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2 Beginnings

During my career in science, now nearly a half century in duration, I have grown more and more aware that success in science, paralleling success in most careers, comes not so much to the most gifted, nor the most skillful, nor the most knowledgeable, nor the most affluent of scientists, but rather to the superior strategist and tactician. The individual who is able to maneuver with propriety through the world of science along a course that regularly puts him or her in a position of serendipity is often the one who excels.

Jack Oliver[1]

Broadly stated, the goal of science is to discover new things about the world and to appraise the truth-value of extant propositions about the world. Consider our exemplars, democracy and vouchers, introduced in Chapter 1. We want to uncover new things about the process of democratization and the impact of vouchers on school performance. At the same time, we want to test extant theories about these two subjects. Social science may, therefore, be understood as a twin quest for *discovery* and for *appraisal*, as summarized in Table 2.1.[2]

The chapter begins by introducing these goals, followed by a review of their implications for more specific methodological tasks. The next section approaches the goal of discovery through the concrete task of finding a research question. Since the remaining chapters of the book assume that a research question – perhaps even a specific hypothesis – has been identified, this chapter functions as a prologue to the rest of the book.

1 Oliver (1991: ix).

2 This contrast can be traced back to Reichenbach (1938), who distinguished between a "context of discovery" and a "context of justification." See also Hanson (1961); McLaughlin (1982); Nickles (1980); Popper (1965); Zahar (1983). Critics (e.g., Schiemann 2003) note that the distinction is not a dichotomy, i.e., the two goals are difficult to separate in practice. My claim, however, is not that they comprise a crisp typology. Rather, I claim that they are two fundamental goals of science that impose somewhat different methodological strategies and criteria upon the activity of science.

Table 2.1 General goals of social science

1. **Discovery** (conjecture, exploration, innovation, theory formation)
   Is it new?
2. **Appraisal** (assessment, demonstration, evaluation, justification, proof, testing, verification/falsification)
   Is it falsifiable?

Discovery

"An author is little to be valued," says Hume in his characteristically blunt fashion, "who tells us nothing but what we can learn from every coffee-house conversation."[3] We should like an argument, and corresponding empirical analysis, to contribute something novel to our understanding of a topic. A good piece of research is one that is innovative, one that makes a novel contribution – usually understood with respect to the key hypothesis or general theory.

Of course, some "discoveries" are not really new, or are not as innovative as they purport to be. Authors sometimes slight the accomplishments of others, formulate their argument against a ridiculous null hypothesis (a "straw man" argument), overstate the accomplishment of their own work, or adopt neologisms that repackage old wine in new bottles. Our contempt for various species of pseudo-innovation confirms the general point: good research should push the frontiers of knowledge forward.

In this quest, researchers are generally forced to adopt an exploratory approach to the world. New territory is entered, or established territories are interrogated for unexpected patterns (anomalies). New explanations are tested or invented out of whole cloth. Discovery requires an aggressive and critical engagement with the status quo.

This is characteristic of initial phases of research. But it is also the goal to which all top researchers aspire, for everyone wishes to situate themselves on the frontiers of knowledge. In the words of one scientist, "the only interesting fields of science are the ones where you still don't know what you're talking about."[4] In this sense, we are all – always – beginners.

Consider the question of democratization, introduced in Chapter 1. How and why do some states democratize, while others do not (or are unable to sustain those gains)? This is not an easy question to answer – some might

[3] Hume (1985: 254). [4] I. I. Rabi, quoted in Root-Bernstein (1989: 407).

argue that it is not amenable to general theory – but it is undoubtedly an important one.[5]

Innovation at the descriptive level concerns ways in which the broad topic of democratization might be productively conceptualized and measured. Is there a critical moment of *transition* at which the process of democratization is achieved? Is there a point of *consolidation* beyond which reversals are unlikely? Are there distinctive *sequences* by which democratization occurs? How should democracy, and its various subtypes (illiberal democracy, electoral democracy, competitive authoritarianism), be defined? These are just a few of the descriptive questions that have occupied scholars in recent years.

At the causal level, scholars have focused on the possible preconditions for successful democratization. Are certain authoritarian regime types more likely to democratize than others? Does the existence of mineral wealth (e.g., oil or diamonds) in a country make democracy less likely? To what extent does a country's colonial experience color its propensity for achieving and maintaining a democratic form of rule? How much impact (if any) does economic development have on democratic/authoritarian outcomes?

In a more general vein, one can identify certain characteristic types of causal innovation. Sometimes, a new factor, X, is proposed as a contributing cause for a well-studied outcome, adding a new variable to existing models. That would describe most of the examples listed in the previous paragraph. Less common is the theoretical eclipse of existing theories about Y with a new causal framework. Thus, Daron Acemoglu and James Robinson have proposed that democratization can be understood as a distributional struggle between the haves and the have-nots.[6] A third type of causal reformulation consists in working back from an established causal factor, X, to some prior cause (X_1) that explains X, and thereby Y (reframing X as a causal mechanism). Thus, it might be argued that geographic circumstances (e.g., climate, soil quality, disease vectors, access to deep-water ports and navigable rivers) affected patterns of colonization and resource extraction, with lasting effects on the distribution of wealth and power, and, ultimately, on a country's propensity to democratize.[7] A fourth type of innovation focuses on the causal mechanisms lying within an established X/Y relationship. In this fashion, a good deal of work has been devoted to the causal links between resource wealth and authoritarian rule. Michael Ross summarizes:

[5] For recent reviews of the literature see Berg-Schlosser (2007); Coppedge (forthcoming); Geddes (2007).
[6] Acemoglu and Robinson (2005).
[7] This follows the line of argument initiated by Acemoglu, Johnson, and Robinson (2001); Sokoloff and Engerman (2000).

> A "rentier effect" . . . suggests that resources rich governments use low tax rates and patronage to relieve pressures for greater accountability; a "repression effect" . . . argues that resources wealth retards democratization by enabling governments to boost their funding for internal security; and a "modernization effect" . . . holds that growth based on the export of oil and minerals fails to bring about the social and cultural changes that tend to produce democratic government.[8]

A study focused on causal mechanisms typically culminates in a new explanation for why X causes Y (in this case, why there is a "resource curse"). If no plausible causal mechanism can be discovered, such a study might also serve to disconfirm the entire hypothesis. A fifth type of innovation focuses on the population of an inference (its breadth or scope). One might argue that the connection between resource wealth and authoritarianism is applicable only to the developing world, and not to advanced industrial countries (e.g., Norway). Or one might attempt to extend the ambit of the theory to apply to different time periods (e.g., Greek city-states) or different phenomena (corporate governance).

Evidently, there are many ways to innovate, which is to say, there are many types of discoveries. This is because there are many types of theories, and each theory has multiple parts – an issue we shall attempt to disentangle in the coming chapters.

Appraisal

The second over-arching goal of science is to ensure that the truth-value of propositions about the world can be tested rigorously. "The criterion of the scientific status of a theory is its *falsifiability*, or refutability, or testability," asserts Karl Popper.[9] This process, in contrast to the goal of discovery, must be hedged about with rules. Otherwise, we shall never be able to reach consensus on anything and the goal of truth (which presumes the possibility of reaching consensus) dissipates. Fortunately, the process of appraisal is more amenable to general principles than the process of discovery. And this, in turn, helps to explain why it has been an abiding preoccupation of methodologists. (It is virtually the sum total of the field of methodology, as traditionally conceived.)

With respect to the construction of arguments, it may be appropriate to begin by repeating an old story (perhaps apocryphal) about a physics doctoral

8 Ross (2001: 327–328). See also Dunning (2008a).

9 Popper (1965: 37). Arguably, Popper's ([1934] 1968) classic treatise, *The Logic of Scientific Discovery*, was mis-named. It offers not a logic of discovery, but rather a logic of testing. In any case, I prefer the term "appraisal" rather than "falsifiability," as the latter presumes a certain approach to testing that may not be entirely justified.

defense. At the conclusion of the proceedings, one of the examiners excoriates the hapless candidate with the following remark: "This is the worst thesis I have ever read. It is not even wrong."

