Scholarship Skills Seminar  
Professor Farnham

How to Write a Two-Page Research Proposal

One of the purposes of this seminar is to ease your transition from student to independent researcher. Students sit back and follow the lead of their professor, for the most part. In order to get into a graduate program, you need to be smart, but you don’t need to show a lot of independent initiative. If you simply do everything your professors tell you (show up to lectures, do the readings, do the problem sets) and think a little bit along the way, you’re likely to do well enough on exams to get into grad school.

When you get to grad school, you continue taking classes, and therefore you continue in the following role of a student. Even in grad school, there’s very little to prepare you for the transition to doing independent research—to switching from being a follower to being a leader. You may work as an RA for a professor, in which case the professor probably instructs you on how to carry out some part of a research project. But that’s not independent research. You’re just continuing on in your role as student, following instructions handed down by a professor.

Some students come up with their own research ideas in grad school, and that’s what we want. Other’s continue in this follower-student role all the way through grad school, simply writing papers that their advisors suggest to them, with heavy guidance from the advisor on how to complete the papers. This is not doing independent research. If you find yourself acting like an RA throughout your Ph.D. research program, you should probably re-think your approach.¹ Students who remain followers in grad school are likely to flounder when they land jobs in which they are expected to do independent research. Getting tenure is not easy, and it is especially hard to get if you turn in a research portfolio that’s entirely coauthored with your advisor. Besides, doing your own research is more fun than doing other people’s research. So let’s start having fun.

Writing a weekly research proposal may seem like a bit of a drag. But it’s a great opportunity to push yourself into creative mode with your research. It also trains you to keep an eye out for ideas that are your own—and we all know that idea generation is critical to a successful academic career. Also, in my opinion the most fun stage of a research project is the beginning. A new idea can be as grandiose as you want. Solve global poverty. Bring world peace. Think of something big (or at least something that can contribute to partially solving a big problem or resolving a

¹ Advisors, to some extent, have an incentive to aid and abet this bad behaviour. That's because it's easier for them to advise students on topics they're very familiar with, than to advise them on topics for which they have less expertise. Also, they're more likely to get coauthorship out of an idea they suggest to you than out of an idea you come up with yourself. Handing you a subsidiary part of their research agenda to work on is super easy for them (and you) and simultaneously advances their career. But it doesn't teach you to become an independent researcher!
big mystery). Coming up with an idea doesn't require you to do the research and publish the results. It's like coming up with the premise for a screenplay. You can sit down and, in a couple hours, map out a neat idea for a blockbuster thriller without technical constraints, the limits of special effects, or actor salaries limiting your thinking. The hard work is the execution, but without the idea there's nothing. So to start moving toward doing independent research, what you need first is ideas.

Of course, developing an idea does require that you come down to earth and think about such niceties as whether a tractable theoretical framework can be developed, whether data exist to test your hypothesis, whether others will find the idea to have sufficient merit to be worth publishing, etc. Many of these issues condemn neat research ideas to the scrap heap. I don’t want you to ignore these issues. But after focusing heavily on the big idea, I do want you to wade at least partway into these more practical issues.

The way we're going to do this, in this seminar, is that each week you will turn in a 2-page research proposal. This will consist of

1) An idea (a clear question you want to answer)
2) A discussion of why the question is important
3) A discussion of proposed methodology—how you would go about approaching and answering the question (this is where you start to come down to earth and think about whether your idea is executable)

Each week in seminar you will hand in your written proposal. Each week, I will hand back your proposal from the previous week with comments. I may elect to forward your proposals to your advisor. This is not so much to put extra pressure on you, but to allow your advisor to also give feedback on your proposals. Your advisor probably knows your area of research better than I do, and getting their perspective on what constitutes a good idea will assist in honing your idea-generating skills. I will also provide you feedback on your writing. Good writing is essential to getting your ideas across to as broad an audience as possible. If your writing is bad, only people extremely interested in your research area will read your papers. If your writing is good, more people will read your papers which will lead to more citations, more invitations to submit to journals, and greater career success. The ability to write a good research proposal is also essential to writing successful grant proposals and getting your papers accepted to conferences.

The Idea

Coming up with an idea is hard. Or at least, coming up with an interesting idea is hard. There are infinite things in the world that economists have yet to explain, but most of those things would make boring questions. What makes an

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2 Don't be constrained by your current area of research. Use this as an opportunity to explore new areas in economics.
An interesting question? There are myriad answers to this question, and your advisor may give a very different answer than I do.

Here are my thoughts on some things that make for a good research question (note: not all of these have to hold for a question to be interesting):

1) It is clearly articulated and answerable, at least in theory (e.g. the question of whether God exists is clearly articulated, but not answerable with economics).

