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From Research Topic to Research Question

The world is a systematic place. It's full of patterns that make sense, patterns that we can discover and explain and use to predict things. Most of us accept this statement as a matter of course in the natural world. If we put a frying pan on a gas stove and light the burner, heat will transfer from the burner's flame to the pan, in proportion to the size of and duration of exposure to the heat source, and then into any food we've placed in the pan. This happens on any day of the week, at any time of the day or year, under any weather circumstances, and for any person who performs this task. We can predict that if we increase the size of the flame or the duration of the pan's exposure to the flame, the pan will become hotter and the food will cook faster.

The social world—and the subset of it that we know as the political world—is also a systematic place with patterns and predictability. Most people greet this statement with at least a little bit of skepticism, if not outright incredulity. We've all heard some casual observer of American politics grumble about how "nothing that happens in Washington makes any sense." Some politicians vote one way, others vote another way; some of them don't vote the way we expected. This one causes a scandal, that one makes a horrible public gaffe, another one inexplicably loses a primary election. From day to day, there's no telling what bit of nonsense is going to emerge next, at least according to that casual grumbler, and on the surface of things, that grumble seems to hold a lot of truth.

If your source of that grumble is anything like mine, though, it's older, sounds awfully like one of your grandparents, and is usually followed by something like "anymore" and a reference to "back in my day." That statement—far from asserting the unpredictability of politics—is actually a profound claim in support of the predictability of the political world. Your grumbler holds beliefs and expectations formed over a long period of time and many observations: which way legislators should vote, based on various characteristics; how they should behave (i.e., what constitutes a gaffe or scandal); that incumbents are usually reelected. The fact that we *can* form expectations and make predictions about political actions or outcomes suggests that patterns do exist and that, subconsciously at least, we recognize those

patterns. Humans are by nature pattern seekers; patterns help us make sense of the world around us. The patterns we find in the social world are not usually as strong or consistent as those in the natural world—certainly not as strong as the first and second laws of thermodynamics, which govern our frying pan example—but they definitely exist, and with a little bit of digging, we can find them.[1]

As a social science, the field of political science is committed to discovering and explaining these patterns, in the domestic politics of both the United States and other countries, and in politics between and across countries. Like our fellow social sciences, economics, social psychology, sociology, and anthropology, we are committed to making sense out of the world that we observe—the *empirical* world—by seeking patterns and explanations for general phenomena as well as for specific cases. Unlike the natural sciences, our patterns are generally contingent on other circumstances. Patterns of legislator behavior, for example, usually differ by country, though we can definitely find other patterns that extend across countries as well. Part of the challenge, and so part of the fascination and interestingness, of the social sciences lies in figuring out what those contingencies and mitigating circumstances are and in determining just how broadly some of our explanations and patterns stretch. This requires looking at many cases and many contexts; one observation does not make a pattern.

Why Do Politicians Dislike Political Science?

Your challenge, if you're reading this book, is to join social scientists in our effort to make sense of the social or political world. You've been assigned a paper that asks you to identify a puzzle or pattern in the political world, to craft an explanation for that puzzle or pattern, and then to test that explanation against the evidence. In short, your goal here is to discover new knowledge: to figure out something that we as a society collectively didn't know before. It's a bit of a daunting idea, but at the end of the course, you'll know something that no one knew before. A little intimidating, yes, but it's also intriguing and enticing and fascinating and a bit exciting.

In this chapter, we'll briefly discuss political science as a social science and what the social sciences like in a "good" theory. We then examine how the paper you're being asked to write for this class differs from other types of papers you may have written before. We'll look at research topics, research questions, and characteristics of good—meaning feasible for an empirical paper like the one you've been assigned—research questions. We end with a discussion of how to create your own research questions, including sources for ideas and ways to phrase your question. You'll even have a chance to practice your skills and prepare for your own paper with the chapter activities. We conclude with a list of terms introduced in this chapter.

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[1]Most modern social scientists reject the idea that we can find laws in the social world that are as strong as those in the natural world. The idea of scientific laws is most prominently associated with Carl Hempel's famous (1966) book, *Philosophy of Natural Science*.

DOING SOCIAL SCIENCE

Social science values several things in theory and research. Four characteristics of theory that we particularly value are parsimony, predictiveness, falsifiability, and fertility. **Parsimony** means that the theory explains more using less. Measuring concepts is very difficult, and so the fewer variables or pieces of information that we need to use, the smaller our chances of getting findings that are actually caused by our errors. Because of this, we deem a theory using fewer pieces of information the "better" theory, and we reject ones that require more but produce the same results. **Predictiveness** means that the theory can help us to understand cases beyond those from which we derived it. Understanding what *has* occurred is useful, but the most useful theory also helps us to predict things that hadn't yet happened at the time the theory was created, or that weren't part of the theorizer's original dataset.