The sign of a nonfalsifiable proposition, Popper points out, is that virtually "any conclusion we please can be derived from it."[10] It may be true by definition, but it is not true by any standards that one might subject to empirical test. Popper charged that a number of highly influential theories, including Marxism and Freudianism, suffered this fatal flaw. They could not be proven or disproven. They were neither right nor wrong.

As it happens, Marxism and Freudianism are still with us, along with Weberianism, realism (a theory of international relations), rational choice and a host of other difficult-to-appraise theoretical frameworks. In the natural sciences, as well, explanations such as string theory persist, despite their seeming nonfalsifiability. It would appear that broad and ambiguous frameworks are sometimes useful, even when they cannot be clearly appraised. Indeed, appraisal is by no means the only criterion of a good argument. That said, there is near universal recognition that falsifiability is *a* virtuous ideal – one to be striven for, even when conditions do not seem to be propitious.

Popper also recognized that falsifiability is not a dichotomous matter (either/or) but rather a matter of degrees. Some theories are more falsifiable than others. Indeed, none of the examples mentioned above are entirely resistant to empirical refutation. And even the most tractable theories put up some resistance.

Generally speaking, an argument is most falsifiable insofar as it is operational, parsimonious, general in purview (offering a large territory for empirical testing), well bounded (so that the population of an inference is clear, and defensible), coherent (internally consistent), clear with respect to counterfactuals and comparisons, and relying on as few assumptions as possible. Additional issues arise during the theory-testing phase of research. For example, one is more inclined to believe a result if a solid "partition" has been maintained between the construction of the argument and its subsequent testing; this ensures that there is minimal wiggle-room to adjust the argument to suit the results of a test or to adjust the test to suit the hypothesis. Good tests are "severe"; bad ones are permissive. With respect to causal analysis, the most stringent tests are usually experimental in nature. And so forth.

A great wealth of factors – many more than Popper explicitly considered – contribute to the rigor with which a hypothesis is appraised. These are explored

10 Popper ([1934] 1968: 92).

in subsequent chapters. Some of these criteria are intrinsic to the formal structure of the argument; others relate to the procedures used to test that argument.

Tradeoffs

Arguably, all the tasks, strategies, and criteria introduced in the remaining chapters are ways of achieving or instantiating either discovery or appraisal. These primal goals inform every methodological endeavor.

Complicating matters, however, these methodological goals are often in tension with one another. On the one hand, researchers are encouraged to seek out the unknown. This requires an *exploratory* approach to the empirical world, for there is no systematic procedure for discovering new things. And the newer the thing (the more revolutionary), the less rule-bound is the procedure. Paul Feyerabend makes this point forcefully:

> The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science meets considerable difficulty when confronted with the results of historical research. We find then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. It becomes evident that such violations are not accidental events, they are not results of insufficient knowledge or of inattention which might have been avoided. On the contrary, we see that they are necessary for progress. Indeed, one of the most striking features of recent discussions in the history and philosophy of science is the realization that events and developments, such as the invention of atomism in antiquity, the Copernican Revolution, the rise of modern atomism (kinetic theory; dispersion theory; sterochemistry; quantum theory), the gradual emergence of the wave theory of light, occurred only because some thinkers either *decided* not to be bound by certain "obvious" methodological rules, or because they *unwittingly broke* them.[11]

The process of discovery is inherently anti-nomothetic – or, as Feyerabend would say, anarchic.[12] From this perspective, traditional scientific methodology is too respectful of existing theoretical constructs and methods. Scientists need to get outside the iron cage of normal science – to a place where the processes

[11] Feyerabend (1975: 23).

[12] Feyerabend (1963, 1975). Although Feyerabend took a radical stance against science (as traditionally understood), his work is digestible within the framework of traditional philosophy of science if approached as a corrective to a naive, Popperian ("positivistic") view of the scientific process. Much of what Feyerabend had to say applied with particular force to the context of discovery (though he rejected the utility of the discovery–appraisal distinction).

of exploration and testing are mutually intertwined and difficult to disentangle. Here, theories are not always neatly and cleanly falsifiable.

On the other hand, researchers are rightly encouraged to develop risky propositions and hard tests, so as to assist in the task of appraisal. This is the conservative moment of science, personified by Karl Popper. Here, there are plenty of rules (or at least general tasks, strategies, and criteria) to guide one's research.

The falsificationist considers the greatest sins of social science to be those of commission, rather than omission. The virtue of good science is to keep quiet when the truth is ambiguous – not to say more than one knows with a reasonable level of certainty. (Indeed, Popper counsels against the use of the term "truth" under *any* circumstances.) Only in this fashion will the products of science be distinguishable from conjectures, the stock-in-trade of politicians, journalists, and cocktail-party prognosticators. Only if the field is clear of nonsense will the long, slow process of scientific cumulation occur.

Many social scientists have embraced this austere, taciturn view of science (at least rhetorically). Here, the primary job of the methodologist is to vigilantly guard the gates of science, ensuring that no unauthorized entrants are admitted. Contra the orthodoxy, I will insist that at least half the battle of science lies in identifying interesting problems to solve. Indeed, finding the right question may be more important in the long run than finding the right answer for a less interesting hypothesis. From this perspective, good science is not just a matter of rigor but also of insight (or, if you prefer a more religiously tinged metaphor, of *inspiration*). Note that theoretical development could not occur, or would occur only very slowly and haltingly, if researchers kept their Popperian blinders on – limiting themselves to pre-formed hypotheses and yes/no empirical tests. A constructive methodology should enable researchers to think about problems in new ways; it should not focus narrowly and obsessively on testing.

To be sure, there is plenty of ammunition for protagonists in both camps. There are those who feel that there is altogether too much testing and not enough theory (or not enough good theory), and that our efforts should therefore be focused on the latter. And there are those who feel that there is too much theory (or too many theories) and not enough testing, and that our efforts should be focused on the latter. Which side of this debate one adopts depends upon how much confidence one has in either venture. If one is confident in one's ability to craft better theories and correspondingly skeptical of our ability to test them, one hews to the discovery camp. If, on the other hand, one is skeptical about obtaining lasting theoretical advances and relatively optimistic about devising new and better tests, one finds oneself in the appraisal camp. This is not a debate that we can settle; I simply note the issue for readers to consider.

The harder, and surely the more important, question is how innovative one ought to be in the choice of topic. Again, there are two positions, each of which has compelling points to make. Some bemoan the lack of theoretical ambition found among the current generation of scholars, presumably by reference to an earlier generation of "Big Thinkers." Adam Przeworski writes:

> The entire structure of incentives of academia in the United States works against taking big intellectual and political risks. Graduate students and assistant professors learn to package their intellectual ambitions into articles publishable by a few journals and to shy away from anything that might look like a political stance. This professionalism does advance knowledge of narrowly formulated questions, but we do not have forums for spreading our knowledge outside academia.[13]

It is probably true that members of today's generation are more apt to accept the norms and extant theories of the discipline than the 1960s generation, which perhaps qualifies them as less theoretically ambitious. Probably, they are less politically engaged – though this is not necessarily connected to intellectual curiosity. Alternatively, one might argue that this generation has focused its energies in a more productive fashion than previous generations. Indeed, many of the "Big Theories" propounded in the social sciences – then and now – are difficult to digest. If a theory is not falsifiable, or does not cumulate well with other theories (either subsuming them or taking its place beside them), it is unlikely to move a field forward.

In sum, the question of how theoretically ambitious one should be is difficult to answer in the general sense. One should be exactly as ambitious as one can be, while retaining touch with the empirical reality under investigation. The goals of theoretical innovation must be balanced by the quest for theory appraisal.

Indeed, from Popper's perspective, the goals of discovery and appraisal are entirely compatible with one another. "Bold conjectures" can be combined with strenuous efforts at "refutation."[14] Sometimes this is possible, and to the extent that it is, it defines the *summum bonum* of science.