2) It addresses an important problem. (Examples: an important methodological problem—statistical or theoretical, or an important social issue like global warming, crime, unemployment, business cycles, the viability of public pension programs, major social trends, etc. This list could go on for pages). One of the scariest questions you can get in a seminar is some smartass raising their hand to say, “Why do we care about this?” You’d better have an answer ready and it better be good, or people will start looking at their watches and heading for the door.

3) It’s generalizable. Many economics papers are, on the surface, about small issues. If it’s about a small issue (e.g. product differentiation in plumbing fittings), that’s only a problem if there’s not some larger message that can be taken away from your work. You might happen to have data on some tiny market that is pretty inconsequential on its own. But if what you learn from those data will extend to other more important markets, then you can justify focusing on the market you happen to have data for. But it’s very important that you make the generalizability of your research apparent to readers and audiences.

4) It solves a puzzle that others have noted.

5) It provides results that are counterintuitive. Economists love models that produce counterintuitive results—where people behave in some way that’s not what one initially expected (before writing down the model). Economists like models that produce counterintuitive results, because they make us look clever. More Guns => Less Crime, for instance. However, don’t get caught in the trap of trying too hard to look clever. Models that are too cute will get called out as such. Your goal should be to say something that’s a significant contribution to understanding, not something that’s just clever.

6) It brings together different subfields of economics in new ways. Taking a methodology in IO and applying it to a problem in International Trade may give you (and others) new insights into the problem. Some of the most innovative research occurs at the intersection of different subfields.

7) It’s simple. Theoretical models can get complex fast. Reduce your question down to the bare essentials (the key relevant tradeoffs) and write down a solvable model. Don’t try to throw everything in. It will frustrate you, your reader, and your audience (and probably cause your math software to crash). Same with empirical work. It’s fine to do complicated econometrics when you need it. But if you can make a basic point with a simple approach, and still have it be credible, you’ll get your message out to more people. Elegance (parsimony) is valued in economics. Doing everything with the highest tech theory and econometrics may well obscure your question and your answers.
8) It redoes bad previous work using better methods. If the only empirical results on a question have been found using OLS and you have reason to believe that endogeneity is a problem, then redoing that research with a good natural experiment, or some other workaround, can be a good way to go. Or if previous theoretical models leave out an important source of market failure, adding that aspect to the model could produce new, interesting, and important results.

Things that don’t make good ideas:

1) Preference-based arguments. Explaining why some people want to save the planet and others don’t isn’t all that interesting. We’re all born with different preferences, blah blah blah. Instead, focus on explaining different choices that people make, how those different choices arise from different constraints that people face, and how those constraints can be manipulated with policy.

2) Descriptive papers. There’s a place for descriptive papers. Many of us read descriptive papers to get background on some issue we’re interested in. It’s possible you will want a chapter in your thesis that carefully describes some trends in behaviour in order to set up the rest of your thesis. But in economics, the greatest value is placed by the profession on papers that explain behaviour or estimate causal effects, rather than on papers that simply describe behavior or interactions. So, if I’m faced with the choice of writing a paper that gives statistics on how many kids enroll in a new charter school program and the demographics of their families, OR writing a paper that hypothesizes how different families will respond to the new program and tests my hypotheses, I’d much rather do the latter. And I will get a better publication for it than for the descriptive paper.

3) Excessive incrementalism. Taking an existing paper and pushing it very slightly by using a marginally different dataset, or a marginally different model, or estimating results for blacks where results have only previously been given for whites can yield a paper. But only pursue papers like this if there’s an important reason to do so (like the dataset includes a really important control omitted from past studies, etc.). Ideally you want your research to be a creative endeavor, and slight tweaks like these don’t challenge you creatively. You can always have an undergrad RA do that paper for you when you get your first job and need some quick publications. But don’t make that the paper you use to sell yourself on the job market. People want to see that you’re a creative researcher, not someone nibbling at the margin.

Inspiration for Ideas

There are various sources of inspiration. Some good ones are:

1) Everyday life. Observe how people behave in different situations. Eventually you’ll come across something about which economics has things to say.
2) News. Read the paper, read the *Economist*, read news online. A good researcher keeps their finger on the pulse of the world, in order to spot important trends, because good journals want to publish papers about important things. Think about what’s happening in the world through an economist’s eye.

3) Blogs. People like to argue online. Listen to their arguments, try to distinguish good arguments from bad arguments, and think about these arguments. They can be the source of ideas.

