

CHAPTER 2

Big Questions, Little Answers

How the Questions You Choose Affect the Answers You Get

For the scholar who wants to contribute to the accumulation of knowledge, the first step in the process is choosing a question to investigate. This chapter makes some suggestions about how to shape research questions to increase the likelihood that they will yield compelling and robust theories. The early part of the chapter is an attempt to articulate some of the values and emotions that I believe motivate good scholars. These values and emotions undergird much research, but they are rarely expressed. On the contrary, the advice given to beginning scholars often implies the opposite values. Here I discuss the role of curiosity, indignation, and passion in the selection and framing of research topics.

In the second and much longer part of the chapter, I suggest a change in the way we usually think about the kinds of big, world-transforming subjects that comparativists often choose to study. Large-scale phenomena such as democratic breakdown, economic development, democratization, economic liberalization, and revolution result from the convergence of a number of different processes, some of which occur independently from others. No simple theory is likely to explain such compound outcomes. In principle, a complex, multifaceted theory might successfully do so, but in practice the task of constructing such theories has daunted most analysts. I propose changing the way we approach these questions. Instead of trying to “explain” such compound outcomes as wholes, I suggest a focus on the various processes that contribute to the final outcome, with the goal of theorizing these processes individually and generating testable propositions about them. In contrast to much of the methodological advice given in this book, the suggestions in this chapter do not derive from the logic of quantitative research. I cannot make any claim that this research strategy is more “correct” than any other. My

argument rests, rather, on the judgment that it is a more effective route to an accumulation of theoretical knowledge. The proof of the pudding is in the eating, however, and until we have some pudding, we cannot taste it. The last part of the chapter is an extended example of how to break up a big question into multiple processes, theorize one of them, and then test one of the implications of the theory thus devised.

"Science as a Vocation," Not Just a Job: Choosing a Research Topic

Students are often advised to choose research topics by looking for holes in the literature or by reading the ads in the *Personnel Newsletter* of the American Political Science Association to see what topics are hot. This advice conveys the impression that the search for research topics can and perhaps should be methodical and instrumental. This impression is false, and the advice, if followed, leads to a number of perversities: taking The Literature seriously whether it merits it or not; the selection of a topic that will become outdated before the dissertation is done; boredom.

Curiosity, fascination, and indignation should guide the choice of research topic. Emotion has been banned from most of the research enterprise, and properly so. But one place it should remain is in the choosing of research topics. The standard advice on how to choose a topic leaves out the role of such emotions as commitment, irritation, and obsession.

An especially thoughtful version of the standard advice is articulated by Gary King, Robert Keohane, and Sidney Verba (1994, 15–17), who advise students to pick topics that are important in the world and that contribute to an identifiable scholarly literature. Beginning scholars are advised to:

1. Choose a hypothesis seen as important by scholars in the literature but for which no one has completed a systematic study. . . .
2. Choose a hypothesis in the literature that we suspect is false (or one we believe has not been adequately confirmed) and investigate whether it is indeed false. . . .
3. Attempt to resolve or provide further evidence of one side of a controversy in the literature — perhaps demon-

strate that the controversy was unfounded from the start.

4. Design research to illuminate or evaluate unquestioned assumptions in the literature.
5. Argue that an important topic has been overlooked in the literature and then proceed to contribute a systematic study to the area. (16–17)

It would be difficult to disagree with any of this advice, and it is probably very useful to students in the sciencelike parts of social science. It assumes, however, that the relevant literature really does contain a considerable accumulation of theory and stylized facts. It thus fails to take into account the real state of a good deal of the literature in comparative politics. The literature on some subjects contains only a few arguments generally accepted as true; many controversies in which the hypotheses on both sides lack both clarity and strong empirical support; and large amounts of opinion and conjecture, unsupported by systematic evidence but nevertheless often referred to as theory. Such a literature creates a fuzzy research frontier. The reader finds not well-defined holes in the literature but swampy quagmires. Students who wade into these literatures often find themselves sinking into the quicksand of contested definitions and chasing after nebulous dependent variables that flit around like will-o'-the-wisps.

Consequently, good research in the field is more often motivated by curiosity about the world and intuition about cause-and-effect relationships than might be true in a field with more accumulated knowledge and a more clearly defined research frontier. Much of what is eventually judged to be exciting research in the comparative field either addresses subjects not covered in the literature or addresses old subjects in very novel ways, rather than extending the existing literature.

Contrary to the advice about looking for holes in the literature, good research in the comparative field often begins either with an intense but unfocused curiosity about why some event or process has happened or with a sense of sputtering indignation at the patent idiocy of some particular argument advanced in the literature. Sometimes political commitments or an aroused sense of injustice drive this curiosity or indignation. Potential researchers who feel little curiosity, intuition, or indignation in response to the social world and the arguments published about it should consider the possibility that they have chosen the wrong job.

The literature does play a role in the choice of a research topic, but not in the mechanical way suggested by the standard advice.

It stimulates the indignation, annoyance, and irritation that often fuel good research. When one reads an argument, finds it utterly implausible, and believes that one can find evidence to demonstrate that it is wrong beyond the possibility of refutation, one has indeed found a hole in the literature. Generally, however, these holes are not found through coolheaded searches. Instead, our gut-level response of irritation causes us to pause and notice them while we are reading for some other purpose. Moreover, such holes cannot be found unless the reader has sufficient background knowledge of facts to notice that the argument seems inconsistent with reality.

Arguments in the literature also create expectations about how events will play out in as yet unexamined cases. When we have some information about such cases that leads us to believe they may not meet expectations, our curiosity is aroused. Cases and outcomes may capture our interest because they differ from other cases or from what theory has led us to expect. Such outcomes call for explanation because they are anomalous when compared with other known or apparently understood instances. At this stage, the comparison may be entirely implicit, and the analyst may focus on the anomalous case; but without the implicit comparison, there would be no basis for considering the case interesting or puzzling.

I emphasize the emotional aspects of choosing research topics because these emotions contribute to the intense commitment to finding out what really causes things to happen that leads to good research. As Max Weber asserted, “Without this strange intoxication, ridiculed by every outsider; without this passion . . . you have *no* calling for science and you should do something else” (1958, 135).

Fostering Creativity

In the same essay, Weber also stressed the importance of the nonmethodical aspects of thought — intuition and inspiration. He emphasized the importance of having ideas, that is, of creativity:

Certainly enthusiasm is a prerequisite of the “inspiration” which is decisive. Nowadays . . . there is a widespread no-

tion that science has become a problem in calculation, fabricated in laboratories or statistical filing systems just as “in a factory,” a calculation involving only the cool intellect and not one’s “heart and soul”. . . . [but] some idea has to occur to someone’s mind . . . if one is to accomplish anything worthwhile. And such intuition cannot be forced. It has nothing to do with any cold calculation. (1958, 135)

Creativity is distributed as unequally among us as everything else, and very little is understood about it. Nevertheless, I believe that the way we train ourselves in graduate school and for the rest of our lives determines how much combustible material our creative sparks will find to ignite. Weber stressed that the soil in which ideas grow is normally prepared by very hard work. I will go further and suggest that some kinds of hard work are more likely to bear fruit than others.