[13] Quoted in Snyder (2007: 20). [14] Popper ([1934] 1968, 1965).

Even so, the tension between discovery and appraisal seems rather more intrinsic and irresolvable than Popper was willing to admit. Consider that if one's primary motivation is the discovery of new theories, then researchers must have latitude to propose broad and abstract theories without clearly testable hypotheses. Insofar as hypotheses are generated and tested, this testing process should be open-ended – involving numerous hypotheses and a continual process of adjustment between theory and evidence – before, during, and after the research is conducted. It is not surprising that research of the "soaking and poking" variety (whether qualitative or quantitative) is not very convincing – though it may be quite provocative, and may lead, down the line, to more convincing demonstrations of truth.

Insofar as one's primary motivation is to test the truth-value of an existing theory one's mode of procedure must be quite different. Here, a theory should be framed in as precise a manner as possible so that it issues specific, testable predictions. The process of theory discovery and appraisal should be segregated from one another as much as possible, so there is little room for subjective interventions in the testing process or *post hoc* alterations of the theory. In all respects, theory and research design should be "risky," allowing many opportunities for a theory to fail. The problem with this style of research is equally apparent. If taken seriously, Popper's injunctions would severely constrain the type of theories admissible to the canon of social science. In addition to Marxism and Freudianism, which Popper explicitly condemned, it would also raise doubts about Weberian theories, social capital theory, evolution-based models, theories of international relations (e.g., realism, liberalism, idealism/constructivism), rational-choice models, and many others as well. Within the natural sciences (Popper's home turf), the demand for falsifiability would presumably force one to reject string theory and other highly abstract and scarcely testable components of modern physics.

Popperians might respond that, whatever messiness might be involved in the process of discovery, *at some point* theories ought to be issued in falsifiable form. This merely begs the question: at what point should this be? Note that most of the theoretical frameworks mentioned previously have been extant for a century or more, and appear to be no closer to a definitive empirical test. Indeed, broad theories rarely fall when they fail empirical tests. These failures, contra Popper, can usually be explained away (perhaps by *ad hoc* adjustments of the theory), or treated as part of the error term.[15]

[15] Gorski (2004); Lakatos (1978).

To adopt a phrase from Douglas MacArthur: old theories never die, but they sometimes fade away. Specifically, they meet their demise when a more compelling theory is proposed, one which attracts researchers formerly committed to the long-established theory. Gradually, theory *B* eclipses theory *A*. The process is Lakatosian (involving grand theoretical frameworks) rather than Popperian (involving middle-range propositions). In this respect, progress at theoretical and empirical levels cannot be separated from one another. And in this respect, again, it may appear that our energies are better focused on the generative component of science than on the falsifiability–verifiability component. Marx, Freud, and Weber ought to be our avatars, not the thousands of assembly-line social scientists who spend their lives testing middle-range theories.

I shall conclude by returning to the central point: good science must embrace both the goal of discovery and the goal of appraisal. One without the other is not serviceable. Indeed, science advances through a dialectic of these two broad research goals.

In the language of statistical tests, the emphasis of exploratory analysis is on avoiding Type II errors (accepting a false null hypothesis), while the emphasis of falsification is on avoiding Type I errors (incorrectly rejecting a true null hypothesis).

In Kuhnian terms, the conflict between theory development and theory-testing may be understood as a contrast between "revolutionary" (paradigm-breaking) science and "normal" (paradigm-constrained) science. Although the terms are perhaps inappropriately apocalyptic, the contrast highlights a recurrent tension in the field of science, where some labor to invent new theories while others labor to test those theories.[16]

[16] One way of negotiating this dispute is to examine the specific circumstances of a piece of research to see which sort of approach is warranted. A falsificationist procedure is likely to be justifiable wherever research on a topic is abundant, the principal hypothesis is well defined, experimental methods can be applied, Type I errors are of greater concern than Type II errors, one has reason to be especially concerned about the personal biases and preconceptions of the researchers, a neutral oversight body is available to monitor research on a topic, and research funding is plentiful – in these cases, hypothesis-generation and hypothesis-testing are appropriately segregated, and rigid rules of procedure ought to be applied. Popper, not Feyerabend, should be our guide. And yet, these conditions are often absent – especially in the social sciences. Given this fact, there is little point in dressing up our research as if it fits the requirements of Popperian science. Note that social science journals frequently insist upon the presentation of *a priori* hypotheses ("suggested by the literature"), which will then (the writer characteristically moves into the future tense) be "tested against the data," even when the procedures actually followed in the course of the research are blithely exploratory. Nothing is gained – and a great deal may be lost – by presenting our findings in this misleading fashion. Recognizing this, the disciplines of social science need to do a better job of distinguishing work that is theory-testing from work that is – rightly, and justifiably – theory-generating. Both should be honored, insofar as circumstances (outlined above) warrant.

g a research question

Most of this book is devoted to problems of appraisal once a specific hypothesis has been identified. This follows standard practice among methodological texts. However, a few words on the problem of theory development are in order. How does one go about identifying a fruitful research question and, ultimately, a specific research hypothesis? This is the very early exploratory phase, when one quite literally does not know what one is looking for, or at. Arguably, it is the most crucial stage of all. Nothing of interest is likely to emanate from research on topics that are trivial, redundant, or theoretically bland – no matter how strong the research is from a falsificationist perspective.

Methodologists generally leave this task to the realm of metaphor – bells, brainstorms, dreams, flashes, impregnations, light bulbs, showers, sparks, and whatnot. The reason for this lack of attention is perhaps to be found in the fact that beginnings are inherently unformulaic. There are few rules or criteria for uncovering new questions or new hypotheses. Methodologists may feel that there is nothing – nothing scientific at any rate – that they can say about this process. Karl Popper states the matter forthrightly, as usual: "There is no such thing as a logical method of having new ideas," he writes. "Discovery contains 'an irrational element,' or a 'creative intuition.'"[17]

However, saying nothing at all may be worse than saying something unsystematic. The rest of this chapter therefore departs from the format adopted elsewhere. What I have to offer is more in the character of a homily than a framework. It reads like an advice column. I urge the reader to study the tradition, begin where you are, get off your home turf, play with ideas, practice dis-belief, observe empathically, theorize wildly, think ahead, and conduct exploratory analyses. As a result, the chapter is ridden with *shoulds* and *should nots*. I apologize in advance for the rather didactic tone.[18]

My advice is largely commonsensical and by no means comprehensive. It cannot help but reflect my own views and experiences, though I have drawn extensively on the writings of other scholars.[19] Nonetheless, it may help to

[17] Quoted in King, Keohane, and Verba (1994: 129).

[18] With regard to my own bona fides, let me note that in this particular area of research ("starting out") I can perhaps claim special authority. Over the past two decades, I have found myself continually starting afresh with new topics, some of which (perhaps inevitably) have turned out to be less enlightening than others.

[19] The literature relevant to this chapter emanates from research on the conjoined subjects of discovery, innovation, and exploration, as well as from advice columns in newsletters and introductory textbooks.

orient those who are setting out on their first journey, or who wish to begin again.

Study the tradition

The question of innovativeness necessarily hinges on the tradition of work that already exists on a subject. This is not a subjective prior; it is one established by a field of scholars working on a topic over many years, and it should be apparent in the published work that they have produced. (If not, the inquiry must be carried out through personal communication with established scholars in a field.)

Consider the state of the field on a topic. What are the frontiers of knowledge? What do we – collectively, as a discipline – know, and what don't we know? Consider also the probable location of this frontier a decade from now, extrapolating from current scholarly trends. What will the cutting-edge be then? Keep in mind that the most active research frontiers are usually moving frontiers; the tradition as it exists today may be quite different when you finish your research. So a better question (though a more difficult one) is, what will the cutting-edge be in a decade?

I doubt if anyone has happened upon a really interesting research topic simply by reading a review of the extant literature. However, this is an efficient method of determining where the state of a field lies and where it might be headed. Be aware that because of the length of time required by the publication process, the most recent work on a subject is usually to be found in conference papers or papers posted on personal web sites. Nowadays, these are easy to locate through search engines. Your first recourse might be Google rather than JSTOR.