4) Skepticism. Immediately drop that old follower-student habit of believing everything you read or hear. I’m not saying to believe nothing you read or hear, but approach everything with a healthy dose of skepticism. The only reason researchers do what they do is because they’re skeptical of the claims of others. No study is perfect. Think about potential flaws of studies you read, and how you might correct those flaws. It’s generally safe to assume that most of the existing stock of knowledge about a given topic is wrong. If you hear an explanation of a phenomenon (whether in the news, in blogs, or in the academic literature), think about whether it seems valid, or whether competing explanations of the phenomenon exist. There’s often an obvious explanation, and there’s often interesting work to be done on the non-obvious explanations. Once you have potential competing explanations of a phenomenon, you have a potentially interesting question to get to the bottom of. Skepticism is essential for academics and it is not sufficiently taught, so you’ll have to teach it to yourself.³

5) The literature. Other economists have written about interesting questions. They can be a source of inspiration. However, try to push your own ideas forward without going too deeply into the existing literature. If you look too carefully at how others have approached your question (or a similar question) you tend to stifle your own creativity. It’s easy to think, “Well, So-And-So is at Harvard and so there’s no way I—as a lowly UVic Ph.D. student—could have a better approach to this question.” As soon as you try to do things exactly like everyone else, your creativity—and hence your contribution—is likely to be diminished. It’s almost like putting blinders on. You may not see the best way forward with an idea if you stare too hard at someone else’s approach.

That said, journals like the *Journal of Economic Perspectives* and the *Journal of Economic Literature* often take on big questions and give sweeping surveys of work done in a given area. These articles are often relatively approachable and can serve as both background reading and inspiration for new questions you might want to pursue.

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³ Skepticism is vital, but if you start showing up to other people’s seminars saying, “This is all bullshit” as a matter of habit, you will quickly lose all friends and admirers in the field. No research is perfect, but that doesn’t mean it’s not worth listening to. You should learn to distinguish more credible research from less credible research.
**Explaining why the question is important**

People rarely admit this, but being a good academic is largely about being a good salesperson. If you can convince people that you answer important questions, they will want to listen to your ideas and read (and publish and cite) your papers. If you can’t, they’ll dismiss you as one of the 99% of academics whose research is essentially unimportant. This doesn’t mean you should oversell your research. A good salesperson chooses the best products to sell. So you want to pick good ideas and then explain clearly to your reader, your audience, a journal editor, a prospective employer, etc. why they are good ideas. How can you do this? Here are some thoughts:

1) **Present some statistics** that demonstrate that your question is addressing an important social problem. For instance, if I’m writing a paper about divorce, I might want to cite some statistics that show the social costs of divorce (on child outcomes, on government welfare expenditures or tax revenues, etc.). People might be prepared to accept that divorce is an important issue, but if I can hit them with some high-impact statistics, they’ll sit up and listen even more closely to what I have to say.

2) **Tell a story.** Anecdotes can have a lot of impact, especially if people can relate to them and if they seem like the rule, rather than the exception. Can you give examples where people seem to be behaving in a systematically irrational way? If you can, and you can demonstrate that the behaviour is widespread, then you can follow up by saying, “In answering my proposed question, I believe I can explain this behaviour.” There’s a decent chance you’ll have people on the edge of their seats at this point.

3) **Point to the existing literature.** If past authors have noted a puzzle, and you’re trying to answer the puzzle, then you can make the case that other serious academics are interested in the question you’re trying to answer. If you can make the case there’s a serious hole in the literature (e.g. a key assumption that remains untested), then that’s an opening for you to make the case that you’re onto something important. Or if people have tried hard to answer a question in the literature, and you can make the case that their analysis is flawed, that also creates an opening for your paper.

**Methodology**

Is your proposed paper theoretical? Empirical? Both? If you want to come across as versatile to prospective employers it’s a good idea to produce a thesis with both. Theory papers without empirical evidence tend to be hard to publish. Empirical papers without theory are easier to publish, but ideally you should be able to do both, and a thesis with both in it will tend to be more favorably looked upon, *ceteris paribus*. That doesn’t mean all your proposals in this seminar need to have both.
If the idea is theoretical, then think about what a basic model would look like. What objective function is being maximized? What are the tradeoffs? What are the constraints? What would the model allow you to show, in terms of comparative statics? Is the equilibrium behavior you’re describing likely to deviate from the socially optimal behaviour? Does the model allow for policy analysis? Could you simulate outcomes, in the absence of data?

If the idea is empirical, then think about what empirical model you would want to estimate. What are the key model parameters that you are estimating? Your hypotheses on the value of these parameters? What obstacles to unbiased or consistent estimation lie in your way? What would be your identification strategy? (i.e. how would you produce consistent estimates of the parameter of interest?). And, of course, what data would you use? Would you gather your own, use an existing dataset? What do you need in a dataset in order to answer your question (representativeness, sample size, variables, etc.) in a compelling way?