We also value **falsifiability**. If our ultimate goal is to understand how the world works, then we need to be able to reject and discard theories that do not explain the world well. To do this, we need to be able to identify what evidence would convince us that the theory is *in*correct, even if we have not actually observed that evidence. If we cannot logically identify types of evidence that are inconsistent with a particular theory, we will never be able to determine if the theory is indeed valid. Finally, we value **fertility** in a theory. Theories that suggest other observable implications or other novel hypotheses are valuable because they prompt us to do further research and to build a cluster of knowledge around that theory and research question. In this way, our knowledge cumulates—builds on itself—rather than remaining as isolated relationships and discoveries. This cumulation helps us move forward as a field rather than persist in reinventing the wheel.[2]

We share with the natural sciences the value of **replicability** of research. We believe that science (the development of new knowledge) should proceed in a public manner, with data and analyses freely available to others, and with the sources of our conclusions clearly explained. Because of this, we place great emphasis on explaining in our research reports exactly how we did things: what we measured and how we measured it, the sources we used for those data, what specific analytical techniques and software we used, why we made certain choices and not others, etc. Ideally, another researcher should be able to get that same data himself or herself and reproduce your results.

RESEARCH QUESTIONS AND THIS COURSE'S RESEARCH PROJECT

Research in political science can take many forms, but four types of research questions are particularly prominent. **Normative** questions ask how things

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[2]This is part of the reason why we review the literature on our question in each research project we write.

should be, or what policies are best. The key feature of a normative question is some element of evaluation against subjective criteria: the "should" or "better" component. What is the best way to address climate change? Well, that depends entirely on your (subjective) definition of "best." Does it mean mitigating the overall cost of management? Does it mean spreading the cost of action equitably—or perhaps proportionately—across countries? **Hypothetical** (or theoretical) questions address issues of what if, or what might be. How might a resurgence of Zapatista guerrillas in southern Mexico affect the Mexican government's ability to wage war against the drug cartels that dominate the north? This is an interesting question; it requires the researcher to find out how the Mexican government is dealing with the drug cartels now, and then to project what would occur if the Zapatista rebellion were to reassert itself. This might never happen in real life, but it is certainly worth thinking about.

Factual/procedural questions, on the other hand, ask questions about the facts of the world. What is the United Nations' policy on population control measures? How does the federal government regulate nuclear power stations? The answers to these questions are objectively verifiable facts of the kind that one might be able to find on Wikipedia or by asking a knowledgeable person. **Empirical**[3] questions, finally, ask about how the world *is,* or how the world *does* work. Their concern is with actual events and phenomena, ones that have occurred or are occurring; they care about the whole of a type of event or phenomenon, rather than about specific instances. Why and how does the entry of a third-party candidate into a Congressional race cause either candidate to change his or her campaign strategy? Why do democracies never fight each other, even though they fight wars overall just as frequently as any other type of state?

Normative, hypothetical, factual, and empirical questions are all valid forms of research, and they serve very important purposes in creating our overall understanding of how the world works. Procedural research often forms the basis for identifying empirical puzzles, and it can provide some of the raw data on which more systematic and generalized empirical research relies. General empirical explanations are necessary for reaching hypothetical conclusions; without understanding the usual effects of certain variables or factors, we cannot reach reasonable conclusions about the effects of changing those variables' values in specific cases.[4] Understanding the possible things that might happen—the hypotheticals—gives us grounds to evaluate our potential futures from a normative position. Those normative positions, then, raise other questions about whether the world actually is the way we want it to be (factual or empirical), and how we might get it there (hypothetical).

This course's research project asks you to conduct empirical research. Empirical research typically seeks a general story or explanation, one that

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[3]The term *empirical* simply means that it is guided by scientific evidence and/or experiment, that it uses real-world evidence in examining its claims.

[4]Most hypothetical research does this intuitively, on the basis of deep background knowledge of the case and/or others like it, rather than relying on explicit empirical research.

applies across cases and across time. It creates new knowledge about the way the world actually works. This is different than many other research papers you've written before for other classes. For those, you usually had a *thesis,* a central argument around which you marshaled evidence. For this paper, on the other hand, you'll have a *hypothesis,* a statement of the relationship that you expect to exist. In your other papers, you definitely had evidence to support your thesis—or at least you did if you wanted to get an A! You might have discussed evidence that didn't support your argument, but you did so mostly in an effort to show that it wasn't very damaging to your claim. In empirical research, you'll have evidence, but it will take different forms. You may very well find that your evidence does *not* support your hypothesis, and that's totally okay. A finding of "no relationship" is important, especially if theory expected that we would find a relationship there. We don't go into an empirical research project already knowing the answer to the question. We enter the project *to* answer the question, and sometimes the answer is not what we expected.