In exposing oneself to the literature on a topic one must guard against two common responses. The first is to worship those that have gone before; the second is to summarily dismiss them. Respect the tradition – don't flagellate the forefathers. There is nothing so jejune as a reversal of hierarchies ("They're wrong and I'm right"). But don't be awed by the tradition either. Try stepping

Regrettably, this literature is focused mostly on the organizational context of discovery (e.g., by social psychologists and sociologists) and on discovery within the natural sciences, where the concept has its counterpart in the notion of a clear "finding." In the social sciences, where definitive findings are scarce and cumulation more dubious, the concept of discovery carries a more ambiguous meaning. With this caveat, the following works proved useful: Koestler (1964); Luker (2008); McGuire (1997); Mills (1959: 195–226); Oliver (1991); Root-Bernstein (1989); Snyder (2007). See also Abbott (2004); Fleck ([1935] 1979); Freedman (2008); Geddes (2003: 27–45); Hanson (1958); King, Keohane, and Verba (1994: 14–19); Kuhn ([1962] 1970); Langley *et al.* (1987); Most (1990); Root-Bernstein and Root-Bernstein (1999); Useem (1997); Watson (1969). On the creative act of constructing formal models, see Cartwright (1983); Hesse (1966); Lave and March (1975).

outside the categories that are conventionally used to describe and explain a subject. By this I mean not simply arguing against the common wisdom, but also thinking up new questions, new issues, that have not been well explored. Insofar as new theoretical paradigms are "revolutionary," this is what they consist of.

As you peruse the literature, be conscious of what excites you and what bothers you. Which issues are under-explored, or badly understood? Where do you suspect the authorities in a field are wrong? What questions have they left unanswered? What questions do you find yourself asking when you finish reading? Where does this line of research lead? Sometimes, typically in a conclusion or a review article, scholars will reflect self-consciously upon the future direction of research; this, too, can be useful.

In any case, you should not limit your eventual review of the literature to only the most recent publications. Of interest is not only the frontier but the history of a subject. Thus, a complementary strategy is to delve into the "classics" – the founding texts of a field or subfield.[20] This is useful (particularly if you have never done so) because it sometimes prompts one to think about familiar subjects in new ways, because classic works tend to be evocative (and thus raise questions), because a different vocabulary is often employed, and because it is a reminder that some things have, in fact, been done before. This last point is educational in two respects: it warns us that we may be about to reinvent the proverbial wheel and it informs us of ways that perceptions and conclusions about a familiar subject have changed within a discipline (and within society at large) over time. Every subject has an intellectual history and it is worthwhile familiarizing yourself with this history, not merely to find a pithy epigraph but also to inform your analysis of a problem.

As C. Wright Mills began his study of elites, he consulted the works of Lasswell, Marx, Michels, Mosca, Pareto, Schumpeter, Veblen, and Weber.[21] In commenting upon this experience, Mills reports:

> I find that they offer three types of statement: (a) from some, you learn directly by restating systematically what the man says on given points or as a whole; (b) some you accept or refute, giving reasons and arguments; (c) others you use as a source of suggestions for your own elaborations and projects. This involves grasping a point and then asking: How can I put this into testable shape, and how can I test it? How can I use this as a center from which to elaborate – as a perspective from which descriptive details emerge as relevant?

[20] Snyder (2007). [21] Mills (1959: 202).

Not every topic is blessed with such a rich heritage; but some are, and there it is worth pausing to read, and to think.

Begin where you are

With questions of method Charles Sanders Peirce points out, "There is only one place from which we ever can start . . . and that is from where we are."[22] The easiest and most intuitive way to undertake a new topic is to build upon what one knows and who one is. This includes one's skills (languages, technical skills), connections, life experiences, and interests.[23]

Hopefully, a chosen topic resonates with your life in some fashion. This is often a source of inspiration and insight, as well as the source from which sustained commitment may be nourished and replenished over the life of a project. C. Wright Mills writes:

> You must learn to use your life experience in your intellectual work: continually to examine and interpret it. In this sense craftsmanship is the center of yourself and you are personally involved in every intellectual product upon which you may work. To say that you can "have experience," means, for one thing, that your past plays into and affects your present, and that it defines your capacity for future experience. As a social scientist, you have to control this rather elaborate interplay, to capture what you experience and sort it out; only in this way can you hope to use it to guide and test your reflection, and in the process shape yourself as an intellectual craftsman.[24]

Because the business of social science is to investigate the activities of people, any personal connections we might have to such people may serve as useful points of leverage. The hermeneutic act is eased if one can establish some personal connection – however distant or imaginative – with the group in question.[25]

Sometimes, our connection with a topic is motivated more by ideas than by personal connections. We are naturally drawn to subjects that are either horrifying or uplifting (or both). Indeed, many research projects begin with some notion – perhaps only dimly formulated – about what is wrong with the world. We all have bees in our bonnets and this normative motivation may be vital to our insight into that topic. What real-life problem, relevant to your discipline, bothers you?[26]

22 Kaplan (1964: 86), paraphrasing Charles Sanders Peirce.
23 Finlay and Gough (2003); Krieger (1991); Mills (1959); Snyder (2007). 24 Mills (1959: 196).
25 Gadamer (1975) refers to this as a fusion of horizons – us and theirs (the actors we are attempting to understand).
26 Gerring and Yesnowitz (2006); Shapiro (2005); Smith (2003).

The desire to redress wrongs also helps to keep social science relevant to the concerns of lay citizens. We all begin, one might say, as citizens, with everyday ("lay") concerns. Over time, we come to attain a degree of distance from our subject, qua scholars. Thus, do the roles of citizen and scholar engage in dialogue with one another (Chapter 14).

Of course, at the end of a project one must have something to say about a topic that goes beyond assertions of right and excoriations of wrong. The topic must be made tractable for scientific inquiry; otherwise, there is no point in approaching it as a scientific endeavor. If one feels that the topic is too close to the heart to reflect upon it dispassionately, then it is probably not a good candidate for study. As a probe, ask yourself whether you would be prepared to publish the results of a study in which your main hypothesis is proven wrong. If you hesitate to answer this question because of normative pre-commitments you should probably settle on another subject.

As a general rule, it is important to undertake questions that one feels are important, but not projects in which one has especially strong moral or psychological predilections for accepting or rejecting the null hypothesis.[27] Thus, one might be motivated to study the role of school vouchers because one is concerned about the quality of education. But one probably should not undertake a study of vouchers in order to prove that they are a good/bad thing.

Get off your home turf

While the previous section emphasized the importance of building upon one's personal profile (skills, connections, druthers), it is also vital for scholars to stray from what is safe, comfortable, and familiar – their home turf.

Consider that the academy is not now, and likely never will be, a representative cross-section of humankind. At present, the denizens of social science are disproportionately white, Anglo-European, and (still, though decreasingly) male. They will probably always be disproportionately privileged in class background. Evidently, if members of these disciplines restrict themselves to topics drawn from their personal experience little attention will be paid to topics relevant to excluded groups, especially those that are less privileged.

The more important point is that advances in knowledge usually come from transgressing familiar contexts. After all, local knowledge is already familiar to those who live it. Whatever value might be added comes from transporting categories, theories, and ways of thinking across contexts, in the hope that new

27 Firebaugh (2008: ch. 1).

perspectives on the familiar will become apparent. A good ethnography, it is sometimes said, renders the exotic familiar *or* the familiar exotic. The same might be said of social science at large. Try to think like a stranger when approaching a topic that seems obvious (from your "home turf" perspective). Likewise, do not be afraid to export categories from your home turf into foreign territory – not willfully, and disregarding all evidence to the contrary, but rather as an operating hypothesis. Sometimes, the foreign-made shoe fits.