How to write this thing

2 pages is not a lot of room to make a complex point. So in order to write a good 2-page research proposal, you need to write concisely without sacrificing clarity.

That means while taking onboard all of the points above, you’ll only be able to address a few of them. That’s OK, because you don’t need to use every sales trick or every empirical methodology in the book to write a good paper.

Focus on generating an interesting question. Give us some context in order to motivate the question. Explain why it’s interesting, cite some work that’s been done in the area (you don’t need an exhaustive literature review for a 2-page proposal) and discuss how you might go about answering the question.

Some tips on writing:

1) Don’t waste words. Use the minimum number of words to make your point clearly. This is generally a good writing habit to have. 2 pages isn’t long, use them well. Your goal as a student, until quite recently, was probably to fill the page allotment for writing assignments. “How do I come up with 10 pages of stuff to say for this 10-page essay?” If you think really carefully about a research idea, you’ll start with (at least) 10 pages of stuff you could say, but now you need to reduce it to 2. If you find yourself struggling to fill 2 pages, you’re not doing the assignment in the spirit intended.

2) Organize your thoughts clearly. If you just sit down and write, you’ll get kind of a stream of consciousness pile of ideas that probably aren’t well organized. It’s fine to just sit down and write, but you need to go back and reorganize what you’ve written in a way that will most clearly convey your message. The responsibility for clear
transmission of your ideas to your reader lies almost entirely with you. You can assume some basic knowledge of your subfield of economics and the English language from your readers, but that’s about it. Failure by them to comprehend your ideas is your failure not theirs. So take responsibility for communicating clearly.

Think about your reader. Are you making yourself clear? Is there anything you assume your reader knows that’s unreasonable to assume they know? Read out loud. Does it sound clear? Would you be happy to read this yourself? The worst thing you can do to a good research idea is to write it up badly. If you write it badly, then even a good idea will bore your reader and look like a bad idea. The same applies to finished papers. Badly presented and badly written research is likely to go nowhere, in spite of all your great efforts.

3) Reference Stephen Hume’s book “Economics Writing”. Most of the tips that apply to undergraduate writing also apply to writing by academics. I will have him read your work at times this term. He will notice if you ignore the points he makes in his book. Another source for advice on writing is Deirdre McCloskey’s “Economical Writing” which is something of a classic in the field. http://www.deirdremccloskey.com/docs/pdf/Article_86.pdf
I don’t agree with all of her points, but I think all of her points are worth considering, and many are worth implementing. Finally, George Orwell was one of the great communicators of the 20th century. His advice on writing is worth reading and implementing: http://www.orwell.ru/library/essays/politics/english/e_pol
You will notice common threads through these sources on good writing.

4) Read my comments on your past writing assignments. Don’t make the same mistakes over and over again. Seek to actively improve your writing over the course of the term. You want your advisors to be working with you on the substance of your research. They have limited time for you—don’t make them spend it on your writing.

Finally

When you come to seminar each week, you will turn in your proposal for the week. You will also be expected to talk about your proposal with the rest of the seminar for a few minutes. In some cases, I will ask you to present your proposal formally, using slides. This is good practice for giving short talks like you will often do in your career. In other cases, I will ask you to present your idea less formally. This is good practice for talking about ideas over coffee as you will often do in your career. In both cases, you need to clearly and quickly explain your idea to an audience who may have no expertise in the area. You’ll develop oral communications skills this way. You’ll also get feedback on your ideas from your colleagues. As other students present their ideas, you should think of suggestions, criticisms, questions, and thoughts that might improve their ideas. This conversation is good for you and for the student presenting their idea. If you want to become a highly-valued colleague, you’ll master the very difficult skill of quickly wrapping your head around someone
else’s idea and pushing and prodding the idea in a way that will benefit that person as they try to develop the idea.

This idea-generation business may seem like a lot of work and a weekly distraction from that idea that you already have that you think will turn into your thesis. This may turn out to be true, but if you take these assignments seriously, at the end of this term you will have 8 or so research ideas written down. If you sensibly back up your hard drive, you can carry these ideas with you into your first real research job, where some of them may turn into papers that make your tenure case or otherwise bring career advancement. You may even get a thesis idea out of this exercise, when your current idea blows up in your face (as research ideas sadly do). You can even talk to people about these ideas during interviews on the job market (the best way to look like a research machine on the job market is to talk about lots of ideas you have that seem interesting to others). And if you get nothing else, you’ll have developed the habit of thinking up ideas. And that’s one of the best habits you can develop as an academic.