Original ideas grow out of having individual and autonomous reactions to the world. We can have such reactions only on the basis of our own inner sense of how the world works. The task of the apprentice scholar, therefore, is to develop this inner sense. This process can be helped along in a somewhat conscious and systematic manner. Good scholarship arises from the interaction of observation and conjecture. We can intentionally increase the amount of observation we have to draw upon and thus deepen our ability to speculate fruitfully; we do this by exposing ourselves to large amounts of information, whether by wide reading about many countries and over long historical periods or via the scrutiny of masses of quantitative data. However it is done, the scholar is filling his or her stores with information within which to hunt for patterns and with which to probe the plausibility of hypotheses. I would urge all students to get into the habit of creating formal or informal “data sets,” that is, collecting and storing in some place other than their own fallible brains large quantities of factual information. (For some kinds of information, Excel spreadsheets are the perfect storage medium, but in other situations there may be no substitute for old-fashioned index cards.)

The kind of information that should be collected, of course, depends on the scholar’s interests. Whatever the topic, however, it is always useful to find out about it in countries and times outside one’s primary area of expertise. For example, if the student’s interests center on how oil wealth has affected government

in Middle Eastern countries, he should also stockpile some information on forms of government and uses of oil revenues in countries of other regions. The student interested in the effects of political institutions on the development of party systems in new democracies should resist the temptation to base her speculations on the experience of the countries most thoroughly covered in the literature—for example, Chile, Argentina, and Brazil—and should make sure she knows basic facts about electoral institutions and party systems in the small, less studied countries of Latin America and, if at all possible, in the new democracies of the rest of the world.

Having this kind of factual knowledge base helps the scholar to avoid making unfounded claims about the uniqueness of particular events, processes, or countries and also to avoid mistaking the simplified portraits of events often found in the literature for realistic descriptions. Much of the literature on many subjects in the comparative field is dominated by descriptions of events in a few much-studied countries. Much of the transitions literature on Latin America, for example, focuses on Brazil, Argentina, and Chile (along with Spain). Scholars working on transitions in other parts of the world assume that this literature accurately describes the general transition experience of Latin America, but it does not. Transitions have also occurred in Bolivia, Ecuador, Paraguay, Peru, and most of Central America. In some ways, the transitions in most of these latter countries more closely resemble those of African countries than those of the more often studied and more industrialized Southern Cone countries. If one's knowledge of Latin American transitions comes solely from the best known transitions literature, conclusions about differences between Latin American transitions and those in other regions will be inaccurate. Increases in factual information, however, improve the chance of finding the patterns that really exist.

A second aid to creativity arises from becoming fluent in the use of various kinds of models. When models—even such simple ones as the prisoner's dilemma—enter our imaginative repertoires, they make possible interpretations of information that simply would not have occurred to us otherwise.

A model is a simplified representation of a process. Its purpose is to illuminate a basic logic underlying the process that might not be perceptible from observation of the entire complicated reality overlaid, as all reality is, with multitudinous irrelevant details. A good model—one that is useful, fruitful, or

exciting — shows both its creator and those who are exposed to it something about the process that they had not perceived before.¹ When a model seems to fit the essential features of a situation, it enables the analyst to understand that situation more clearly and deeply than before. It also aids in communicating this understanding to others.

The collective action problem, usually expressed in purely verbal terms, is probably the best-known example of a model that simply changed the way we understand the world. Prior to the dissemination of the idea that individuals will not find it rational to expend their own resources in order to secure public goods for groups of which they are members, the failure of various disadvantaged groups to organize politically was considered puzzling. Much ink was spilled explaining false consciousness. Since Mancur Olson's very striking articulation (1965) of the collective action problem, our baseline expectations about political mobilization have been inverted. We now find it puzzling, and hence worthy of explanation, when large groups do manage to organize in order to press for some public good.

Another widely used model is the idea of evolutionary selection. The central idea here is that outcomes may occur in the absence of intentional decision making because the actors, organizations, states, parties, or other entities that fail to behave in certain ways will lose office or go out of existence. Thus the only ones that remain will be those that did behave as required, even though they may not have understood their situation or made conscious decisions about it. Probably the most famous example of the use of this logic comes from Richard Nelson and Sidney Winter (1982), who found that managers of firms do not really think much about maximizing profit. Nevertheless, they argue, firms behave as though their managers sought to maximize profit, because the firms of those managers who deviate greatly from what they would do if they were maximizing profit go bankrupt. The same logic can be used to explain the prevalence of contiguous territorial states as the main large-scale form of governance in the world today. At one time, many rulers laid claims to noncontiguous pieces of territory, and they did not decide to give up outlying bits in order to concentrate on consolidating their rule in the contiguous areas. Instead, wars, uprisings, and the

1. For an extensive and wonderfully useful discussion of models in the social sciences, see Lave and March (1975).

spread of nationalism led to the consolidation of contiguous states that were relatively large (compared to what preceded them) at the expense of smaller and more scattered ones. In other words, even though rulers may not have consciously sought to limit their domains to contiguous areas, competition among them eliminated noncontiguous areas, which were much more difficult to defend militarily, and allowed the rulers of large contiguous areas to consolidate their territorial claims at the expense of others’.

The two models described here can be applied using only words. There are many others that usually need to be expressed mathematically or graphically because the processes they examine are too complicated to be captured easily by words alone. Widely used models include divide-the-dollar games, which illuminate how different rules and time horizons affect the outcome of bargaining over distribution; signaling models, which describe the effect of costly symbolic actions on the perceptions of others; information cascades, which describe the diffusion of changes in information or perception of risk; spatial models of preferences, used to think about voting behavior, policy choice by legislators, and lots of other issues; prospect theory, which models the effect of prior gains and losses on risk aversion; and contagion models, which can be used to think about anything from the diffusion of technological innovation to the spread of religious fundamentalism. The internal logic of these models is too complicated to be fully and simply articulated in words. In such arguments, equations and graphical representations are used in addition to verbal descriptions as a way of making all aspects of the logic precise and clear.

Even if the student has no interest in becoming proficient in the use of such models, exposure to them enriches the theoretical imagination. It improves the quality of our speculations, which are, in the words of Charles Lave and James March (1975, 2), “the soul of social science.”

A form of hard work that seems to me much less likely to fertilize the soil in which the imagination may grow is the kind of reading that is often considered preparation for qualifying exams. Being able to read the introductions and conclusions of “great books” in order to summarize the main argument in a few sentences is a skill in its own right, a skill often rewarded in graduate school. But it does not seem to be correlated with the ability to do imaginative research.

When one reads, whether in preparation for qualifying exams or not, one should ponder and even brood over the discussions of *why* one thing causes another. This, not the simple identification of cause and effect, is the crux of a work of scholarship, and the reader needs to think about whether it rings true, whether it fits with what he thinks he already knows about the world. If individuals are not the unit of analysis in the argument, it is very useful to think through which individuals would have to be motivated, in what way, and to do what in order for the argument to hold. If individuals are the main actors in the argument, it is useful to ask oneself whether the motivations implied by the argument seem to be plausible accounts of how, on average, people behave.

Readers should also scrutinize the evidence the author offers to support the argument. They should never accept an author's assertion that evidence supports the claims made without looking at the evidence and thinking about it. One cannot assess the evidence supporting arguments without reading the middle parts of books. If one does not have time to read everything one should — and one never does — it is better to read carefully what one can of the evidence than to read only the introduction and conclusion for a summary of the argument.

