert E. Lucas, asked himself the same thing: Is there, asked Lucas, some measure a government can put in place in order for countries that grow slowly to be able to grow at the same pace as those that grow quickly? If so, what? And if not, why not?

The consequences for human welfare involved in questions like these are simply staggering: once one starts to think about them, it is hard to think about anything else.

Yet most people prefer not to worry about matters such as these, or they trust that there are others who know the answers. This is what makes researchers different from other people. Researchers are people who are being kept up at night by their research questions, and they never trust that others know the answers. It's like having a rash somewhere on your body. It itches and itches and you cannot stop yourself from scratching. Research is nothing but intellectually motivated scratching. Looked at in this
way, research might appear as a ridiculous and perhaps as a pointless occupation, but try not to itch if you can. After a while you go completely mad.

If you don’t know what to write about — and few students do when they start their work — it might help to look for a pattern of some kind. Compare how something varies over time or varies between two cases, countries or regions. The pattern you discover in this way will often need an explanation and the explanation is what your research paper will supply. The simplest way to go about this is to look for some piece of statistics. Why did Chinese export grow so dramatically after 2001? Why have Indian defense expenditures varied the way they have during the last ten years? Why is Vietnam attracting more foreign investments than Burma? Why are Finnish schools better than Swedish? Why is the French health-care system cheaper than the American? It might of course be rather flatfooted to only occupy yourself with numbers, but numbers always require an explanation which is qualitative rather than quantitative. The numbers help you identify puzzles which your research goes on to solve. The solutions always go way beyond the numbers themselves.

There is a very simple way to find out whether you have found a good question or not. When your mother, or perhaps a fellow student in your dorm, asks you what you are writing about, let them know. If you can explain yourself in a simple and comprehensible fashion, you probably know what you are doing. And when you explain yourself, look at their eyes. You can always tell from the eyes of a person if they are interested or not. Really good questions will make their eyes lit up. Bad questions make their eyes glaze over. If your mother or your friend then adds something to the effect that “wow, that sounds really interesting!,” you know for sure that you are on the wrong track. To say that something sounds “really interesting” is to say absolutely nothing.

I have stressed the importance of a research question since everything else involved with the writing of an academic paper follows from that. The rule is simple: it is only that which helps you answer your question that should be included in the paper,

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everything else you can leave behind. Take the use of sources. There are different kinds of sources — texts, statistics, interviews, observations — but what counts as source material is entirely determined by the question you ask. *The National Enquirer* is not normally regarded as the best source of knowledge about the world, but if we ask a question about how images of celebrities have changed in the course of the twentieth-century, it becomes an important primary source. Often we don't know what the sources look like before we start our research, and if the sources don't exist — if, for example, we are studying something secret or very controversial — we might have to redefine our research project. When you are looking for a research question, always think in terms of primary sources. That is, which are the primary sources that can help me answer this particular question?

In general it's difficult to write about big questions. Big questions are inevitably complex and contain too many variables and causal relationships. It is a lot easier to write about small question. But small questions can of course be quite irrelevant. The best questions are for that reason small but significant. They are small questions that allow us to give big answers, questions that concern a lot more than they actually investigate. I just finished a book about how a combined Anglo-French army destroyed the palace of the emperor of China outside of Beijing during the Second Opium War in 1860. The event as such might not be all that important, but by explaining what the Anglo-French army was doing it is possible to understand a lot about European imperialism in China, and in other places too, during the nineteenth-century. In fact, I even argued that my research can help us understand American attitudes to Iraq and other non-Western countries. Perhaps I exaggerated. I probably did.

The same applies in the case of the choice of "methods" and "theory." Scientific method concerns which procedures we should employ in order to find an answer. The choice of procedure will for that reason always be connected to the sources we identify. If we wonder what the British people think of EU membership, we must find a way of

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asking them. The source here will be opinion surveys and the methods we employ will be the ones appropriate for the analysis of opinion surveys. If we, on the other hand, ask what the relationship might be between economic development and democracy, we must find a way of measuring that relationship. Here too the method will follow from the sources. Some researchers are stats jocks while others advocate historical or interpretative approaches. This is just another way of saying that researchers tend to pose different kinds of questions, questions that require different kinds of answers. But it is not possible to say that one method is better than another. It all depends on what it is that we want to know.