What constitutes evidence is different in this kind of paper as well. In thesis-based papers, you could cite another author's argument as evidence for your own, and you could get all of your evidence from other published works. In fact, you were usually encouraged to do this. Good thesis-based papers took existing arguments and evidence and marshaled them in an innovative manner, or introduced evidence that previously hadn't been associated with this argument. In empirical research, on the other hand, you are *contributing* to our knowledge, not simply reanalyzing others' information or collecting it in a new format or structure or for a new purpose. We do this by collecting raw data (information) and analyzing them using some highly specified, rather rigid techniques. The use of specified, rigid analysis techniques makes our research—our transformation of data into findings—replicable by other scholars. These techniques produce certain kinds of conclusions that emerge entirely from the data. The conclusions are not statements of opinion, nor are they particularly influenced by our own personal opinions. This makes the conclusions more credible to others. Even if others disagree with our opinions, if they agree that we collected the appropriate data and analyzed them correctly, then they must accept the conclusions that follow.[5]

FROM RESEARCH TOPIC TO RESEARCH QUESTION

Most people begin a paper with a **research topic**—some single noun or expression that interests you. Your current answer to the question "So, what are you interested in?" is almost certainly a research topic: the Middle East,

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[5]In their classic text *Designing Social Inquiry*, King, Keohane, and Verba describe the situation quite bluntly: "To put it most directly but quite indelicately, no one cares what we think—the scholarly community only cares what we can demonstrate" (1994, 15). Using collectively accepted empirical methods and reliable, public data makes our conclusions more convincing to others, even if they disagree with us. This is why knowledge of—and adherence to—professional norms of research design and analysis is so important.

Congressional midterm elections, microcredit, arms control, state education policy, climate change, AIDS, the European Union, etc. The list is literally endless.

Research topics, however, are not the most helpful things in helping you to shape a specific research paper. They lack any direction or any guidance for what to do: They simply state a topic. They don't focus our attention onto any part of it, or help us to find an entry point into the material. Imagine that some (mildly sadistic and probably somewhat inebriated) relative asked you about your interests, and then challenged you by saying, "Oh yeah? Tell me everything you know about [insert your interest here]." Where would you start? What are the main parts? What is interesting about it? Simply having a topic of interest does not give us any help to answer that [insert your choice of adjective here] relative's question.

A **research question** differs crucially from a research topic in that its question format provides guidance. It defines a limited scope or boundary for the topic; it tells us what constitutes an end to the answer to your relative's question. By being open-ended, though, it gives us guidance for what to do next: Answer it. In other words, it allows us to bite off a section of the topic that's big enough to chew and swallow. It directs our attention to a manageable, defined, bounded problem and then tells us what constitutes a complete response.

Crafting a Research Question

Crafting a good research question for your research project is a bit like Goldilocks looking for a bed in the home of the three bears. You need a question that is not too broad and not too narrow. A "just right" question has an answer that takes about as much space to answer as you have for your paper.[6] The only way to get good at generating research questions, unfortunately, is to practice. We have no magic formula for creating a good research question. Below I present a list of some basic principles to help you formulate your own research questions.

Good questions . . .

- *Usually draw on background knowledge.* Start with something you know. Think about what you've studied in other classes that interested you or that made you wonder. What made you stick around after class to ask the professor more questions?
- *Often identify or begin from empirical puzzles or anomalous outcomes.* What things make you go "hmm"? Have you ever noticed two very similar initial situations that end up with radically different outcomes? Have

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[6]In graduate school, this gets turned on its head: A paper is as long as you need to answer your question properly. Faculty will usually give some expectations to help you establish an appropriately bounded research question, but answering your question completely and well is usually more important.

you ever noticed something that just didn't seem to make sense, or seen behaviors that don't seem to be achieving the actors' goals but they keep doing them anyway? These types of things usually make really good research questions. Your background knowledge can be useful here in helping you identify them.

- *Often use "reporter questions": who, what, when, where, why, how.* Good empirical questions are actual questions—they end with a question mark and express some uncertainty—that go beyond asking about basic facts. They ask things such as, "Under what conditions?" "How much?" and "What relationship?" As we discussed above, questions are better than statements because they provide both direction and boundaries.
- *May link to or draw on theories from the field.* The field of political science is littered with theories that you can use as inspiration for your own arguments: pluralism, federalism, corporatism, rational choice, constructivism, judicial activism—again, the list is nearly endless.[7] Much empirical research has ties to questions raised by these schools of thought, but not all. It is possible to *theorize* without relying on a major theory. Theorizing is simply imagining a logically plausible connection or path between a cause and an effect, and an *-ism* is not essential to this. -Isms can suggest potential paths and potential causes, but so can your imagination.[8]
- *Are usually about the outcomes rather than about the causes.* A question that is narrow enough for a single paper is usually about explaining some outcome ("Y") rather than about some independent variable or cause ("X"). Answering the question "What causes Y?" is a more specific and focused task than trying to answer the question "What are the effects of X?" "What are the effects of X" is an incredibly broad question—the list of potential dependent variables (outcomes) is again infinite.[9]

Good questions usually do not . . .