Although it is all-important to absorb both information about the world and models of how information can be organized and interpreted, this is not enough. Scholars must also constantly, though often implicitly, ask themselves the question, What do I think? Do I believe this? Students cannot develop an autonomous reaction to the world by constantly worrying about what others think. They must worry about what they think themselves, and make sure they think something. The vocation of science is not for the other-directed. The gradual accretion of thoughts entertained in response to information and models will be the basis of one's own creative ideas and scientifically important discoveries.

The Mentor's Role

I turn now to the delicate subject of mentors. Having an apprenticeship relationship with an experienced scholar can be a very useful training experience. The student can learn how a seasoned scholar approaches intellectual puzzles and how to make practical use of the statistical and modeling tools acquired in classes. Typically, an

experienced researcher has figured out, stumbled on, and borrowed lots of tricks and efficiencies over the years that can be passed on to students. The student also gains professional socialization and sometimes a leg up in the job market via coauthored publications. These are the advantages of a close mentoring relationship, and they are very substantial.

The mentoring relationship can be intellectually seductive to the student, however. Graduate students, like the peasants described by James Scott (1976), feel powerless in an unpredictable world. Among other survival strategies, students often attempt to cultivate patron-client relationships with faculty members, who they hope can protect them from the various hazardous forces of nature they face.² In this environment of situationally induced dependence, students may become so imbued with the mentor's worldview and research project that they dismiss evidence that conflicts with the mentor's arguments. Students may even experience something akin to hostage syndrome, in which they come to identify completely with the mentor's point of view, feeling that all the adviser's opponents and all other ways of thinking are wrongheaded or even contemptible. Such narrow-minded partisanship is a rather common but perverse result of the mentoring relationship. Students should guard against it, and mentors should make all possible efforts to limit students' natural impulse toward partisanship. When students rely so heavily on the mentor, they may be unable to conceive of research projects other than subsets of the mentor's research.

Advisers may, through inertia or inattention, seem to want students to defer to all their ideas, but what good advisers really want is for their students to be unafraid to challenge them in sophisticated and well-informed ways. The best scholars are not the best research assistants in graduate school, but rather those who challenge, extend, and go beyond their teachers, and good advisers know that.

By the dissertation stage, a student should be perched on the mentor's shoulder, having absorbed what the mentor has to teach and poised to take off in independent flight. He should not be huddled under the mentor's protective wing. It is part of the mentor's job to push reluctant fledglings out of the nest if necessary.

Just as young people in the West do not allow their parents to

2. Students also make use of the "weapons of the weak" noted by Scott — gossip, slander, ostracism, and shirking — to punish the village notables who fail to perform their allotted roles in the departmental moral economy.

choose mates for them, students should not allow advisers to choose their dissertation topics. They should listen carefully to the adviser's advice, as one listens to parental advice, but ultimately the scholar must feel an intense fascination in order to sustain the commitment needed for such a massive research endeavor. The average comparativist lives with a dissertation topic for between eight and twenty years, from starting to think about it to publication of the dissertation-book and possible spin-offs and extensions. That is as long as many marriages last. Many comparativists continue working on their dissertation subject for the rest of their careers. No one but the person who will be putting in this massive amount of time and effort is really qualified to choose the subject.

Romantic Questions, Reliable Answers

Having allowed passion, fascination, or indignation to influence the choice of topic, the researcher then faces a very different kind of task: devising a research strategy. Many of the classic works in the comparative field focus on big, romantic questions, and the same kinds of questions draw many into the field. The choice of a strategy for investigating such topics requires methodical thought as well as romantic attraction. Outcomes such as democratization, the collapse of empires, and revolution result from the convergence of a number of different processes, some of which may occur independently of others. Insufficient attention to research strategy when approaching such big questions accounts for quite a few sand castles.

Because the complex outcomes are rare and undertheorized, inductive research strategies prevail. Either researchers immerse themselves in the history and social structure of a few cases that have experienced the outcome of interest and come up with a list of events and characteristics that predate the outcome, or they cull indicators of potential causes from large public data sets and plop them into statistical models. Thus, the implicit or explicit model of explanation, even for those who reject quantitative research, turns out to be a kitchen-sink regression. But correlation is not causation, even in nonquantitative research.

At its best, this unstructured inductive approach to investigating complex social outcomes is analogous to that of medical researchers who try to understand the onset of cancer by amassing data on all the dietary and environmental factors that correlate

with an increased incidence of the disease. These studies are useful. They lead to the accumulation of hypotheses, some of which are ultimately confirmed and some not. “But though this sort of fact-collecting has been essential to the origin of many significant sciences,” Thomas Kuhn notes that anyone who examines famous instances of pretheoretic work “will discover that it produces a morass” (1970, 16). It does not by itself lead to an understanding of the process through which cancer develops. For that, researchers have had to step back from the aggregate outcome, the diseased person, and focus instead on basic mechanisms—for example, the factors that regulate cell division and death. They must concentrate on the units within which the process occurs (the cell and the gene) rather than on the outcome (the diseased organism).

In a similar manner, students of comparative politics need to seek to understand underlying political processes rather than to “explain,” in the sense of identifying the correlates of, complex outcomes. What I am proposing here bears a resemblance to the research strategy that Robert Bates et al. have called analytic narratives. I concur with their belief that we need to

seek to locate and explore particular mechanisms that shape the interplay between strategic actors and that thereby generate outcomes. [We need to] focus on the mechanisms that translate such macrohistorical forces into specific political outcomes. By isolating and unpacking such mechanisms, analytic narratives thus contribute to structural accounts. (1998, 12–13)

In order to unpack these mechanisms, we need to focus on the fundamental unit of politics, in most cases individuals. We need to break up the traditional big questions into more precisely defined questions about what individuals do in specific situations that recur often enough to support generalizations about them. I depart from Bates et al., however, in that I see “analytic narratives” as an essential part of the research enterprise, but not its end product. A carefully constructed explanatory argument built up from fundamentals usually has multiple implications, at least some of which are testable. The research effort is not complete until empirical tests have shown that implications drawn from the argument are consistent with reality.

Figuring out the implications of an argument involves repeat-

edly asking, “If this argument were true, what would I see in the real world?” Some scholars seem impelled by intuition to engage in this kind of reasoning, but anyone can train himself to do it as part of a regular routine. To demonstrate deriving implications from an argument, let us use Barrington Moore’s famous aphorism “no bourgeois, no democracy”³ (1966, 418) as a simple example. Since there are no contemporary societies that are literally without a bourgeoisie, and since the aphorism is stated in absolutes but the world is probabilistic, it can be restated in social sciencesese as: “The likelihood of democracy increases once the size of the bourgeoisie has passed a certain threshold.” If *bourgeois* is taken to refer to the commercial and industrial bourgeoisie but not government bureaucrats, the implications of this argument include the following:

- Democracies would not be expected to occur before the industrial and commercial revolutions.
- The establishment of democracies would be expected first in the countries that industrialized first.
- In the contemporary world, democracy would be more likely in more industrialized countries.
- Democracy would be less likely in countries in which wealth comes mainly from the export of mineral resources (because comparative advantage might be expected to reduce industrial investment).
- The likelihood of democracy would decline as state ownership of economic resources rose.
- Democracy would be less likely in countries in which foreigners or pariah capitalists excluded from the political community own most enterprises.

