

Framing a Research Project: Five Questions to Think About*

Sanford C. Gordon
Wilf Family Department of Politics
New York University

Spring 2022

I find it useful to always keep five questions in mind when thinking about a research project. When *commencing* a project, they help me and clarify my research aims and determine whether a project is worth investing time in. When *conducting* research, they help remind me why I'm doing what I'm doing at that particular moment, or help clarify whether I should or shouldn't be spending my time on that particular task. When *communicating* about the research product – discussing it with colleagues, writing up the results, presenting, and seeking to publish it – they can be indispensable in keeping me from missing the forest for the trees, and be a set of guideposts around which to structure how I frame the project for an audience.

By the same token, if I find myself at any point unable to provide a compelling answer – for myself or someone else – to any one of these questions, it's a good sign that something is amiss and that I should take a step back and think about what I'm doing and why, and enlisting trusted colleagues to help with this.

Because most of the research I do is empirical, my descriptions below correspond most closely to how I think about empirical projects. But rest assured that there are analogous things you should be asking yourself if you are writing a theory paper. In a future draft I'll try to flesh out those analogs.

Question 1. What is the research question itself? What is the overall question for which your research will endeavor to provide the answer? Most of you will be conducting research with a strong quantitative bent; the way you frame your research question will have important implications for the design and conduct of the research itself. A question that is too narrow, or that admits only a yes or no answer (e.g., “Does priming partisanship in a convenience sample of NYU undergraduates increase stated vote intention?”) places limits on what we can hope to learn from the research. At the opposite extreme, overly broad questions (e.g., “What causes civil wars?”) are impossible to answer in the context of a single research paper; will lead to an unfocused, scattershot approach; and encourage the worst tendencies among social scientists (p-hacking, the file drawer problem, confirmation bias, post-hoc hypothesizing, etc.).

In contemporary social science, most quantitative research concerns “effects of causes” questions:

*Draft – not for circulation.

what is the effect of X on Y, where X (the treatment) and Y (the outcome) are specified in advance. The modern insistence on a strong causal identification strategy as a necessary condition for compelling empirical research is, to a great extent, premised on the effects of causes emphasis.

Insofar as we maintain this emphasis, a richer project will often be framed around a question such as, “To what extent, and under what conditions, does X affect Y?” Framing effects-of-causes questions in this way admits a wider variety of interesting answers. If conditional or heterogeneous effects are strongly connected to theory rather than introduced in an ad hoc or impressionistic way, they present an opportunity to expand the scope of the inquiry (and thus the scope of learning) in a disciplined (and thus more compelling) way.

Social scientists are also interested in “causes of effects” questions: why (or via what mechanism) does phenomenon Y occur? When addressing causes-of-effects questions, it is critical to posit a specific mechanism that you hypothesize is responsible for generating the outcome of interest. It is also generally critical to have a set of “privileged” alternative mechanisms in mind that could also be responsible for the observed result. While it is generally not possible to directly test a mechanism, it is often the case that well-defined mechanisms imply a variety of different empirical implications for which there may be indirect tests. Sometimes, different mechanisms will produce the same empirical implications; other times, their predictions will differ. These latter instances create opportunities to adjudicate among competing mechanisms and bolster our confidence that one (or the other) is in operation.

Question 2. Why do we care? Why is the question (and its answer) important enough for me to spend a significant portion of my life working on it? Note that while “importance” is an inherently subjective concept, there are reasons that prospective audiences will find more and less compelling as you try to convince them that they should be engaging with your work. Fortunately, most humans find politics an inherently interesting subject, which makes our jobs a bit easier. Here are some “hooks” that you might think about when asking why we care about some phenomenon:

- The phenomenon under study is an important part of our contemporary political world (i.e., it affects a large number of people)
- The answer to the question I’m addressing touches on normative considerations that we care about (e.g., inequality, democratic accountability, liberty vs. coercion, etc.)
- There is an unresolved yet pressing issue in previous research that prevents a more comprehensive understanding of the phenomenon in question
- There is some political behavior or institution that, at first blush, seems puzzling, incoherent, or irrational (and my magic trick will be to show you that what you thought was irrational actually makes perfect sense)

Note that *by itself*, “there is a gap in the literature” isn’t a particularly compelling argument about a topic’s importance. There are innumerable gaps in innumerable literatures, but not all are worth devoting attention to. That being said, one of the above hooks may be harnessed to a gap in the literature in a fruitful way: for example, previous research has failed to provide a compelling or rigorous account of the phenomenon understudy, and I am going to fill that gap.