- *Have one-sentence or factual answers.* Unfortunately, this eliminates many *who, what,* and *when* questions. Good empirical research questions are not ones where you could get the answer from a publicly available source like Wikipedia, or by asking one knowledgeable person.

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[7]International relations (IR) as a field is unlike almost all other fields of social science, including comparative politics, in that it has Grand Theories that purport to explain all behaviors that we observe. In IR, these are realism, liberalism, and, to a certain extent, constructivism.

[8]We'll explore theory and theorizing in Chapter 2.

[9]An even better research question than "What causes Y?" is "How does X affect Y?" Notice how the second question narrows the scope of the paper to a focus on one particular independent (X) variable.

- *Contain a lot of proper nouns.* Proper nouns, by definition, refer to specific cases, events, things, or people.[10] The point of empirical research is usually to look for general causes or patterns across many cases, and using proper nouns limits the set of cases. For example, the question "Why did the United States invade Iraq?" is too narrow for good empirical research. A better question might ask, "Why do states initiate wars?" Notice how the proper nouns became common ones: I determined what the specific case was an instance of—here, of a state initiating a war—and then asked my question about that. My new question's answers will explain not only why the United States attacked Iraq, but also why other states have started other wars.

- *Have a single correct answer.* A good empirical research question is one where reasonable, educated people could disagree about the answer, at least before the analysis occurs. How does economic development affect the environment? One answer might be that development harms the natural environment because industrialization usually involves pollution, and economic growth usually involves increased numbers of personal automobiles (with their accompanying emissions) and increased consumption. Another answer might be that development is good for the environment, because as people's basic needs are increasingly met—as their personal economic situation improves—they have time and energy and money to address things like the environment. Until we actually do the research, we have no way to know which of those equally plausible answers is actually correct. It's also entirely possible that *neither* of them is correct, or that *both* are correct.

- *Focus on specific measures or indicators of a concept.* Good empirical questions have theory behind them, and theories are about concepts. "How does birth rate affect GDP?" is about specific measures of concepts. What the question is really asking is, "How does maternal health [or women's status or something similar] affect development?" We prefer research questions about concepts because these types of questions allow us to generate additional observable implications—in other words, they're more fertile (no pun intended) than questions about measures. If the real underlying research question here relates maternal health to development, then we should expect birth rate to predict other measures of development, and we should also expect other maternal health indicators such as fertility rate, access to contraception, female HIV/AIDS rate (and mother-to-child HIV/AIDS transmission rates), etc., to also predict various measures of development.

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[10]In English, a proper noun is a word that begins with a capital letter—for example, English, Greek, and Latin are examples of specific languages.

For your research paper for this class, you should think broadly and explore your interests. Think creatively, but also think—at least a little—in terms of doableness. Data on the effects of fair trade on individual rural Nicaraguan villages, on rural school boards' attitudes toward federal educational reform efforts, or on the safe-sex practices of male sex workers in India are not generally going to be available to you from your current location. They're great ideas for your bachelor's or master's thesis or dissertation, though, or any other project where you've got a longer time period and access to funding opportunities.

☞ **Peer Pointer**

> "One of the best pieces of advice I received from my adviser was that the best research comes from the best research questions. The part of the process that I expected to move most quickly—that is, creating a specific, meaningful research question that addressed a gap in the literature—proved to be the most challenging and probably the most important step."
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> —Elizabeth M., University of Michigan

FINDING AND REFINING A RESEARCH QUESTION

Most people—experienced and beginning researchers alike—complain that finding a good research question is often the hardest part of the research process. Getting a research question alone is not particularly hard; the (more) difficult part is narrowing or broadening it as necessary to fit within the available time or paper space. The biggest piece of advice that I can provide for finding and refining a research question is that good questions are the result of lots of brainstorming and multiple rounds of revision and feedback. Don't plan on using the first one that pops into your mind. Take the time to generate several options. Keep your need for a research question in the back of your head, and jot down ideas as they occur to you over the course of a week or so—when you're reading, when you're in other classes, when you hear something intriguing on the news, etc. The more potential starting points you have, the better your chances will be to find a question that will work.

Where Do Research Questions Come From?

As I suggested above, research questions come from many places. The most common place to "find" a research question is in something else that you read—for another paper, for another class, or even that you read in the news. Most published (empirical) papers contain at least a brief review of the literature, where the author briefly summarizes existing knowledge about a question

(or family of related questions) and places his or her own question into that scholarly context. In doing so, the author usually highlights several gaps or weaknesses in the literature. These "holes" are great places for other authors to situate their own work; bridging gaps between related ideas or findings is a very important contribution to the scholarly enterprise and to cumulating knowledge in a particular research program.[11] So, a first place to look is in the literature review of an (empirical) article that really intrigued you. Sometimes the authors will identify these areas for future research explicitly, often in the conclusion of the article.