The point of this rather simpleminded exercise is that to test the famous aphorism, one need not count the members of the industrial and commercial bourgeoisie in each country and then correlate the count with the Freedom House democracy scale. Instead of, or in addition to, a direct test of an argument, one can figure out some of its observable implications and test them. Some of the implications of any argument will be consistent with more than one theory, but if enough implications can be drawn,

3. This academic sound bite is Moore’s summary of Marx, not of his own argument. It is useful in the current context because it is so simple, not because it captures Moore’s argument.

not all will be consistent with both the proposed argument and the same rival hypothesis. Although one cannot test all arguments and cannot always reject alternative interpretations for given sets of findings, one can, through tests of multiple implications, build support for a particular causal explanation one brick at a time.

If instead of the aphorism — which is itself an assertion about a correlation — I had used an argument that, like those advocated by Bates et al., showed the moving parts in the causal mechanism, the number of implications would have been multiplied. Implications can be drawn from every link in the logical chain, not just from the hypothesized relationship between initial cause and final effect. Big, romantic, untestable ideas can be made amenable to rigorous investigation by first breaking them up into their component processes and then theorizing these processes one at a time. In the example below, I demonstrate drawing implications from causal mechanisms.

Breaking up the traditional big questions of comparative politics into the processes that contribute to them would make possible the construction and testing of theories. I would not label this shift in the focus of analysis as a move from grand to mid-range theory. A persuasive theory, backed by solid evidence, about one of the several processes that combine to lead to a transformational outcome strikes me as very grand indeed.

An Example of Breaking Up a Big Question into Processes

Abstract methodological prescriptions are rarely compelling or even fully intelligible. In an effort to move from the abstract to the concrete and thus make a more persuasive argument for a change in research strategy, the rest of this chapter focuses on transitions from authoritarianism as an extended illustration of both the problems associated with big questions and the usefulness of disaggregation into multiple processes as a research strategy. It will also demonstrate the leverage that very simple models can bring to bear on a question and show the usefulness of collecting a large mass of information about a subject.

When we read research results in books and journals, we usually see only the finished product reporting the encounter between argument and evidence. Often, however, the most difficult part of research comes before any evidence is collected,

during the stage when the analyst has to figure out how to think about the problem in a fruitful way. This example goes through those initial steps in considerable detail.

I chose transitions as an example because of its normative and academic importance. During recent decades, the last authoritarian holdouts in capitalist Europe, nearly all countries in Latin America and Eastern Europe, and some countries in Asia and Africa have democratized. At the beginning of 1974, the year identified by Samuel Huntington (1991) as the start of the “third wave” of democratization, dictatorships of one kind or another governed 80 countries.⁴ Only 15 of these dictatorships still survived at the end of 2000. During these years, 93 authoritarian regimes collapsed (some countries endured more than one dictatorship during the period). These transitions had resulted in 40 democracies that survived at the end of 2000, some quite flawed but many stable and broadly competitive; 9 democracies that lasted only a short time before being overthrown in their turn; and 35 new authoritarian regimes, 15 of which lasted into the new millennium.⁵ No one knows if these will be the last transitions for these countries, but so far, contrary to initial expectations, new democracies have proved fairly resilient. The study of these transitions has become a major focus of scholarly attention.

Some of the finest minds in comparative politics have worked on this subject. The body of literature on transitions now includes hundreds, perhaps thousands, of case studies of particular transitions, dozens of comparisons among small numbers of cases, and at least half a dozen important efforts at theoretically informed generalizations. A number of descriptive generalizations have become rather widely accepted. One example is the observation that “there is no transition whose beginning is not the consequence — direct or indirect — of important divisions within the authoritarian regime itself” (O'Donnell and Schmitter 1986, 19); a second is that pacts between competing elites facilitate the successful transition to democracy (Karl 1986, 1990; Higley and Gunther 1992).

4. Figures here and elsewhere in this chapter are drawn from a data set I have collected that includes all authoritarian regimes (except monarchies) lasting three years or more, in existence at any time since 1946, in countries with a million or more inhabitants. If monarchies and countries with less than a million inhabitants were included, the number of authoritarian regimes would be larger. See Geddes (1999a) for more details about the data set.

5. Outcome numbers exclude regimes in countries created as a result of border changes during transitions, thus they do not sum to 93.

These and similar inductive generalizations emerging from studies of particular groups of countries have added to our factual knowledge, and they have forced the abandonment of some dearly held preconceptions. These are important advances. Nevertheless, despite the passage of more than twenty-five years since the current wave of democratization began and the sacrifice of whole forests to the production of literature on the subject, few new theories of democratization have been created. When fine scholars—several of whom have in the past constructed theories of great elegance and plausibility—seem to have backed away from theorizing about this topic, it behooves us to think about why.

A part of the difficulty, I believe, stems from certain common choices about research design. Of the fifty-six volumes on transitions reviewed in the *American Political Science Review* between 1985 and 1995, thirty-one were studies of single countries, and many of the others were edited volumes made up of individual case studies of several countries but lacking a theoretical synthesis of the different experiences. In nearly all these books, the cases were selected on the dependent variable; that is, authors sought to explain one or more cases of political liberalization or democratization without comparing them to cases in which change had failed to occur. Many of these studies supply readers with valuable factual information, but the research design chosen prevents their authors from testing their theoretical claims.

Furthermore, in the majority of the studies, the outcome of interest (liberalization, transition, or consolidation) had not yet finished happening when the study was written. The desire of authors to write about the most important political events of the time, and of publishers to publish things at the peak of interest in them, is understandable. This rush to publish, however, has devastating effects on the accumulation of theoretical knowledge. There is no way to test causal arguments if the outcome being explained has not yet happened at the time the study is done. Becoming embroiled in controversies over the causes of something that has not happened is like arguing about what the angels dancing on the head of a pin look like without first having made sure that at least one angel really performs there.

These would be short-term problems, with theories emerging over time, if analysts continued working on the same problems after the outcomes had become clear and if readers treated very tentative conclusions with appropriate skepticism, but most do

not. Around the time it became clear that transitions to democracy really had occurred in a large number of countries, many scholars shifted their attention to trying to explain the consolidation of democracy, which of course had not yet happened. Interest in transitions declined at precisely the time when enough experience had accumulated to make theory building possible.

The rush to publish is not unique to the study of regime change, of course, and thus cannot carry all the blame for its modest generation of theory. A further cause, I suggest, arises from the choice of a compound outcome—that is, an outcome that results from the confluence of multiple causal processes—as the object of study, while maintaining an approach more suited to simple outcomes.

To show exactly what I mean, in the pages that follow I develop a concrete research strategy that begins with the disaggregation of the big question—why democratization occurs—into a series of more researchable questions about mechanisms. The second step is a theorization of the specific process chosen for study—in this case, the internal authoritarian politics that sometimes lead to transition. The third step is the articulation of testable implications derived from the theorization. Decisions about the domains of different testable implications constitute the fourth step. The fifth is the actual discovery or collection of evidence on which to test the implications; the sixth is the testing itself; and the seventh is the interpretation of and response to test results.

What I am aiming for here, and in other examples in this book, is the self-conscious articulation of steps in the research process that, like the values discussed above, occur in the practice of good scholarship but are rarely described in detail. At various points, I shall step back from the description of the steps involved in setting up the research question to comment on why I made certain decisions, to mention where ideas came from, or to reiterate methodological points. The example in this chapter emphasizes steps one through three as outlined in the previous paragraph, leaving detailed discussion of issues involved in testing to later chapters.