The same argument applies to the question of “theory.” Students are often stressing themselves out regarding “theories” — what they are and how they should be used. Theory sounds complicated and it is theory, we are sometimes told, that distinguishes academic from other kinds of writing. In order to be scientific — in order to sound smart — “you have to have a theory.” Usually students are introduced to the theories they can choose between already in their introductory classes when they learn how various famous researchers have gone about explaining the world. The study of international politics provides an illustration. Here there are Realists, Pluralists, Idealists, Liberals, Constructivists, Feminists, Structuralists, Post-structuralists, Marxists and a large number of other -ists, and all their writing seems to take place within one or another of these intellectual folds. To write theoretically thus becomes to adopt one of these perspectives. We chose a theory much as we chose how to dress ourselves in the morning or decide what to eat for dinner.

But this is not how things should work. A theory is not something that we choose separately from the questions we ask. A theory is a way to define a situation, identity variables and causal connections. A theory is a tool which helps us carry out a certain intellectual work. A certain theory can be like a hammer, another like a screwdriver or a saw. If we want to build a bookcase we don’t need a pneumatic drill, but a pneumatic
drill might really come in handy if we want to redo our driveway. In exactly the same fashion, the questions we pose will come to determine the theory we need. In many situations there are several theories that might fit the bill. In that case, we test the alternatives against the data we have discovered. A person calling herself a Realist, Pluralist, Marxist, Feminist or whatever, is a person who only can think in one fashion. For a person who only has a hammer, everything looks like a nail. Such a person has a problem. Good research forces us to think in many different ways and to use many different kinds of tools. Often at the same time.

But this is nothing to lose sleep over. Try instead to simply forget what they taught you regarding “theories” in your methods classes. And you should under no circumstances add “theories” to your work in order to sound more “scientific.” You don't sound scientific as a result, only pretentious. Instead, start with the question. When you have found your dead body in the library, look for the kinds of intellectual tools that can help you catch the murderer. Once you do this, the theoretical arguments will come flowing back into the analysis by themselves and without you having to worry about it. Theory is nothing but another name for thinking, after all, and once you start thinking about your problem, you will theorize without even noticing it. When you have given your explanation, and everyone is happy, there is nothing more to do. Adding more “theories” doesn't make the paper any better.

There is, we said, no essential difference between the questions that researchers ask and the questions that normal people, or children, ask. We now understand that there is no essential difference between the answers researchers and normal people come up with. The difference is rather that research has to be much more carefully executed. Good research is the product of a much deeper, more reflective, and more explicit process than what normal people have the time and energy for. Good research takes into consideration more information, more alternative explanations, and above all research must be subject to criticism from other researchers. We draw conclusions on
certain grounds, but the grounds must be exposed so that they can be criticized by others. This makes research into something different than beliefs, which need no proofs. It also makes research different from mere opinions, which are based in a certain person's outlook on the world. Good research gives better answers than what normal people provide, but the answers are not of a different kind.

I sometimes say that I do research since I never could deal very well with recreational drugs. Just like a recreational drug, however, research has the power to make the world into an amazing place and it can make you feel very good about yourself. Suddenly you realize something that no one else has realized before. As a result you feel not only smart, but invincible. You rule the world. But before long the buzz will always vanish and you are left with a headache, and your usual self-doubt, and then you realize that the deadline for the paper is tomorrow and in the end you're happy just to hand in the damn thing. But of course research is never actually about you, but it is always a collective enterprise. Everything we do, and ever can do, depends on the contributions of others. Research is also the most noble activity in which you can engage as a human being. It is a matter of our desperate attempt to explain and understand the world we have ended up in, and as a researcher — even as a writer of a freshman's term-paper — you are a part of this great project of exploration. Research takes place elsewhere in society too but it is only in the universities that research is a main preoccupation. Don't miss the opportunity to write a good paper.

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