How to Develop Research Ideas: A Guide for PhD Students in Economics, Business, and the Social Sciences

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1 Introduction

This essay offers informal guidance to PhD students on how to come up with research ideas. I should preface by saying that I am an ordinary empirical microeconomist, at best. But I decided to write this because I went through the process from scratch and can offer the perspective of someone who felt the struggle and slowly grew out of it. I remember how hard it was to come up with ideas during the first year of the PhD. I was not sure what counted as a research idea, let alone a good one. Five years later, I had four chapters in my dissertation, each based on a decent research idea, if I may say so, that I came up with independently. Besides those four, I had another five or six ideas simmering in the background. At the time of writing this note, four years after graduation, I have honestly lost count of how many turn-key research ideas I have lined up. That transformation did not happen overnight, and it did not come naturally to me. It came from struggling, experimenting, observing, and learning[1].

This essay is written for PhD students in economics, business, and the social sciences. Most of what I say is drawn from my experience in empirical microeconomics. If you are in a theoretical field, or if your methods are very different, some of this may be less applicable to you. This is not a step-by-step guide, and it does not promise shortcuts. It is a collection of heuristics from someone still figuring things out.

Your first attempts to come up with research ideas will be intellectually demanding, emotionally taxing, and often demoralizing. But after a few years, ideas will begin coming to you more naturally and effortlessly, in a way that you may find enjoyable.

Here is how this essay is organized. In section 2, I tell you my journey into generating research ideas, and how I went from having no good research ideas to losing count of them. In section 3, I elaborate on my way of identifying promising research ideas. In section 4, I walk you through two examples in which I turned vague and broad ideas into specific and clear ones that turned into my first and second academic publications. Section 5 enumerates other idea-generation strategies. I have not used them myself. But some of them may resonate with you.

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[1]Learning never ends, and this note is far from perfect. I welcome all feedback and suggestions: rabbani.maysam@gmail.com, rabbanim@montclair.edu.

2   How it all began

When I started my PhD studies, I only had a vague sense of direction. I knew I was interested in the healthcare industry. I was aware that healthcare prices were high, affordability was a concern, and policy tools seemed lacking.

As I went through the standard sequence of core courses in the first year of the PhD (mathematical economics, microeconomics, macroeconomics, econometrics, etc.) I treated the lectures not just as material to learn, but as opportunities to train myself to generate ideas. I was not aiming to revolutionize every subfield I encountered. I simply wanted to get into the habit of looking at models, theorems, or empirical patterns and asking myself, "Can I improve this? Is there something here worth digging into?" Most of the time the answer was no, but that was beside the point. It was an exercise that helped me warm up to the process of thinking like a researcher.

During professors' lectures, when I did stumble upon something that resembled a research idea, I would bring it to office hours of the same professor. This probably happened three to five times per semester. I would explain the idea with great excitement, and the professor would gently let me know that my idea is garbage, but more importantly, they took the time to explain why. Sometimes the idea was too difficult and ambitious for a PhD student to accomplish. Other times, it had already been done decades ago. Sometimes, the idea was simply unimportant: even if fully executed, there was no point in wasting time on it because it was devoid of meaningful contribution.

None of those early ideas went anywhere. The same was true in my second year. But those office hour conversations were invaluable. They taught me to think critically about what makes an idea feasible and worthwhile. Gradually, I internalized that skill. The number of office hour visits declined over time, not because I had stopped seeking feedback, but because I had become better at challenging my own ideas. Increasingly, I could tell on my own when an idea was impractical or trivial.

It was not until late in my third year that I came up with a research idea that received strong support from my dissertation committee. It turned into the first chapter of my dissertation. In the fourth and fifth year, I added three more chapters to my dissertation. I defended my dissertation in 2021. Nowadays, research ideas come to me more easily and naturally. Now, the number of my turn-key ideas grows faster than I can turn them into papers. This is a good problem to have. I hope that something in this note can help you get there too.

3   Elements of a good research idea

In my early attempts to understand what makes a good research idea, I found myself overwhelmed by the number of criteria people seemed to care about. A research idea, I was told, must be novel, important, feasible, clear, identifiable, scalable, tractable, grounded in theory, relevant to contemporary debate, an incremental improvement over the existing literature, and more. The more I read, the more confusing it all sounded. So, I do not want to do that here. Instead of giving you an exhaustive checklist, I will focus on two conditions that help you sift through research ideas and identify strong ones. If your idea meets both conditions, you are in good shape. If it fails either one, you may want to discard it. The two conditions are importance and feasibility. Importance is what you should check first. But because feasibility is easier to explain, let me start with that.