Theory-Based Disaggregation

The first issue that confronts the researcher attempting to follow the research strategy suggested here is figuring out how to

disaggregate the processes leading to the compound outcome. There will always be multiple ways to do this, some more fruitful than others. The only general advice that can be given is that the disaggregation should be based on theoretical intuition and that more than one should be tried. The paragraphs below sketch an example of the process involved based on my theoretical intuitions and fairly wide reading about transitions. Another observer's intuitions might be different and at least equally useful.

The Intuition

A regime transition is a change in the basic institutions that determine who will rule, how rulers will be chosen, and how basic distributive decisions will be made. When such a change in institutions occurs as a result of revolution or violent seizure of power, a standard way of simplifying reality for the purpose of theory building is to focus attention on the winning and losing groups in the power struggle, assuming implicitly that institutions chosen will reflect the interests of the winners and that any bargaining that occurs over institutions is bargaining over details among winners. Then, to explain such regime changes, we try to understand why groups concluded that the old regime had become intolerable and how they developed the organizational strength and popular support needed to overthrow it.

Our intuitions about regime change in general seem to derive from observing such forcible seizures of power, but these are not, as it happens, very useful for understanding most transitions to democracy. The breakdown of an authoritarian regime need not lead to democratization, but when it does, the transition involves bargaining and negotiation. Unlike revolutionary victories and authoritarian seizures of power, transitions to more participatory forms of government cannot be accomplished entirely by force, and the institutions that emerge during such transitions reflect compromises among groups, not domination by a single group. Even when the authoritarian regime is overthrown by the military, bargaining is necessary in order to complete the transition to democracy. No single group wins and imposes its institutional choices on all others. Furthermore, this bargaining occurs over a period of time, during which the identity of particular negotiators can change. Institutional changes may be accomplished in increments. It is only at the end of the process that the observer can look at the set of institutional changes and make a judgment

about whether democratization has occurred. In short, bargaining over institutions is a central feature of regime change.

Several different processes can affect this bargaining. Political competition and rivalry within the authoritarian elite can cause splits that may increase the willingness of factions to bargain, as Guillermo O'Donnell and Philippe Schmitter (1986) have noted. Members of the upper class who had initially benefited from regime policies may become critical of later policies or performance and may withdraw their support and their investments, thereby destabilizing the economy and the regime. Economic crisis or some other disaster may push ordinary citizens into clamorous opposition, despite its risks. Such societal changes can strengthen opposition bargainers and weaken elites. Changes in the international economy or the influence of powerful neighbors may alter the cost-benefit calculations of both leaders and led about the feasibility of regime change. Not all these processes will be salient in every transition, but often several of them are. They may interact with each other, but they may also be independent.

The theoretical disaggregation that begins the research strategy should focus on such possibly independent processes identified by the researcher. The disaggregation I suggest places the bargaining over institutions at the center of analysis and seeks to explain how these processes affected bargaining among different actors at different times during the transition.

The Topics

With these ideas in mind, a possible set of topics would include the following:

1. The politics within authoritarian governments, that is, how political rivalries, policy disagreements, and bargaining within different kinds of authoritarian regimes affect the incentives of authoritarian rulers to liberalize⁶

6. Except for discussions of hard-liners and soft-liners who cannot be identified a priori (e.g., Przeworski 1992), this is a topic that received little attention in the early analyses of regime change. Przeworski (1991) has even asserted that characteristics of the old regime do not affect outcomes in the new one. Remmer (1989) and Bratton and van de Walle (1994, 1997), however, have argued that different kinds of authoritarian regimes dissolve in characteristically different ways, which has consequences both for the likelihood of transition and for the kind of regime likely to emerge as a result. For a review of some of these issues, see Snyder and Mahoney (1999).

2. The determinants of upper-class support for authoritarian rule and the effects of loss of such support on bargaining between government and opposition, and hence on regime maintenance⁷
3. The causes and risks of mass expressions of discontent and the influence of mass mobilization on bargaining between government and opposition⁸
4. The effect of the relationship between opposition elites and masses on bargaining between government and opposition⁹
5. The relationship among (a) the timing of institutional choices, (b) the interests of the bargainers at particular times, and (c) extent of democratization¹⁰
6. The relationship between economic modernization and citizen influence on regime choice¹¹
7. The effect of international economic and geopolitical shocks on the decisions and actions of regime leaders, regime supporters, and ordinary citizens

7. Many case studies note the fickleness and ingratitude of bourgeois and other upper-class supporters of authoritarian regimes, along with the role these groups have played in opposition to authoritarian governments. Cardoso's study (1986) of the Brazilian bourgeoisie during democratization is one of the earliest and most insightful.

8. Many case studies describe the effect of demonstrations and other mass actions on the decisions of authoritarian rulers. In addition, several authors have emphasized the importance of popular opposition in bringing about transitions (e.g., Bratton and van de Walle 1997; Casper and Taylor 1996; Collier 1999; Collier and Mahoney 1997; Bermeo 1997). These studies are largely descriptive, however. Though initial theoretical steps have been taken from several different directions to account for why large numbers of people, after having suffered oppression and poverty for long periods of time, suddenly rise up to voice their indignation (Przeworski 1986; Geddes and Zaller 1989; Lohmann 1994), much more work remains to be done. Furthermore, to my knowledge, no one has offered a compelling explanation of why authoritarian regimes sometimes respond with coercion to mass protests and at other times hasten to compromise.

9. It should be possible to extend work on nested games (Tsebelis 1990) to deal with this subject, though adaptation will be required to accommodate the institutional fluidity characteristic of transitions.

10. Much of the work on this subject has focused on pacts (e.g., Higley and Gunther 1992; Karl 1986, 1990). This topic has only begun to be more fully and systematically explored (e.g., Przeworski 1991; Geddes 1995, 1996; Mainwaring 1994).

11. The correlation between economic development and democracy is one of the best established in comparative politics (Bollen 1979; Bollen and Jackman 1985; Przeworski et al. 2000; Burkhart and Lewis-Beck 1994; Barro 1999). The causes of this relationship, however, continue to be debated.

Though different researchers would break up the big question in different ways, any disaggregation into constituent processes should have some of the characteristics of the topics on this list. Each topic is posed as a general comparative question. We would hesitate to propose an argument about any of these topics based on the experience of only one country. Research on these topics seems, on the contrary, to demand comparison across cases. None of these topics imply selection bias, that is, none imply limiting studies to those countries that have completed transitions. All governments face opposition, and the absence of bargaining in particular times and places requires explanation; it is not a reason to exclude cases from examination. I will return to issues related to appropriate selection of cases in later chapters. For now, the important thing to note is that each topic listed here is worthy of a project in itself. When processes are described separately in this way, it becomes clear why it might be difficult to theorize transitions as a whole.

Some of these topics, especially 2 and 3, have received considerable attention in the case study literature. The next step in developing research strategies to investigate them would be to build theories that subsume and explain the observations made in the case studies. In the extended example below, I examine the first topic, to which somewhat less attention has been paid. I propose an argument about the incentives facing leaders in different kinds of authoritarianism that helps to explain, first, why some authoritarian governments initiate liberalization when they face little societal pressure to do so; and, second, why and when the factions that always exist within dictatorships may contribute to democratization. This argument thus offers an explanation for two elements in the process of regime change that a number of studies note without explaining, but it does not try to account for the final outcome of democratization itself.