This brings me to an important point: the role played by previous literature in the framing and conduct of your research. Needless to say, it is essential that you be familiar with previous research on a subject so that you (a) know something about what you are studying and (b) can credibly claim the originality of your research and don't inadvertently reinvent the wheel.

Beyond that, what is the purpose of a literature review in academic writing? Many students get bogged down endeavoring to summarize everything that has been written on a particular topic in a largely unreadable section titled "Literature Review." This practice misses the point. In effect, a review of previous research in your area of study should serve three functions:

First (and expanding on the above), a comprehensive review of previous research in your area of study is a costly signal to your audience that you have some idea of what you're talking about, and, further, that your claims of originality are authentic. In a similar vein, it is a signal of professional respect: that you have thoroughly engaged with previous arguments and evidence on the topic. Perfunctory engagement with the literature conveys a lack of seriousness to the reader. Note that this does not mean that you must engage with everything that has been written on a particular topic.¹ But ultimately, a review of the research should draw a distinction between what you are doing and what previous authors have done. In this regard, a thorough review of the literature signals that the gap you identify is, in fact, an actual gap, and not one that exists simply because you didn't read sufficiently deeply.

Second, by framing your research as contributing to a particular literature (or literatures), you are telling other participants in that research program that they really ought to engage with your research, and explaining why that is.

Finally, a review of the research can serve to point out (respectfully, of course), flaws in existing studies. Perhaps you are answering a question that others have sought to identify, but doing so in a more methodologically sophisticated way, or with better data. This brings me to the third question:

Question 3. Why is it hard? Everyone has heard the old joke about the economist who refuses to grab a twenty dollar bill lying on the ground because, per the efficient market hypothesis, someone should have already picked it up. A natural question a reader might ask about your research is the following: if this question is as important as you claim, why has no one done the research you're now doing? The answer is that while the question may be interesting, it's also hard. You, the author, are arbitraging your knowledge, skills, and research acumen to do what couldn't previously be done. Some common answers to the "Why is it hard" question:

- No one has collected good data (but I have!)
- There's an inherent selection bias problem (that I've solved!)
- Causal identification is hard to come by because the explanatory variable I care about is never randomly assigned (but look at my amazing experiment/natural experiment!)

¹That being said, it is frequently the case that reviewers will ask you to cite a piece of research that is at most tangential to the research you're conducting. Wait for them to suggest it before including it.

Question 4. What will I do (or later, what did I do)? This question concerns research design: what is the research target – i.e., the ultimate subject of the inquiry? Does the sample you are using approximate the research target? What is the empirical strategy you are employing to the research target to gain purchase on answering your question? What is your strategy for inference? Are you making a causal claim or a descriptive one? If the former, what are the identifying assumptions? If the latter, how do the predictions perform out of sample? And does evidence in favor of a descriptive claim speak to/inform us about a specific causal mechanism? What measures are you using? Are their potential issues of construct validity in those measurements? If you are running an experiment, what is the ecological validity of that experiment? Are the quantities represented in the data commensurate with theoretical quantities of interest?

As a matter of professional advancement, potential employers generally favor hiring candidates who are interesting and smart. Questions 1 and 2 speak to the interesting dimension; Questions 3 and 4 tell them something about the smart dimension.

Question 5. What can I hope to learn (or later, what did I learn)? Most academics are prone to a very specific form of anxiety of the following sort: what will happen if my research doesn't work out the way I expect? I think there's a more basic question that a researcher should ask before embarking on a new project – one that far too few of us have the intellectual courage to face, but that a failure to face will inevitably create problems down the line:

Suppose I conduct my research, and everything goes *exactly as I had hoped*. What will I know after conducting the research that I didn't know before I started?

You want to conduct research on questions where the prior beliefs “out there” are relatively diffuse, or where you have strong reason to believe that the consensus on a particular topic may be wrong. This is not a call toward contrarianism or favoring counterintuitive findings for their own sake; rather, it is an acknowledgement that our time is finite and so we may as well try to apply our efforts toward questions where we have the most to learn.

A related and final point that I can't stress enough: we all want to work on problems that we are passionate about, as this encourages us to keep striving in the face of adversity and setbacks. But passion is a double-edged sword if we have a stake not only in *answering the research question* but in *a specific answer to the research question*. A personal or ideological stake in a particular research finding creates terrible incentives to engage in dubious research practices. If you anticipate being disappointed if your study might yield a result that goes against your own ideological biases, seriously consider working on a different topic.