A research idea is feasible if you can actually implement it. More specifically: do you have access to the right data, and do you know (or can you learn) how to apply an appropriate statistical model to that data in order to answer your research question? Sometimes the answer is obvious, for example, if you know a good dataset and model. Other times, you may need to ask your professors and peers if they know of any dataset that helps and a model that suits. This process is relatively mechanical and verifiable: you either have the data and model or you do not. If the answer is no, then the research idea is infeasible, at least for now, and you should move on to another idea.

Verifying importance means to ask yourself: is there a point to doing this research? You do not want to spend your time answering a trivial question that no one cares about. The core mental exercise here is to imagine that your research has been completed and that everything worked perfectly—your empirical strategy succeeded, and the results strongly support what you wanted to see. Then ask yourself: so what? To be more concrete, here are three versions of the "so what" question that I find especially useful:

- Version 1: what did we not know before that we now know thanks to your research? How do your findings change our understanding? Why does this matter?

- Version 2: what policy or decision should be rethought because of your findings? What mistake were scholars, policymakers, or practitioners making that your paper helps correct?

- Version 3: what theoretical contribution does your paper make? Are you calling into question an accepted model, or filling in a meaningful gap? Why does that improvement matter?

The three versions above are by no means exhaustive. You may come up with several more of your own. If you can convincingly answer at least one version of this question, then your research idea is probably important enough to pursue. If not, then you may want to move on. This approach is a heuristic tool which is not grounded in theory. But I am sharing it because it served me well.

4   Examples

To clarify the process of turning a vague idea into a refined one, below, I walk through the steps I took that eventually turned two crude ideas into two academic papers. The goal is to show how your diamond-in-the-rough of a research idea could be refined into its full potential.

4.1   Example 1: I just wanted to do a hospital merger analysis

It was late in the third year of PhD, and the pressure was building. I had only a few months left to come up with a research idea and turn it into my dissertation proposal. Around that time, I had just received access to a rich, claims-level dataset that was highly granular and ideal for hospital merger analysis. The dataset covered the years 2010 to 2012. All I needed was to find a merger that occurred in 2011, so I would have enough pre- and post-merger observations to study the effects.

I began scanning hospital mergers, reading the news, and spending anywhere from a few minutes to twenty minutes per case, trying to get a rough idea of the size and potential

significance of each merger. Out of several mergers that caught my attention, one of them became increasingly interesting the more I read: the merger between ProMedica and St. Luke's Hospital in Toledo, Ohio. It had generated considerable local controversy. Public concern centered around the fear that this merger would increase the cost of giving birth in Toledo. The local market for inpatient birth services was small, with only three hospitals, and the merger reduced it to two.

That alone seemed like a solid basis for a PhD proposal. But something else about the case drew me in: it was a merger between two nonprofit hospitals. As I dug deeper into court documents related to nonprofit hospital mergers, I noticed a pattern: courts often dismissed antitrust concerns simply because the merging hospitals were nonprofit. They presumed that nonprofit status inherently nullifies anticompetitive behavior. This crystalized my research question: could a purely nonprofit hospital merger lead to higher prices, and could this hinder healthcare utilization? I implemented a straightforward difference-in-differences strategy using my dataset. The results were unambiguous: after the merger, the cost of inpatient obstetric services rose sharply, while utilization declined significantly. This implied that the merger inflated prices, pushing price-sensitive patients into alternative birth settings, which could be unsafe for mothers or babies.

This idea checked the boxes of feasibility and importance. It was feasible because I had the data and empirical model. As for importance, my answer to the "so what?" question would be: My findings showed that nonprofit hospitals seem to respond to financial incentives like for-profit hospitals. This calls into question the widespread practice of granting legal leniency to nonprofit entities during merger review. If nonprofit status does not restrain opportunistic behavior, then perhaps we should stop affording nonprofits extra leniency, and assess mergers regardless of the nonprofit or for-profit status.

This turned into my first paper [Rabbani, 2021]. As you can tell by now, the research question started vague, broad, and bland. But it gradually became specific and interesting. I did not know beforehand what to expect. I just kept my eyes open for new angles and let the data, empirics, news, and the process lead me in new directions.