A Theorization of One Process: Politics in Authoritarian Regimes

O'Donnell and Schmitter's observation (1986) about the importance of splits within authoritarian governments, noted above, alerts us to the importance of individuals near the center of power during the transition process. Although political factions and disagreements can be found within any authoritarian

government,¹² *every* transition is not actually a *consequence* of “important divisions within the authoritarian regime itself” (O’Donnell and Schmitter 1986, 19). The Salazar-Caetano dictatorship in Portugal did not fall as a result of internal splits (though such splits existed, of course), unless that regime is defined as including the midlevel military officers who had spent most of their careers in Africa and who forcibly overthrew the dictatorship. Arguably, a number of other dictators (such as Somoza in Nicaragua and the Shah in Iran) fell not because of divisions within the regime itself—meaning splits among those with decision-making power—but because of desertions from the societal and military coalition originally supporting them. And, though O’Donnell and Schmitter had no way of knowing it at the time they wrote, the collapses of communist regimes in countries such as Bulgaria and East Germany were not caused by splits within the regime, unless those regimes are defined as including not only high officials of the Bulgarian and East German communist parties but also their Soviet allies. Nor, according to Michael Bratton and Nicolas van de Walle (1992, 1997), has the initiation of liberalization in many African countries been a consequence of splits internal to regimes.

It is nevertheless true that in a large number of the recent transitions from authoritarianism, the initial steps toward what became democratization were taken by those in power, for reasons internal to the ruling elite rather than in response to pressure from either supporters or opponents in the larger society. The observation of this pattern in a number of cases surprised observers who were accustomed to thinking of institutional changes as consequences of power shifts, not as the causes of them.

One of the reasons that regime transitions have proven so theoretically intractable is that different kinds of authoritarianism break down in different ways. The beginnings of some can be traced to splits within the regime, but others begin in other ways. Dictatorships can differ from each other as much as they differ from democracy, and these differences affect the way they collapse. They draw on different groups to staff government offices and on different segments of society for support. They have different procedures for making decisions, different characteris-

12. Numerous descriptions exist of factionalism within authoritarian regimes in every region of the world; see, for example, Stepan (1971); Fontana (1987); Sandbrook (1986); and Waterbury (1973).

tic forms of intraelite factionalism and competition, different ways of choosing leaders and handling succession, and different ways of responding to societal interests and opposition. Because analysts have not studied these differences systematically, what theorizing exists about authoritarian regimes is posed at a highly abstract level, and few authors have considered how characteristics of dictatorships affect transitions. These differences, however, cause authoritarian regimes to break down in systematically different ways, as I show below.

To explain the first incremental institutional changes that set some countries on the path toward democratization, we need a theory of politics within authoritarian regimes. Where do we get one? Standard theories of politics in democratic regimes begin with two simplifying assumptions: first, that officials want to remain in office; second, that the best strategy for doing so is to give constituents what they want. Much of the literature on democratic politics concerns how different political institutions affect the survival strategies of politicians. The analysis of transitions requires an analogous investigation of the effects of differences among various kinds of authoritarian institutions.

To begin the task of investigating the effects of authoritarian institutions, we need first to assess the plausibility of the standard assumptions, and then, possibly, to revise them. Most obviously, in the absence of routine ways for citizens to remove authoritarian leaders from office, empirical investigation is needed to answer questions about who exactly the constituents of dictators are, how satisfied they have to be, and what factors besides satisfaction with regime performance affect constituents' acquiescence. These questions cannot be answered in the abstract, nor can answers be assumed, as in the study of democratic politics. Topics 2, 3, 6, and 7 as outlined above deal with these issues.

Less obviously, it should not be assumed that the officers, parties, and cliques supporting authoritarian leaders always want to remain in power. Military officers, in contrast to cadres in single-party and personalist regimes, may not want to. If there are circumstances in which they can achieve their ends better while out of power, as I will argue there are, then we can expect them to return voluntarily to the barracks. Furthermore, the costs of leaving office vary for different kinds of authoritarian leaders. Military officers can return to the profession that called them in the first place, usually without suffering punishment for actions while in office. Cadres in single parties lose their monopoly on the

advantages of office, but they also usually remain free to compete for office after a transition and thus to continue their chosen profession. The allies of a personalist leader, however, generally find it hard to continue the life to which they have become accustomed. Compared with other kinds of authoritarians, they are more likely to lose the opportunity for future office, and possibly also their property and lives, in the wake of a transition.

To begin building an understanding of authoritarian politics, I focus on rivalries and relationships within the entity from which authoritarian governments are drawn: the officer corps, the single party, the clique surrounding the ruler, or some combination of these. Most of the time, the greatest threat to the survival of the leader in office — though not necessarily to the survival of the regime — comes from within this ruling group, not from outside opposition. In normal times, most of what we would call politics, namely, the struggle over office, spoils, and policy decisions, takes place within this ruling group.

Politics within the ruling group tells only part of the story of regime change, but it is a part about which we understand little. Opposition from outside the ruling coalition and exogenous shocks, such as the Soviet collapse, the international economic crisis of the 1980s, and the economic reforms induced by that crisis, have affected regime survival, sometimes decisively. By focusing on the political dynamics within different kinds of authoritarian regimes, however, I aim to show why some forms of authoritarianism are more vulnerable than others to exogenous shocks and popular opposition.

The Classification of Authoritarian Regimes

Before we can use differences among authoritarian regimes as the basis for elaborating theoretical arguments about the consequences of these differences, we need to develop a simplifying classificatory scheme of regime types and clear criteria for assigning cases to categories. Without this kind of simplification of reality, we would be inundated by complexity and unable to see the patterns underlying it. The aim here is to “carve nature at its joints,” that is, to find the places in the complicated whole at which elements seem to divide naturally. As with carving a chicken, we must know a fair amount about the basic structure of the beast in order to find the right places to hack. Because I consider the most important differences among authoritarian re-

gimes to be qualitative, I create a typology for “measuring” regimes rather than a scale or index. Typologies are theoretical constructs used when variables can only be measured nominally. Like other theoretical constructs, they are useful or not useful rather than true or false. To be useful, they have to capture differences that are essential to the argument being made.¹³

In this section, I discuss the bases for assigning regimes to one category or another.¹⁴ I initially classified regimes as personalist, military, or single-party. In military regimes, a group of officers decides who will rule and exercises some influence on policy. In single-party regimes, one party dominates access to political office and control over policy, though other parties may exist and compete as minor players in elections. Personalist regimes differ from both military and single-party in that access to office and the fruits of office depend much more on the discretion of an individual leader. The leader may be an officer and may have created a party to support himself, but neither the military nor the party exercises independent decision-making power insulated from the whims of the ruler (cf. Bratton and van de Walle 1997, 61–96; Chehabi and Linz 1998, 4–45; and Snyder 1998). I had to add intermediate categories to this classification scheme after discovering how many of the cases simply resisted being crammed into one or another of the original categories.

My initial guess about what kind of classification would best capture the important differences among authoritarian regimes grew out of reading about many such regimes. Let me again emphasize the importance of collecting information about a wide range of cases. Although I had to hunt for information in a much more systematic way further on in the research process, the initial ideas that motivated this study came from reading military sociology and descriptions of events in many countries during transitions just because I was curious.