Indeed, novel descriptive and causal inferences often arise when an extant concept or theory is transplanted from one area to another. For example, the concept of *corporatism* arose initially in the context of Catholic social theory as an alternative to state socialism. It was later adopted by fascist regimes as a way of legitimating their control over important economic and social actors. More recently, it has been seen as a key to explaining the divergent trajectories of welfare states across the OECD, and for explaining the persistence and resilience of authoritarian rule in the developing world.[28] There are endless ways of adapting old theories to new contexts. Sometimes these transplantations are fruitful; other times, they are not.

Most important, try to maintain a conversation with different perspectives on your subject. What would so-and-so say about *X*? If this does not drive you mad, it may serve as a helpful form of triangulation on your topic.

Another sort of boundary crossing is that which occurs across disciplines, theories, and methods. The trend of the contemporary era seems to be toward ever greater specialization, and to be sure, specialization has its uses. It is difficult to master more than one area of work, given the increasingly technical and specialized techniques and vocabulary developed within each subfield over the past several decades. Making a contribution to a field necessitates a deep familiarity with that field, and this requires a concentrated focus over many years.

Yet it is worth reflecting upon the fact that many of the works that we regard today as path-breaking have been the product of exotic encounters across fields and subfields. Indeed, all fields and subfields were the product of long-ago transgressions. Someone moved outside their comfort zone, and others followed. Note also that the social sciences are not divided up into discrete and well-defined fields. So, try reading inside, *and outside*, your area of training. Talk to people in distant fields. See how they respond when you describe your questions, and your projected research, to them. Beware of cultivating a narrow expertise, for this is apt to lead to work that is theoretically circumscribed or

28 Collier (1995); Schmitter (1974).

mundane. If all academic work is theft of one sort or another, one is well advised to steal from distant sources. Another word for this sort of theft is creativity.

Play with ideas

The literature on invention and discovery – penned by science writers, philosophers of science, and by inventors themselves – is in consensus on one point. Original discoveries are usually not the product of superior brainpower (i.e., the ability to calculate or reason). Robert Root-Bernstein is emphatic:

> Famous scientists aren't any more intelligent than those who aren't famous. [Moreover,] I'm convinced that successful ones aren't right any more often than their colleagues, either. I believe that the architects of science are simply more curious, more iconoclastic, more persistent, readier to make detours, and more willing to tackle bigger and more fundamental problems. Most important, they possess intellectual courage, daring. They work at the edge of their competence; their reach exceeds their grasp . . . Thus, they not only succeed more often and out of all proportion; they also fail more often and on the same scale. Even their failures, however, better define the limits of science than the successes of more conventional and safe scientists, and thus the pioneers better serve science.[29]

The key question, as Root-Bernstein frames it, is "How can one best survive on the edge of ignorance?"[30]

One way of answering this question is suggested by Richard Hofstadter, who describes intellectual life as a counterpoint of *piety* and *playfulness*. The first refers to the somber and dogged search for truth. The second, which saves the enterprise from dogmatism and which may be less obvious, is the intellectual's capacity to play:

> Ideally, the pursuit of truth is said to be at the heart of the intellectual's business, but this credits his business too much and not quite enough. As with the pursuit of happiness, the pursuit of truth is itself gratifying, whereas the consummation often turns out to be elusive. Truth captured loses its glamor; truths long known and widely believed have a way of turning false with time; easy truths are a bore, and too many of them become half-truths. Whatever the intellectual is too certain of, if he is healthily playful, he begins to find unsatisfactory. The meaning of his intellectual life lies not in the possession of truth but in the quest for new uncertainties. Harold Rosenberg summed up this side of the life of the mind supremely well when he said that the intellectual is one who turns answers into questions.

29 Root-Bernstein (1989: 408). 30 Root-Bernstein (1989: 408).

Echoing Hofstadter's description, one might say that there are two distinct moments in any research project. The first is open-ended, playful; here, a wide variety of different ideas are generated and given a trial run. The second is filled with zeal and piety; here, one grips tightly to a single idea in the quest to develop it into a full-blown theory and test it against some empirical reality. This conforms to the distinction between discovery and appraisal introduced above. Whatever the shortcomings of this dichotomy, there is no question that the academic endeavor requires a crucial shift of attitude at some point in the enterprise. Since we are concerned here with the initial phase, we shall dwell on techniques of playfulness.

Although the art of discovery cannot be taught (at least not in the way that the technique of multiple regression can be taught), it may be helpful to think for a moment about thinking. The act of creation is mysterious; yet there seem to be a few persistent features. Arthur Koestler, synthesizing the work of many writers, emphasizes that discoveries are usually "already there," in the sense of being present in some body of work – though perhaps not the body of work with which it had heretofore been associated. To discover is, therefore, to connect things that had previously been considered separate. To discover is to think *analogically*:

> This leads to the paradox that the more original a discovery the more obvious it seems afterwards. The creative act is not an act of creation in the sense of the Old Testament. It does not create something out of nothing; it uncovers, selects, re-shuffles, combines, synthesizes already existing facts, ideas, faculties, skills. The more familiar the parts, the more striking the new whole. Man's knowledge of the changes of the tides and the phases of the moon is as old as his observation that apples fall to earth in the ripeness of time. Yet the combination of these and other equally familiar data in Newton's theory of gravity changed mankind's outlook on the world.[31]

What frame of mind does this require? How does one think analogically? This trick seems to have something to do with the capacity to "relinquish conscious controls," to block out the academic superego that inhibits new thoughts by punishing transgressions against the tradition.[32] Above all, one must feel free to make mistakes:

> Just as in the dream the codes of logical reasoning are suspended, so "thinking aside" is a temporary liberation from the tyranny of over-precise verbal concepts, of the axioms and prejudices engrained in the very texture of specialized ways of thought. It allows the mind to discard the strait-jacket of habit, to shrug off apparent contradictions, to

[31] Koestler (1964: 119–120). [32] Koestler (1964: 169).

> un-learn and forget – and to acquire, in exchange, a greater fluidity, versatility, and gullibility. This rebellion against constraints which are necessary to maintain the order and discipline of conventional thought, but an impediment to the creative leap, is symptomatic both of the genius and the crank; what distinguishes them is the intuitive guidance which only the former enjoys.[33]

It might be added that what also distinguishes the genius and the crank is that the former has mastered the tradition of work on a subject. The genius' liminal moments are creative because they take place on a foundation of knowledge. In order to forget, and thence recombine features of a problem, one must first know.

The analogy of discovery with a dream-like trance, although it borders on silliness, may not be far off. Koestler writes:

> The dreamer constantly bisociates – innocently as it were – frames of reference which are regarded as incompatible in the waking state; he drifts effortlessly from matrix to matrix, without being aware of it; in his inner landscape, the bisociative techniques of humour and discovery are reflected upside down, like trees in a pond. The most fertile region seems to be the marshy shore, the borderland between sleep and full awakening – where the matrices of disciplined thought are already operating but have not yet sufficiently hardened to obstruct the dreamlike fluidity of imagination.[34]

It has often been suggested that the mind works semi-consciously on problems once they have been identified, and when sufficient motivation is present. At this stage, one becomes possessed by a question.

Practice dis-belief

One cannot think without words, but sometimes one cannot think well with them either. Sometimes, ordinary language serves to constrain thought-patterns, reifying phenomena that are scarcely there. When we define, Edmund Burke commented, "we seem in danger of circumscribing nature within the bounds of our own notions."[35] Language suggests, for example, that where a referential term exists a coherent class of entities also exists, and where two referential terms exist there are two empirically differentiable classes of entities. Sometimes this is true, and sometimes it is not. Just because we have a word for "social movement" does not mean that there are actually phenomena out there that are similar to each other and easily differentiated from other phenomena. Ditto for "social capital," "interest group," and virtually every other key concept in the

[33] Koestler (1964: 210). [34] Koestler (1964: 210). [35] Quoted in Robinson (1954: 6).

social science lexicon. Words do not always carve nature at its joints. Sometimes, they are highly arbitrary ("constructed"). *A fortiori*, just because we have a word for some phenomenon does not mean that cases of this phenomenon all stem from the same cause, or the same set of causes. It is not even clear that the same causal factors will be *relevant* for all members of the so-named set of phenomena.