A second and related place to look is in literature reviews. Some journals publish stand-alone article-length literature reviews that explore and elucidate the range of our knowledge on a particular research program. Two journals, the *Annual Review of Political Science* (ARPS) and *International Studies Review* (ISR), publish exclusively literature reviews (and in the case of ISR, book reviews as well).[12] The *Oxford Handbook of Political Science* is a 10-volume collection of stand-alone literature reviews that compensate for infrequent updating by being incredibly comprehensive in their coverage. As *Reading and Understanding Political Science* (Powner 2007) discusses, literature review articles impose order (or at least try to, anyway) on the assorted literature on a question and illuminate areas of that research program where theory and/or empirics are particularly weak. The purpose of these articles, then, is twofold: to organize existing knowledge and to indicate where we need additional knowledge to enhance our understanding. An article-length literature review will almost certainly include calls for further research on particular unilluminated corners of the research program. Sometimes these are embedded in the body of the article; other times, they're in a separate section at the end of the piece.

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[11]A research program is a set of interrelated research questions focusing on a fairly well-defined topic. One popular research program, to which many scholars have contributed, addresses the "democratic peace"—the empirical observation that no two democracies have ever fought a war with each other. Some scholars have questioned (or sought to demonstrate) that such a peace even exists. Others then tried to establish correlates or causes for the observed relationship (or nonrelationship, depending on your position in the debate). Yet another group of scholars has worked to arbitrate between the competing (and sometimes incompatible) sets of explanations to see which family of mechanisms (normative, cultural, leader-incentive-based, institutional, etc.) had more support. The list can go on, but these are some of the most common research questions associated with the democratic peace research program.

[12]Other journals, especially *World Politics* (WP) and *International Organization* (IO)*,* increasingly publish stand-alone literature reviews. Much like the reviews in ARPS, these pieces are solicited: The editors explicitly ask top scholars in the field to write them, so that the review reflects, as much as possible, the cutting edge and/or state of the art in that research program. ISR publishes a much broader set of authors, and while the journal's peer review process provides some measure of quality control, the standards of IO, ARPS, and WP are significantly higher than those of ISR. Unfortunately, these types of article-length reviews are relatively less common outside of journals of international affairs; ARPS appears to be the only source that regularly publishes them.

Sometimes students feel that taking a research question idea from another article is somehow "cheating," or even "stealing" someone else's idea; they feel that using a question suggested by someone else is somehow being intellectually dishonest. Others feel that piggy-backing on someone else's research question is "not making a contribution," or is not making "enough" of a contribution to be worth their time and effort. Neither of these is even remotely true. If you are concerned about the ethics of using a research question first suggested somewhere else, simply cite the source in your work, and then include the source in the references as you would any other cited piece. Not having "invented" the idea itself does not undermine your work or otherwise decrease its value, nor is it academically dishonest.

Talking Tips

To attribute a research question to another author or source, you might say something like this:

- "This paper responds to Smith and Jones's (2001) call for further investigation of the relationship between chickens and eggs."
- "As Smith and Jones (2001, 24) note, the absence of research on the chicken-egg relationship constitutes a significant gap in our understanding of this phenomenon. This paper addresses this gap by . . ."

If your concern is about the potential magnitude of the contribution, your concern is misplaced. Most pieces of research are not earth-shattering; the earth-shattering, in fact, are few and far between. Building on the work of others contributes to the literature more than most people otherwise think. By doing spin-off projects, you are demonstrating that the original work is fertile, and that is a highly desirable quality in a theory (see earlier in this chapter, on the qualities of theory). You are also showing that the original findings are replicable—another desirable quality in research—and that the original piece was done correctly. It also builds cumulative knowledge in a research program. Think about it: If every scholar pursued his or her own research program, working entirely independently of other scholars (even those in closely related research programs), we would duplicate a lot of work without gaining any additional knowledge. We would have islands of knowledge with no bridges or connections between them, and this is not how the sciences—social or otherwise—progress. Doing work suggested by others who also work in the field is not beneath you or otherwise denigrating or inappropriate; it is a valuable and absolutely essential part of making progress as a field.

Even more, responding explicitly to another piece of scholarship can give you a leg up on the data collection phase of a project. The original author would have noted his or her data sources in the cited paper, meaning that you could easily re-create the dataset from the designated sources and focus your energy on collecting that one new variable you need to test your new part of the question. Sometimes, authors will even post the (quantitative) datasets that they used in the paper. These replication datasets may be on the authors' own personal websites, or they may be on the journal's common data web page.[13] In a one-term project like the one you are probably writing for this course, having such a well-developed starting point is a big help.