4.2   Example 2: looking for a pharmaceutical merger

This idea turned into the second chapter of my dissertation and was eventually published in a better outlet [Rabbani, 2023]. After a dissertation chapter on a hospital merger, I wanted to have a chapter on a pharmaceutical merger. I began scanning major pharmaceutical mergers around 2011, looking for large and high-profile deals. I landed on Gilead Sciences' acquisition of Pharmasset. With an $11.2 billion transaction, it was one of the most expensive pharmaceutical mergers ever.

At first glance, the price tag was puzzling. Pharmasset had no products on the market and no revenue, and it made little sense that the world's largest antiviral firm (Gilead) spent over a third of its entire assets to acquire it. But the logic slowly emerged. Pharmasset was a clinical-stage company with three drugs in Phase II trials. One of them (Sovaldi) was a promising Hepatitis C (HCV) treatment, projected to enter the market within two years. If successful, it would become the first viable cure for HCV and turn into a blockbuster. The other two investigational drugs targeted HIV/AIDS (HIV) and Hepatitis B (HBV), and both were considered superior to incumbent treatments.

As the largest antiviral manufacturer, Gilead owned more than half the HIV/HBV market, but they had no HCV treatment in the pipeline. Meanwhile, their competitors were making headway in HCV treatment. In this context, Pharmasset was a threat and an

opportunity: it could unseat Gilead's HIV/HBV dominance, and it could be the key to a blockbuster HCV treatment. From Gilead's perspective, acquiring Pharmasset made strategic sense: spend heavily today to neutralize a future rival and preserve long-run dominance. Gilead was not trying to reshape current competition; it was preemptively neutralizing future competition. This was the key insight: the merger undermined future competition without altering current market shares.

Antitrust law lacks this foresight. The Hart–Scott–Rodino Act requires large mergers to file with the Federal Trade Commission (FTC) or Department of Justice (DOJ) for antitrust review. With a price tag nearly 100 times the reporting threshold, this merger should have drawn scrutiny. But it didn't. There was no investigation, no challenge, and no litigation. Why? Because Pharmasset had a market share of zero at the time of the merger. Under current antitrust doctrine, any quantitative evidence of harm must be based on changes in market concentration now, using the Herfindahl-Hirschman Index (HHI). Since acquiring a firm with zero market share leaves the HHI intact, the FTC or DOJ had no legal footing to block the merger.

This created a regulatory blind spot. Firms like Gilead could use forward-looking, strategic logic to acquire nascent rivals. But antitrust law is handicapped to seeing current or past evidence. The result is a systematic loophole: incumbent firms can maintain market dominance indefinitely, as long as they are able to identify and acquire promising future rivals before they become big enough to trigger a measurable HHI increase.

At this point, I knew that likely I had a paper at hand that satisfied feasibility and importance. Feasibility was straightforward. I had the data, and a simple diff-in-diff would be the model. As for importance, the "so what?" question was easy to satisfy: I was pointing to an antitrust loophole that allowed markets to remain artificially concentrated. If antitrust enforcement is blind to credible threats of future competition, then we need to rethink how merger analysis is done. My findings raised the need for expanding the evidentiary standards in antitrust law to allow credible forward-looking evidence when assessing the competitive effects of mergers.

5 Other ways to find ideas

I discussed my way of coming up with research ideas. But there are many other ways. Below I list some alternatives. I have not personally used them, so I cannot offer much guidance on them. But some of them may resonate with you:

- Watch for regulatory changes, court rulings, or natural experiments. They often generate variation that is ripe for causal analysis.

- Talk to policymakers, experts, and people in the industry. Learn about their priorities, challenges, and unanswered questions. They often face puzzles or inefficiencies that academia is yet to address.

- Skim recent working papers from places like NBER, CEPR, or SSRN. It gives you a sense of what prominent people in the field are interested in, what the ongoing debate is about, and what avenues of future research attract academic interest.

- Use existing papers. Some published papers enumerate several avenues for future work. Others might have answered an important question using a weak or inconclusive methodology that you can improve upon and make a more compelling case for.

Alternatively, you may improve upon the assumptions and model, if that is in your toolkit.

- Follow academic Twitter or blogs. If the public is hotly debating a topic in real time, it may dictate the broad direction of your next paper.

- Attend conferences in your field. It familiarizes you with ongoing research in and around your field. On a single day, you can sit in a dozen presentations and scan many more posters. This is a great way to get a sense of what topics and issues matter the most.

- Go to seminars outside your core field. The language or tools might differ, but exposure to other disciplines can spark fresh angles or lead to interdisciplinary work.

6 Acknowledgements

References

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