The reader might respond that, surely, concepts are defined the way they are because they are useful for some purposes. Precisely. But it follows that these same concepts may not be useful for *other* purposes. And since one's objective at this stage of the research game is to think unconventionally, it is important to call into question conventional language. For heuristic purposes, try assuming a nominalist perspective: words are merely arbitrary lexical containers. As an exercise, put brackets around all your key terms ("social movement"). Try out different visions; see if any of them are persuasive. (This is a good example, incidentally, of the differing criteria applicable to the discovery and appraisal moments of science. A nominalist perspective on concepts is problematic when the writer turns to the task of formalizing his or her research. Here, the usual counsel is to *avoid* neologism, unless absolutely required [Chapter 6].) Another technique for thinking anew about a subject is to consider the terms that foreign lexicons or ancient lexicons impose upon a concept; often they will have different connotations or suggest different distinctions among phenomena.

A parallel skepticism must be extended to numbers, which also naturalize phenomena that may, or may not, go together in the suggested fashion. Here, the claim is more complicated. First, the use of a number is explicitly linked to a dimension – for example, temperature, GDP, number of auto accidents – that is thought to be relevant in some way. Moreover, the imposition of a numerical scale presupposes a particular type of relationship between phenomena with different scores on that variable – nominal, ordinal, interval, or ratio (Chapter 7). But is it *really*? More broadly, is this the dimension that matters (for understanding the topic in question)? Or are there other dimensions, perhaps less readily quantified, that provide more accurate or insightful information?

Another sort of conventional wisdom is contained in paradigm-cases. These are cases that, by virtue of their theoretical or everyday prominence, help to define a phenomenon: the way Italy defines fascism; the Holocaust defines genocide; the United States defines individualism; Sweden defines the welfare state; and the Soviet Union (for many years) defined socialism. Paradigm-cases exist in virtually every realm of social science inquiry. They often provide good points of entry into a topic because they are overloaded

with attributes; they operate in this respect like ideal-types (Chapter 6). Yet because they anchor thinking on these topics, they are also thought-constraining. And because they are also apt to be somewhat unusual – for example, extreme – examples of the phenomenon in question, they may present misleading depictions of that phenomenon.

With respect to words, numbers, and paradigm-cases – not to mention full-blown theories – it is important to maintain a skeptical attitude. Perhaps they are true and useful, perhaps only partially so, or only for certain purposes. In order to test their utility, try adopting the Socratic guise of complete ignorance (perhaps better labeled as thoroughgoing skepticism). Once having assumed this pose, you are then free to pose naive questions of sources, of experts, and of informants. It is a canny strategy and can be extraordinarily revealing – particularly when "obvious" questions cannot be readily answered, or are answered in unexpected ways.

Observe empathically

One technique of discovery is empathic, or (to invoke the philosophical jargon) hermeneutic.[36] Here, one employs observational techniques to enter into the world of the actors who are engaged in some activity of interest – playing ball, drafting a bill, murdering opponents, casting a vote, and so forth – in order to understand their perspective on the phenomenon. Of course, this is easier when the actors are our contemporaries and can be studied directly (i.e., ethnographically). It is harder, and yet sometimes more revealing, if the actions took place long ago or are removed from direct observation and must be reconstructed. In any case, non-obvious perceptions require interpretation, and this interpretation should be grounded in an assessment of how actors may have viewed their own actions.

Consider that the process of understanding begins with an ability to re-create or re-imagine the experiences of those actors whose ideas and behavior we wish to make sense of. Somehow a link must be formed between our experiential horizons and the horizons of the group we wish to study. This may involve a form of role-playing (what would I do in situation *X* if I were person *Y*?). Some level of sympathy with one's subjects is probably essential for gaining insight into a phenomenon. This may be difficult to muster if the subject is grotesque. No one wants to empathize with Nazis. But the hermeneutic challenge remains; some way must be found to enter into the lives and perceptions of these

[36] Gadamer (1975).

important historical actors in order to explain their actions, however strange and repellant.

Although those who identify with the interpretivist label are not always theoretically inclined, we may grant that many of those who identify as "theorists" have at one time or another employed interpretive techniques (on the sly). In any case, this technique need not be monopolized by a few specialist practitioners ("interpretivists," "ethnographers," etc.). It is a game we can all play – indeed, must play, if we are to be successful social scientists.

Theorize wildly

Rather than working single-mindedly toward One Big Idea, you might consider the benefits of working simultaneously along several tracks. This way, you avoid becoming overly committed to a single topic too early. You can also compare different topics against one another, evaluating their strengths and weaknesses. "Just have lots of ideas and throw away the bad ones," advises Linus Pauling.[37]

At the same time, you should do your best to maintain a record of your ideas as you go along.[38] Take a look at this idea diary every so often and see which projects you find yourself coming back to, obsessing about, inquiring about. The objective should be to keep your mind as open as possible for as long as possible (given the practicalities of life and scholarly deadlines). "Let your mind become a moving prism catching light from as many angles as possible."[39]

Historians of natural science identify productive moments of science with the solving of anomalies – features of the world that do not comport comfortably with existing theories.[40] If these anomalies can be solved in a more than *ad hoc* manner, the frontiers of knowledge are pushed forward. Perhaps even a new "paradigm" of knowledge will be created.

One may question whether social science is ripe with theoretically tractable anomalies. Some would say that it exists entirely of anomalies; there are no unsolved interstices to fill, only a deep abyss of highly stochastic behavior that is resistant to theorizing of any sort. It seems clear that most social science fields are not – or not yet – in the realm of Kuhnian normal science. Still, we focus our energies, quite rightly, on areas that are thought to be less well explained. Whether these are understood as anomalies or as "areas of deeper-than-usual ignorance" hardly matters for present purposes.

[37] Quoted in Root-Bernstein (1989: 409). [38] Mills (1959: 196). [39] Mills (1959: 214).
[40] Kuhn ([1962] 1970); Lakatos (1978); Laudan (1977).

Another technique for theorizing wildly is to juxtapose things that do not seem to fit naturally together. Theorizing often consists of dis-associating and re-associating. One version of this is to examine a familiar terrain and think about what it resembles. What is "X" an example of? Charles Ragin refers to this as "casing" a subject.[41] Another tactic is to examine several diverse terrains in order to perceive similarities. (Can colonialism, federalism, and corporatism all be conceptualized as systems of "indirect rule"?[42]) A third version is to examine a familiar terrain with the aim of recognizing a new principle of organization. Linnaeus famously suggested that animals should be classified on the basis of their bone structures, a new principle of classification that turned out to be extraordinarily fecund.[43] In the realm of social science, scholars have provided organizational schemes for political parties, bureaucracies, welfare states, and other social phenomena – though few, it must be noted, have proven as fruitful or as enduring as the Linnaean. Of course, a reorganization of knowledge by way of classification need not be eternal or ubiquitous in order to prove useful for certain purposes. Each re-classification may have distinct uses.

A third technique for loosening the theoretical wheels is to push a conventional idea to its logical extreme. That is, consider an explanation that seems to work for a particular event or in a particular context. (It may be your idea, or someone else's.) Now push that idea outward to other settings. Does it still work? What sort of adjustments are necessary to make it work? Or consider the logical ramifications of a theory – if it were fully implemented. What would the theory seem to require?

Theories are tested when they are pushed to their limits, when they are tried out in very different contexts. Root-Bernstein observes that this strategy leads, at the very least, to an investigation of the boundaries of an idea, a useful thing to know. Alternatively, it may help us to reformulate a theory in ways that allow it to travel more successfully, that is, to increase its breadth. A third possibility, perhaps the most exciting, is that it may lead to a new theory that explains the new empirical realm.[44]

In theorizing wildly, it is important to keep a list of all possible explanations that one has run across in the literature, or intuited. As part of this canvas, one might consider some of the more general models of human behavior, for example, individual (aka rational) choice, exchange, adaptation (aka evolution), diffusion, and so forth.[45] Sometimes, these abstract models have applications to very specific problems that might not be immediately apparent. (How might the

[41] Ragin (1992). [42] Gerring *et al.* (2011). [43] Linsley and Usinger (1959).
[44] Root-Bernstein (1989: 413). [45] Lave and March (1975).

topic of romance be understood as an exchange? As an adaptation? As a product of diffusion?)