Besides coming from others' suggestions, research questions can also come from your own personal experiences or observations: something you read in the newspaper, something you experienced while traveling abroad, or any of a host of other sources. These are valid sources of ideas, too. They may be a bit more difficult to articulate than ones where other scholars have already blazed a trail, or they may require a bit more original data collection on your part, but they are very appropriate sources of ideas. Just be sure to be in touch with your instructor(s). You will eventually need to ground your argument in existing literatures, and your instructor(s) can help by suggesting related, parallel, or supporting lines of argument for you to investigate.

How to Phrase a Research Question

There is no one correct way to state a research question. Some ways are better than others, but which is better depends heavily on the question. As the Note to Students suggested, this is the part where the answer to many questions about this project will be, "It depends." We can, however, make a few generalizations about ways of phrasing research questions that are frequently better than others.

First, good research questions are open-minded. They don't go in with a biased or predetermined notion of what the research should find. "How can we resolve the intractable problems of the Arab-Israeli conflict?" is not a good research question. First, it assumes "problems"—a word with a distinctly negative connotation that already sets a tone for the argument that would follow. Second, it declares—again by assumption—that these problems are intractable. If the problems are intractable, then by definition, they cannot be resolved.[14] Likewise, "How much does conflict reduce trade between states?" presumes (before the author even completes the research) that the effect of conflict on trade is actually negative.

Second, good empirical research questions allow for the possibility of several potential answers. "How does conflict affect trade?" is a better version of the question, because conflict could possibly have any of several relationships

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[13]We revisit replication datasets, their organization and use, and their ethics in Chapter 7.

[14]This is also not a very good empirical research question; it's closer to a hypothetical one.

(positive, negative, no relationship) with the outcome of interest, trade. "Why do Americans vote for third-party candidates?" also could have multiple potential answers. How or why questions often work better here than "does" questions—"Does conflict affect trade?" has only two possible answers, yes or no, and we could easily broaden it to provide information about the direction of that relationship without adding additional work to the research.

Finally, most people generally find that narrowing a question is easier than broadening it. When in doubt, I suggest you phrase your question in the broadest way possible. If necessary, you can then narrow down the scope of the investigation—even if the theory and research question themselves are more expansive—to something that is doable for a particular project. Empirical research is typically about producing generalizable knowledge, that is to say, making claims that have support (or at least potential applicability) beyond any one time and space, and so one can usually make a case for testing on a sample of convenience. For example, you may have an argument about the effect of disasters on birth rates. We have reliable data for both of these things for virtually all countries of the world, but only for the mid-1960s to the present. We have little reason to believe that people in this era have a fundamentally different response to disasters than people in previous periods,[15] so testing simply on the sample of convenience does not impede the development of knowledge.
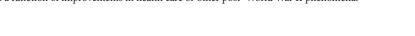
On a related note, however, one main caveat about the breadth of empirical research questions requires discussion here. We cannot limit the scope of a research question to, say, one election or one geographic region or one time period, simply because that one case personally interests us. An empirical research question asks about a general phenomenon. If we artificially limit the range of cases we consider by imposing some type of arbitrary geographic (or other) criterion, we risk biasing our pool of cases and getting spurious (and perhaps more dangerously, incorrect) findings. We should theorize about—and plan to test our theories on—the entire pool of cases where the phenomenon of interest occurs.

Of course, using the full population of cases is not always doable in practice. The biggest reason is data availability. US politics is one of the few areas where most of the data we could want are available, but even there, some significant gaps exist. For example, until 1940, US census forms did not ask any questions other than name, age, gender, and a crude coding of race. We thus lack sufficient data to study the effects of literacy and income on women and minority voters in the interwar period; that's unfortunate, because that window—between when women won the right to vote and when voting access became much easier for women but contested for minorities—could provide valuable insights about politics at the intersection of class, race, and gender. As another example, no reliable and

Examples of Bounded Theories and Bounded Investigations

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[15]Survival rates for the infants might differ from 1960 onward, but this does not affect the raw birth rate (live births per 1,000 women). So inferences made on the convenience sample are unlikely to be a function of improvements in health care or other post–World War II phenomena.

consistent national gross domestic product (GDP) data exist before the early 1940s (for the United States or anywhere else). Few important economic variables such as GDP and trade penetration are available at the level of US states. These are circumstances where the data simply don't exist and are nearly impossible to construct from available information. In these situations, we must find other ways to investigate the issues of interest. For the race, class, and gender question, for example, we might construct a qualitative investigation using a carefully selected set of oral history interviews from the Library of Congress and the National Archives and diaries written by women of that era as our primary data sources.[16]

Sometimes, data availability is limited by the deliberate choice of the data-collecting agencies, which often choose to focus on cases that are "important" or "interesting" for their agenda. The prevalence of the AIDS disease in Africa has led to the collection of much more detailed (and accurate) statistics for that region than for anywhere else. Only the EuroBarometer and Latinobarometro surveys ask a battery of questions about individual attitudes toward economic integration that are both cross-nationally comparable and asked in a consistent time series; of those, only the Latinobarometro series focuses on issues relevant to developing countries. These types of limitations are artificial and are products of choices by the data-collecting agencies, and using just these cases without acknowledgement of their unusualness risks very serious problems. Where relevant and where possible, we should attempt to expand the pool of data to include other relevant cases. These cases are "interesting" for a reason that probably correlates with the same thing we're interested in, and so using only "interesting" cases—even if they're the only ones for which data are readily available—leads to some possible problems.[17]