In this classification scheme, a military regime, in contrast to a personalist regime led by a military officer, is one in which a group of officers determines who will lead the country and has some

13. Typologies have been much and justly maligned in the comparative field because their creation was at one time seen by some as an end in itself, and scholars used to waste their time comparing them and arguing about them. They have a useful role, however, as a way of categorizing causes and effects that cannot be measured using numbers.

14. The classification of individual cases is a “measurement” issue. Measurement will be discussed later in this chapter and in chapter 4. Here I am concerned with creating an overall “coding scheme.”

influence on policy. In an institutionalized military regime, senior officers have agreed upon some formula for sharing or rotating power, and consultation is somewhat routinized. Examples of military regimes include that of Brazil (1964–85), in which senior officers, in consultation with a small number of civilian allies, picked each successive president in keeping with rules specified by the institutions of the authoritarian regime; and that of Argentina (1976–83), in which, despite intense factional struggle and the efforts of some military presidents to renege on precoup agreements establishing an elaborate arrangement for consultation and predictable rotation in office, senior officers did not permanently lose control of succession and policy.

Many regimes headed by a military officer are not, however, really controlled by a group of senior officers. It is common for military interventions to lead to short periods of collegial military rule followed by the consolidation of power by a single officer and the political marginalization of much of the rest of the officer corps. These are personal dictatorships, even though the leader wears a uniform. Regimes such as that of Rafael Trujillo in the Dominican Republic (1930–61) and Idi Amin in Uganda (1971–79) are somewhat extreme instances of the transformation of a military intervention into personal tyranny. Other regimes, such as that of Augusto Pinochet in Chile (1973–89) and Sani Abacha in Nigeria (1993–99), are harder to classify; the military institution retained some autonomy and influence, but the concentration of power in the hands of a single man prevents them from being categorized simply as military.¹⁵ I classify regimes on the margin between the two categories as military-personalist hybrids.

Since most dictators form parties to support themselves, distinguishing between “real” and nominal single-party regimes involves the same difficulties as distinguishing between military regimes and personalist regimes led by military officers. In real single-party regimes, a party organization exercises some power over the leader at least part of the time, controls the selection of officials, organizes the distribution of benefits to supporters, and mobilizes citizens to vote and show support for party leaders in other ways. Examples of single-party regimes include that of the Institutional Revolutionary Party (PRI) in Mexico, the Revolutionary Party of Tanzania (CCM), and the Leninist parties in

15. This classification of Pinochet is supported by Remmer’s analysis (1989) and by Huntington (1991). The classification of Abacha is supported by Obasanjo (1998).

various East European countries. Regimes in which the leader himself maintains a near monopoly over policy and personnel decisions despite the existence of a support party — such as those led by Manuel Odría in Peru and Etienne Eyadema in Togo — are personalist.

Personalist dictators range from vicious psychopaths to benevolent populists. Institutionally, what they have in common is that although they are often supported by parties and militaries, these organizations have not become sufficiently developed or autonomous to prevent the leader from taking personal control of policy decisions and the selection of regime personnel. The fear of potential rivals leads such rulers to undermine these and other institutions that might serve as power bases for potential challenges. They rely instead on informal, and sometimes quite unstable, personal networks — sometimes based on kinship, ethnicity, or region — within which particularistic favors are exchanged for support. Typically, regime personnel are rotated frequently to prevent them from developing autonomous bases of support, and erstwhile supporters who become rivals or dissidents are quickly and unceremoniously deprived of office, influence, and sometimes their lives (cf. Bratton and van de Walle 1994).

Leaders' Interests and Intraregime Politics

In order to build a theory about particular actors, one must first have some knowledge about their goals. I have argued that the goals of leaders in different kinds of authoritarian regimes typically differ from each other. In this section, I discuss their different interests and the evidence supporting my assessment of these interests.

The dictator who leads a personalist regime after having clawed his way to the top in intense and often deadly struggles among regime insiders can reasonably be assumed to have a strong and abiding determination to remain in office. No similar assumption can be made, however, about most of the officials of military regimes. Some individual leaders, especially those who have managed to scramble to the very top during the early chaos of military takeovers, undoubtedly feel as intense a desire to remain there as any other leader, but many officers do not. The discussion below describes the interests of members of the primary supporting institution or informal group in each type of regime, starting with military.

Research on the attitudes and preferences of military officers in many different societies finds that officers in different countries come from different socioeconomic, ethnic, and educational backgrounds. They have different ideologies and feel sympathetic toward different societal interests. No generalizations can be made about the societal interests or policies they are likely to support. According to the scholarly consensus, however, most professional soldiers place a higher value on the survival and efficacy of the military itself than on anything else (Janowitz 1960, 1977; Finer 1975; Bienen 1978; Decalo 1976; Kennedy 1974; Van Doorn 1968, 1969).

This corporate interest implies a concern with the maintenance of hierarchy, discipline, and cohesiveness within the military; autonomy from civilian intervention in postings and promotions; and budgets sufficient to attract high-quality recruits and buy state-of-the-art weapons. Officers also value their nation's territorial integrity and internal order, but the effective pursuit of these goals requires unity, discipline, and adequate supplies (Stepan 1971; Nordlinger 1977; Barros 1978). Such preferences might result from socialization in military schools (Stepan 1971; Barros 1978) or from a rational calculation of the effect of the health of the military institution on the officer's own career prospects. For the purposes of this study, the source of these preferences does not matter.

In countries in which joining the military has become a standard path to personal enrichment (as, for example, during some time periods in Bolivia, Nicaragua, Nigeria, Thailand, Indonesia, and the Congo), acquisitive motives can be assumed to rank high in most officers' preferences. Such motives will occupy first place for some officers and rank second or third for others, if only because the continued existence of lucrative opportunities for officers may depend on the survival of the military as an effective organization. Where acquisitive motives have swamped concern for corporate survival and effectiveness, however, the professionalism of the military deteriorates, and the officer corps is less likely to serve as a successful counterweight to ambitious political leaders.

Where corporate interests prevail, most officers agree to join coup conspiracies only when they believe that the civilian government prevents the achievement of their main goals. Many officers, in fact, will join only if they believe that the military institution itself is threatened. These preferences are consistent with

the observations by Alfred Stepan (1971) and Eric Nordlinger (1977) about the importance of threats to the military as an institution in the decisions of officers to join coup conspiracies. They are also consistent with the observation that coups do not usually occur in fully professionalized armies until a consensus exists among senior officers (Stepan 1971; Valenzuela 1978), since the worst possible outcome for the military as an institution is a civil war in which part of the military fights on each side.

Consequently, the most important concern for many officers in deciding whether to join a coup conspiracy is their assessment of how many other officers will join. What Nordlinger, Stepan, and others are describing resembles a classic battle-of-the-sexes game. The insight behind this game comes from the following scenario: One member of a couple would prefer to go to a movie and the other would prefer the symphony, but each would prefer doing something together to doing something alone. Going to either event together is a potential equilibrium, but no dominant strategy exists, since the best outcome for either player always depends on what the other chooses.

The logic of decisions about seizing power or returning to the barracks is the same. Some officers always want to intervene, others have legalist values that preclude intervention except in the most extreme circumstances, and most are located somewhere in between—but almost all care most about the survival and efficacy of the military and thus want the military to move either in or out of power as a cohesive whole. Figure 2.1 depicts this set of preferences as a game.