Once achieved, this list of possible explanations for phenomenon *Y* can then be rearranged and decomposed (perhaps some propositions are subsets of others). Recall that theoretical work often involves recombining extant explanations in new ways. Your list of potential explanations also comprises the set of rival hypotheses that you will be obliged to refute, mitigate, and/or control for (empirically) in your work. So it is important that it be as comprehensive as possible.

In order to figure out how to correctly model complex interrelationships it is often helpful to draw pictures. (If one is sufficiently fluent in graphic design, this may be handled on a computer screen. For the rest of us, pencil and paper are probably the best expedients.) Laying out ideas with boxes and arrows, or perhaps with Venn diagrams or decision trees, allows one to illustrate potential relationships in a more free-flowing way than is possible with prose or math. One can "think" abstractly on paper without falling prey to the constraints of words and numbers. It is also a highly synoptic format, allowing one to fit an entire argument, in all (or most) of its complexity, onto a single sheet or wallboard.

Think ahead

All elements of the research process are intimately connected. This means that there is no such thing as a good topic if that topic is not joined to a good theory and a workable research design. So, the choice of a "topic" turns out to be more involved than it first appears. Of course, all the elements that make for a successful piece of research are unlikely to fall into place at once. And yet one is obliged to wrestle with them, even – one might say, especially – at the very outset.

Recalling the elements of your topic – containing, let us say, a theory, a set of phenomena, and a possible research design – it is vital to maintain a degree of fluidity among all these parts until such time as you can convince yourself that you have achieved the best possible fit. Beware of premature closure. At the same time, to avoid endless cycling it may be helpful to identify that element of your topic to which you feel most committed, that is, that which is likely to make the greatest contribution to scholarship. If this can be identified, it will provide an anchor in this process of continual readjustment.

Consider the initial decision of a topic as an investment in the future. As with any investment, the pay-off depends upon lots of things falling into place

over subsequent years. One can never anticipate all the potential difficulties. But the more one can "game" this process, the better the chance of a pay-off when the research is completed. And the better the chance that the research will be completed at all. (Really bad ideas are often difficult to bring to fruition; the more they advance, the more obstacles they encounter.)

Although the prospect may seem daunting, one is obliged to think forward at the "getting started" stage of research. Try to map out how your idea might work: what sort of theory will eventuate, what sort of research design, and so forth. If everything works out as anticipated, what will the completed thesis/book/article look like? (This brings us to the topics entertained in the rest of the book, that is, what *are* good concepts, descriptive inferences, causal inferences, and research designs?)

An obvious question to consider is what "results" a study is likely to generate. Regardless of the type of study undertaken there will presumably be some encounter with the empirical world, and hence some set of findings. Will the evidence necessary to test a theory, or generate a theory, be available? Will the main hypothesis be borne out?

Sometimes, the failure to reject a null hypothesis means that the researcher has very little to show for his or her research. Conventional wisdom has prevailed. Other times, the failure to prove a hypothesis can be quite enlightening.[46] Sometimes, a topic is so new, or a research design so much more compelling than others that came before, that *any* finding is informative. This is ideal from the perspective of the scholar's investment of time and energy, as it cannot fail to pay off.

In any case, it may be helpful to inquire of those who know a subject intimately (experts, key informants) what they think you will find if you pursue your projected line of research. What is their best hunch? And how would they respond to a failure to reject the null hypothesis? Would it be publishable? Would the *rejection* of your null hypothesis be publishable? This is an even more important question, and it is not always apparent to the novice researcher. That which seems novel to you may seem less novel to those who have labored in a field for many decades. And, by the same token, that which seems obvious to you may be surprising to others. Thus, you are well advised to market-test various findings. Consider how your anticipated findings might be situated within the literature on a topic. How will they be perceived? What will be their value-added? Will they be considered more compelling than other extant work

[46] This raises the question of how one ought to define a "null" hypothesis; but let us leave this matter in abeyance.

on the subject? Will they stand the test of current scholarship and the test of future scholarship (the "test of time")?

In test-driving your idea you should also keep a close eye on yourself. See if your oral presentation of the project changes as you explain it to friends and colleagues. At what point do you feel most confident, or most uncertain? When do you feel as if you are bull-shitting? These are important signals with respect to the strengths and weaknesses of your proposal. Indeed, the process of presenting – aside from any concrete feedback you receive – may force you to reconsider issues that were not initially apparent.

Conduct exploratory analyses

When the time is right, consider conducting an exploratory probe. This should be constructed so as to be as efficient as possible – requiring the least expenditure of time, energy, and money. You need to get a feel for your subject, and what the data might say; there is no pretense of drawing firm conclusions. Sometimes, the best way to think through a proposal is to implement the idea in a schematic fashion.

One time-honored approach is the exploratory case study, enabling one to gain more in-depth knowledge of one or a few cases that are thought to exemplify key features of a topic. Here, one finds a number of (more or less well-known) varieties.[47] A *typical* case is one that exhibits traits that are deemed to be highly representative of the phenomenon of interest. It may be useful as a clue to what is going on within other similar cases. An *extreme* case is one that exhibits an extreme (or rare) value on a relevant (X or Y) parameter. When understood against the backdrop of "normal" cases (lying nearer to the mean), an extreme case offers supreme variation on the parameter of interest; this may offer insights into what is going on across the larger population. A sample of *diverse* cases are those that exhibit a range of variation on one more or the relevant (X, Y, or X/Y) parameters. With only a small set of cases, this provides a way of exploring all the available variation that a larger population offers. A *deviant* case is one that exhibits an unexpected outcome, according to some set of background assumptions. This is commonly used to open up new avenues of inquiry, a way of identifying anomalies. A *most-similar* sample of cases have similar background characteristics, but exhibit

47 Gerring (2007: ch. 5).

different outcomes along some parameter of theoretical interest. This allows the researcher to generate hypotheses about the possible causes of an outcome that varies across otherwise similar cases.[48]

Another exploratory approach allows one to probe a larger sample of cases in a more superficial fashion. The researcher might begin with an existing dataset (to which additional variables of interest can be added). Or the researcher may try to construct his or her own "truth-table," focusing upon a small number of cases and variables of interest. Suppose one is attempting to determine why some countries in sub-Saharan Africa have democratized while others have not in the decades since independence. One would begin by coding the dependent variable (autocracy/democracy), and proceed to add possibly relevant causal factors – economic growth, urbanization, landlocked status, colonial history, and so forth. Some of these factors might be binary, while others could be coded continuously or reduced to a binary format (e.g., high/low). Some of these factors are likely to be easy to code ("objective"), while others may involve considerable judgment on the part of the coder ("subjective"). In any case, this simple data-reduction technique allows one to incorporate a large number of hypotheses and to eye-ball their fit with the evidence across a small- or medium-sized sample.

The key point of these adventures in data exploration is to reveal new hypotheses and to expose one's hunches to preliminary tests, as quickly as possible. Do not be afraid to deal in stylized facts – rough guesstimates about the reality under consideration. More systematic testing procedures can wait for a later stage of the process. Data exploration should be understood as a series of plausibility probes.[49]

Of course, the point at which theory exploration segues into theory-testing is never entirely clear-cut. Any method of exploration is also, to some degree, a method of testing, and vice versa. The expectation, in any case, is that once a key hypothesis has been identified it will be subjected to more stringent tests than were employed in its discovery. The emphasis of research shifts subtly but importantly from avoiding Type II errors (failing to reject a false null hypothesis) to avoiding Type I errors (incorrectly rejecting a true null hypothesis), as discussed.