Other times, some phenomena only happen in certain geographic regions. The only countries undergoing transitions from communist economies to purely market-based ones are in central and eastern Europe;[18] only southeastern US states have regular experiences with hurricanes. These types of limitations are part of the natural world—they are constraints on the scope of comparable phenomena or cases that arise independently of any action or intervention by the investigator. Regardless of the source of the constraints,

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[16]We know that widespread differences in literacy rates existed at this time, so using diaries alone would bias our sample. The use of oral history interviews as a complementary data source helps to mitigate the effects of literacy.

[17]The classic exposition of these types of "selection bias" issues is Geddes (2003), especially Chapter 3 ("How the Cases You Choose Affect the Results You Get"). We return to these issues in Chapters 5, 6, and 7.

[18]Afro-socialism did not, for example, involve extensive efforts at heavy industrialization and regionwide central planning in the same manner that Soviet-governed economies did. Cuba and China are, arguably, still mostly communist economies, or at least are not making explicit transitions to capitalist market structures, so neither of these sets of states are relevant to the research question.

though, the pool of available data limits the set of cases about which we can draw conclusions. In both cases, either natural limitations or imposed ones, the researcher must be aware of the limitations that data availability can cause and be careful to express his or her findings in a way that reflects these limits on inference.

In short, any boundary you place on an empirical investigation, geographic, chronologic, or otherwise, has to be theoretically defensible, or at a minimum (in the case of data availability or manageability constraints) empirically justifiable. If you have one of these relatively rare bounded questions, or are concerned that you may need to limit your study to a range narrower than that of your theory, you should plan to consult with your instructor often.

Preview 1.1 Sample Research Questions

Previous students have examined the questions below in their papers. I explain here why these questions were good and suggest variants of them that would not have worked well.

   A.   *How does a midterm recession affect presidential economic policy?*

   This question worked really well. The student wanted to know whether presiding over a recession that starts during a term makes a president become more centrist or more extreme in his preferred economic policy. The student studied all post–World War II US presidents who experienced a marked decline in economic growth. By looking at how frequently the presidents used specific key words in their annual State of the Union speeches before and after the recessions, the student could identify trends in economic policy preferences that were distinct from enacted policies (some requiring central bank or Congressional consent) and economic outcomes (which depend on many nonpolicy forces). Studying a single president and a single recession would not give a representative sample; we can't generalize from that.

   B.   *How do disasters affect the birth rate?*

   This question is mostly viable, but as it's currently phrased, it's rather broad and the outcome of interest, birth rate, is a specific indicator instead of a concept. What larger idea does the birth rate represent here—population trends? Social response to external threat? What kind of disasters: only natural ones, or do man-made ones (like wars) count too? Answering that question depends fundamentally on your theory—the argument you have for why you expect disasters to influence birth rates. If you expect disasters

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to influence birth rates because citizens realize the effect of the disaster on the population's age distribution, and so a bulge in the birth rate is intended to repopulate the country, then yes, including man-made disasters in the research probably makes sense. If you have a different type of argument to link the cause (independent) and outcome (dependent) variables—perhaps that it's a religious or faith-motivated response against the injustice of the (deity-inflicted) disaster—then including man-made disasters does not make sense on a theoretical basis. Adding a geographic or chronological boundary to this question likewise does not make sense from a theoretically justifiable point of view.

C. *Why does the political left become social democratic in some Latin American countries and populist in others?*

This is a viable research question. The geographic limitation here is acceptable because the phenomenon of interest—populist political movements—only occurs in Latin America, at least in a form that is comparable to the other cases. Using only Latin American cases removes the need to control for (make an adjustment to accommodate) potential background variables that may vary across cases but affect their values on the dependent variable—colonial history, geographic region, previous experience with both popular and authoritarian rule, etc.

Practice 1.1 Evaluating Research Questions

Consider each of the questions below. Determine if each is clearly specified enough to be the basis for a research project (i.e., a single paper, about 20–25 pages). If it is not, identify what parts are too broad, or too narrow, and find a way to rephrase it to make the question viable for a single research project.

A. Why do presidents spend so much time talking about foreign policy during the campaign when only 1–2% of voters base their votes on it?

B. How do countries' economic interests affect their positions on the conflict in Syria?

C. Why did the British election of 2010 produce a Conservative minority government?

D. Does international law cause states to change their behavior?

Progress 1.1 Brainstorming Research Questions

Use this activity to begin brainstorming some potential research questions for your research project. You should plan to generate at least two or three ideas, in case one turns out not to be viable for some reason or another.