48 These varied case-selection strategies can be implemented in qualitative (informal) or quantitative (formalized) ways. The latter requires a large sample of potential cases and relevant data on the parameters of interest. Statistical techniques for selecting one or a few cases from a large sample are explored in Gerring (2007: ch. 5).

49 Eckstein (1975).

Concluding thoughts on beginnings

Published work in the social sciences presents a misleading appearance of order and predictability. The author begins by outlining a general topic or research question, then states a general theory, and from thence to the specific hypothesis that will be tested and his or her chosen research design. Finally, the evidence is presented and discussed, and concluding thoughts are offered.

This is nothing at all like the progress of most research, which is, by comparison, circuitous and unpredictable – hardly ever following a step-by-step walk down the ladder of abstraction. One reason for this is that knowledge in the social sciences is not neatly parceled into distinct research areas, each with specific and stable questions, theories, and methods. Instead, it is characterized by a high degree of open-endedness – in questions, theories, and methods.

Another factor is the circularity of the enterprise. Each element of social science – the research question, theory, hypothesis, key concepts, and research design – is interdependent. This is because each element is defined in terms of all the others. Thus, any adjustment in one element is likely to require an adjustment all around. As soon as I change my theory I may also have to change my research design, and vice versa. There is no Archimedean point.

This means that there are many points of entry. One might begin with a general topic, a research question, a key concept, a general theory, a specific hypothesis, a compelling anomaly, an event, a research venue (e.g., a site, archive, or dataset), a method of analysis, and so forth. Accordingly, some research is problem- or question-driven, some research is theory-driven, some research is method-driven, and other research is phenomenon-driven (motivated by the desire to understand a particular event or set of events). These are obviously quite different styles of research – even though, at the end of the day, each study must be held accountable to the same methodological criteria (summarized in Table 1.1).

Once begun, the correct procedure is difficult to diagram in a series of temporally discrete steps – unless one imagines hopping to-and-fro and back-and-forth in a rather frenetic fashion. Empirical investigation is necessarily contingent on pre-formed concepts and theories, as well as our general notions of the world; yet further investigation may alter these notions in unpredictable ways. In so doing, we revise our conception of what we are studying. In this respect, social science offers a good example of the so-called hermeneutic circle.[50]

[50] Hoy (1982).

To reiterate, there is no right or wrong place to start. All that matters is where you end up. And yet, where one ends up has a lot to do with where one starts out, so it is not incidental. Scholars are rightly wary of the consequences of choosing a bad topic – one that, let us say, promises few interesting surprises, has little theoretical or practical significance, or offers insufficient evidence to demonstrate a proposition about the world. No matter how well executed that research might be, little can be expected from it.

Moreover, changing topics midstream is costly. Once one has developed expertise in an area it is difficult to re-tool. Research, like many things in life, is heavily path-dependent. For this reason, one should anticipate living with one's choice of topic for a very long time. A dissertation will not only absorb your life over the course of its duration but also, in all likelihood, for decades to come – perhaps for the rest of your life. Indeed, many scholars continue to be defined, for better and for worse, by their first published work. So, the question of choosing a topic is by no means trivial. A great deal is at stake.

Because the selection of a good topic is difficult, careful deliberation is in order. Note that the difficulty of topic selection is a product of the fact that everyone is looking for the same thing: fruitful topics for research, the next breaking wave. This means that the low-hanging fruit is probably already picked. Accordingly, one should not expect a great and heretofore unexplored topic to fall into one's lap. Even if it so happens that one's first hunch is correct it will take some time before the promise of this topic is fully apparent. Many initial probes will have to be followed through and an extensive literature review must be undertaken in order to confirm that the topic is truly innovative.

In this arduous process, advice is welcome – from friends, family, advisors, experts in the field. Solicit all the feedback you can. But make sure that, at the end of the day, you are comfortable with the choice you make. It should represent your considered judgment.

This is likely to require some time. How much, it is difficult to say. Finding a topic is a process, not an event. It doesn't happen all of a sudden. It starts as soon as one takes up scholarship and transposes gradually into the research itself. There is no clear beginning or end-date. Although the writer may be required to compose a formal grant proposal or prospectus this usually turns out, in retrospect, to be an arbitrary marker within the ongoing life of a project.

Many scholars are not prepared for the agonizing and time-consuming task of head-scratching (aka chin-rubbing, forelock-tugging – choose your metaphor), which seems to run counter to the injunction to publish, publish, publish (quickly, quickly, quickly). Once upon a time, life in the academy was extolled as a *via contemplativa*. Nowadays, one is struck by the fact that there is a great deal of publishing but relatively little sustained cogitation. Most of our time is spent in the implementation of projects. We secure funding, oversee staff, construct surveys, design experiments, peruse evidence, write up results, all the while maintaining a frenetic email correspondence. Only in brief moments do we allow ourselves the luxury of thinking deeply about a subject. By this, I mean thinking in truly open-ended ways, ways that might lead to new insights.

At what point should one make a commitment to a research question and a specific hypothesis? How does one know when to reach for closure? Evidently, there are dangers associated with precipitous decisions and with decisions that are too long delayed.

Consider this familiar scenario, related by Kristin Luker. A student ("you") enters his or her advisor's office with a hazily framed idea of what he or she would like to work on. The advisor demands to know what the hypothesis is.

> If you flounder around trying to answer this question, he or she may follow up by asking what your independent and dependent variables are. Even more basically, he or she will ask what your research question is. You just go blank, feeling like a rabbit trapped on the roadway with the headlights bearing down on you, as you try desperately to explain what's so interesting about, say, privatized water, or rising rates of imprisonment in America, or adolescent sexuality. When you and your advisor part at the end of the time allotted to you, more likely than not, you part in mutual frustration.[51]

In this setting, the student is probably not ready to identify a research question, much less a specific hypothesis. It is still a relevant question, and the advisor is obliged to raise it. However, in the haste to answer this question in a satisfactory way – and escape from the scene with self-esteem intact – the student may commit to a question that is not, in the long run, very fruitful. The same thing happens with arbitrary deadlines imposed by the academic calendar – a conference to which one has committed to present, a prospectus defense date, and so forth. This is the Scylla of premature closure.

On the other extreme, one encounters the danger of belated closure. Luker continues:

> Suppose on the other hand that you have an easygoing advisor, and you are permitted to go off "into the field" ... without answering his or her questions. An even more dreaded fate may well await you, worse than being tortured into producing independent and dependent variables on demand for your advisor, namely ... the Damnation of the Ten Thousand Index Cards or the Ten Thousand Entries into your computer-assisted note-taking system. The Damnation of the Ten Thousand Whatevers happens

[51] Luker (2008: 18).

> to unwitting graduate students who have spent many years ... gathering data without having stumbled upon exactly what it was that they were looking for when they first went out to that fabulous field site (... or library ...). There they sit, doomed and damned, in front of the computer screen, wondering how to make a story out of the ten thousand entries. Or, worse yet, they finally do stumble onto a story as they pore yet again over the ten thousand entries, but the single piece of information (or the body of data) which they need to really nail the point beyond quibbles is back in the field and they didn't know they needed it, or it's disappeared, or they can't afford to go back. Or they do find it, and realize that eighty percent of the data they have gathered is irrelevant ... An in-between outcome ... is that you may actually find the research question, come up with the data that you need to make the case, and have a compelling and ... well-written story to tell. The only problem is that you have eighteen boxes of data left over, and the entire enterprise took you at least four years longer than it should have.[52]

To describe this sort of disaster, Luker quotes a line from Pauline Bart: "Data, data everywhere and not a thought to think."[53]

In our own research – and regardless of whether we are just starting out as students of social science or have spent decades in the business – we must avoid the Scylla of premature closure as well as the Charybdis of belated closure. Neither will serve the cause of science, or our own careers. Push yourself to find a research question as quickly as possible, but don't settle on something that doesn't seem meaningful to you or to your intended audience.

[52] Luker (2008: 19). [53] Luker (2008: 19).