A. On a clean sheet of paper, take 2 to 3 minutes to make a list of any political science or international relations terms that come to mind. Use a timer or other device (one song on the radio or your MP3 player, for example) to manage the time. Just write anything that comes to mind.

B. Take another 2 to 3 minutes to review your list. Using a pencil, put an *x* by anything that is less attractive to you. Put a check mark by anything that is particularly interesting to you. Put a question mark by any term that is a proper noun (starts with a capital letter). Again, manage the time. Don't spend too long on this.

C. Get out a second sheet of paper. Make a second pass through the list, and this time, write (on the new page) any pairs of checkmarked words that seem to fit together. If you suspect they appear to have a relationship—for example, household wealth and educational outcomes—put an arrow between them showing the direction of the suspected relationship; don't worry about whether the relationship increases or decreases for now. Try to avoid the words marked with a question mark. You have about 5 or 6 minutes for this stage—one longer track or two shorter (radio version) songs.

D. Return to your new list and consider the pairs that you've associated. Can you formulate them into research questions? Play around with them, reversing the order in which the terms appear, perhaps adding another term that you forgot in the initial brainstorm, or by adding some more specific terms or ideas to them. Again, you probably shouldn't spend more than 5 or 6 minutes here. Try to write two to three potentially viable research questions. Consider how and where you might need to narrow them to make them doable, and where you might need to broaden them to think in terms of generalizable explanations.

WRITING YOUR PAPER

As we continue through the book, you'll find sections like this at the end of most chapters. They are designed to help you with the actual writing of the paper, section by section. Most researchers write the final draft of their paper in a short period around the completion of the actual research. This is normal, even for faculty.[19] To write the paper successfully at the end, though, you will need two important documents: a research notebook and a Leftovers file.

Your research notebook can be any old notebook or even a computer file. I personally prefer using a paper notebook because I can work simultaneously on the paper and on my computer screen more easily. This may be a generational handicap, so your mileage may vary. The voice of experience, however, strongly implores you not to settle for scribbles on scraps of paper. They will get lost. Use a notebook or something else with pages that are all attached together. Your research notebook is the place to scribble answers to any Progress activities that your instructor doesn't ask you to turn in. It's also a place to jot your thoughts, questions, and realizations as you work your way through the paper.

Tendencies toward procrastination aside, I encourage you to begin sketching out sections of your paper in writing as you encounter these sections in the book. Like any piece of good writing, a quality research paper goes through a number of drafts. Very little of this initial writing will appear in your final draft, but the writing itself will be invaluable for several reasons. First, you can ensure that you actually know all the things you need to know to write the section—and if you don't know them, it will identify exactly what you don't know so you can reread, do research, or otherwise figure out that information before the paper deadline. It forces you to articulate everything explicitly rather than letting it slide past when you can't easily find words. Second, it provides you a record of what you know and what you were thinking at various stages of your research project. Don't rely on your memory for keeping track of everything. You won't remember, and you'll find yourself missing key things when you go to write. Making a note now takes a lot less time and effort than trying to reverse-engineer your decisions when you're under the pressure of a deadline. Third, and perhaps most important, you can get the ideas started so they can marinate. Your argument will evolve, especially as you delve deeper into the project. Do not expect that your earliest versions will actually end up in the paper; most will end up in your Leftovers file.

This is where we come to our second necessary document: the Leftovers file. Much like a good spaghetti sauce, some ideas improve with a bit of time chilling in another container. The Leftovers file is where you stick all those bits

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[19]One of the reasons we apply to and attend conferences is so that we have deadlines for writing our papers. And even then, most of us don't seriously start writing until about a month before, which allows about 2 weeks to write before we have to send off the paper. (This is a dirty little professional secret; don't tell your professor that I told you.)

of ideas from your early drafts that don't make the final cut. Sometimes, this is because your ideas have changed; other times, you've found better ways to express them. Occasionally, you get a really cool idea for something else that just doesn't go into this particular paper. But the key thing about a Leftovers file is that it's a single place to dump this stuff, where you can find it again later when you need it. For me, this is usually a single running Word document, but again, your mileage may vary.

SUMMARY

In this chapter, we explored the idea of the social world as a collection of patterned phenomena, and how the social sciences attempt to make sense of those patterns. We value the characteristics of parsimony, predictiveness, falsifiability, fertility, and replicability in research. Research questions are one of four types—normative, hypothetical, factual/procedural, or empirical—depending on the goal or purpose of the investigation. Empirical research questions deal with the world "as it is," seeking general explanations for patterns of outcomes or classes of phenomena. A good research question is one whose answer takes as much space as your paper has length.

KEY TERMS

- Parsimony
- Predictiveness
- Falsifiability
- Fertility
- Replicability
- Normative
- Hypothetical
- Factual/procedural
- Empirical
- Research topic
- Research question