In the figure, the two numbers in each cell represent the respective payoffs to the two factions, the first number being the payoff for the majority faction and the second number the payoff for the minority faction.¹⁶ In the game depicted, the majority prefers that a united military remain in the barracks. The payoffs to both factions for remaining in the barracks are shown in the lower right cell. The upper left cell shows the payoffs for a successful intervention carried out by a united military. The minority is better off than it was in the barracks, but the majority is slightly worse off since it would have preferred not to intervene.

The minority faction prefers to intervene, but it would be far worse off if it initiated an unsuccessful coup without support

16. I have used numbers in this and other matrices because I think they are easier to understand. The specific numbers used here, however, have no meaning. The logic of the game would be the same for any numbers that maintained the same order.

		Minority Faction	
		intervene	barracks
Majority Faction	intervene	4, 5	2, -10
	barracks	3, -10	5, 4

Fig. 2.1. Game between military factions

from the majority than if they remained unhappily in the barracks (payoffs for this outcome are shown in the lower left cell).¹⁷ Participants in an unsuccessful coup attempt face possible demotion, discharge, court-martial, or execution for treason, so their payoff is shown as negative. The majority faction that opposed the coup is also damaged by the attempt, since the armed forces are weakened and the government is likely to respond with greater oversight, reorganization, and interference with promotions and postings to try to ensure greater future loyalty, all of which reduce military autonomy.

The final possible, though unlikely, outcome is a successful coup carried out despite minority opposition (payoffs are shown in the upper right cell).¹⁸ In this event, the minority that remains loyal to the ousted civilian government is likely to face the same costs as unsuccessful conspirators: demotion, discharge, exile, prison, death. The winners achieve power, but a weakened military institution reduces their chances of keeping it. Future conspiracies supported by those demoted or discharged after the coup become more likely. Once factions of the military take up arms against each other, it takes years or decades to restore unity and trust.

This is a coordination game: once the military is either in power (upper left cell) or out of power (lower right cell), neither

17. The use of *majority* and *minority* here is not meant to imply that the success of coup attempts is determined by which side has the most support. Support affects the likelihood of success but is by no means decisive. For ease of exposition, however, I describe the majority faction as successful if it attempts a coup and the minority faction as unsuccessful. A more realistic game would introduce uncertainty about the likelihood of a successful coup, but the payoffs in the off-diagonal cells would remain lower for both actors than those in the upper left and lower right cells regardless of the outcome of the coup attempt.

18. Since the majority prefers not to intervene, it is hard to imagine anything other than profound misinformation that would lead to this outcome. Even if the majority preferred intervention, however, their payoff for intervening without full support would be lower than for remaining in the barracks.

faction can improve its position unilaterally. Each faction must have the other's cooperation in order to secure its preferred option. When the military is out of power, even if the majority comes to believe it should intervene, it cannot shift equilibria without cooperation from the minority.

There are two ways to solve coordination problems: one is to negotiate until consensus is reached, and the other is to make a credible first move that confronts the second mover with the choice between joining the first mover or receiving the payoff associated with a divided choice. Some military decisions to seize power have been carefully negotiated over a period of months until rules for sharing power have been hammered out and the last legalist holdout has either given in or retired. Such negotiated interventions occurred in Argentina in 1976, Brazil in 1964, and Chile in 1973 (Fontana 1987; Stepan 1971; Valenzuela 1978). Since extended negotiations carry considerable potential for discovery, however, most military interventions have employed a first-mover strategy in which a small group of conspirators seizes the presidential palace, the airport, television and radio stations, military installations in the capital, and perhaps a few other key buildings. It then announces that it has taken power and counts on the rest of the armed forces to go along (Nordlinger 1977). They usually do — but not always.

For the first-mover strategy to work, the first move has to be credible, meaning that other officers have to be convinced that the seizure of power is irreversible. The attempted coup of 1981 in Spain is an example of a failed first-mover strategy. The coup plotters seized the requisite number of installations in Madrid and had reason to believe that garrison commanders in the rest of the country would go along with them. King Juan Carlos, however, immediately began telephoning the garrison commanders, telling them that he opposed the intervention and that if they joined it, they would be guilty of treason. He also went on television to rally citizens against the coup. Once the king had taken such a strong stand, the first move lost its credibility, and most of the military refused to go along. Josep Colomer reports that one of the coup conspirators, when interviewed later, said, "Next time, cut the king's phone line" (1995, 121). Colomer suggests that had the king not been able to use television and the phone to rally support, the first-mover strategy might well have worked, because many in the officer corps sympathized with the goals of the conspirators (110–23).

When the military controls the government, the logic remains the same. Most officers will go along with a credible move by one faction to return to the barracks. Strong disagreements among leading officers over how to respond to economic difficulties or who among them will next occupy the presidency lead to intense factionalization. When this happens, one group is likely to prefer returning to the barracks as a way of avoiding institution-damaging conflict. Observers see splits in the officer corps at the time of the first moves toward democracy because the concern over divisions within the military causes some factions to prefer a return to the barracks. Both hard-line and soft-line factions can use the first-mover strategy, however. Military presidents can make quite credible first moves heading back toward the barracks, and most officers will go along. Hard-liners can also chance first-mover strategies, ousting more moderate military presidents. Again, if the move is credible, most of the officer corps will go along.

For the officer who ends up as paramount leader of the post-coup junta, the game may change after a successful seizure of power, as it did for Pinochet and those like him in other countries who sought to concentrate power in their own hands; but other officers usually see their situation as resembling a battle-of-the-sexes game, even in the most politicized and factionalized militaries. Repeated coups by different factions, as occurred in Syria prior to 1970 and Benin (then called Dahomey) before 1972, would not be possible if most of the army did not go along with the first mover, either in seizing power or in handing it back to civilians.

This analysis demonstrates the usefulness of having some simple models in one's theoretical toolkit. The military's concern about professional unity has been described verbally by a number of scholars, but using the game to show the logic of the situation demonstrates the consequences of this concern in a very clear and stark manner. The comparison between this game and the ones to be developed below will show the effects of differences in the interests of cadres in different kinds of regimes more clearly than could a verbal comparison alone. These models are not, of course, the endpoint of the analysis. After the models have been described, they will need empirical confirmation.

The preferences of party cadres are much simpler than those of officers. Like democratic politicians, party cadres simply want to hold office. Some value office because they want to control

		Majority (Leader's Faction)	
		in office	out of office
Rival Faction	in office	8, 10	5, 1
	out of office	3, 9	0, 0

Fig. 2.2. Game between factions in single-party regimes

policy, some for the pure enjoyment of influence and power, and some for the illicit material gains that can come with office. The game between single-party factions is shown in figure 2.2. The insight behind this game is that everyone's cooperation is needed in order to achieve a desired end, and no one can achieve it alone. In this game, no one ever has an incentive to do anything but cooperate to remain in office.

In this game the best outcome for everyone is for both the majority faction and the rival faction to hold office (payoffs are shown in the upper left cell). The worst outcome occurs when both are out of power (shown in the lower right cell). The upper right cell shows the payoffs when the party has lost control of government but the minority faction still fills some seats in the legislature or holds other offices as an opposition to the new government. The minority's payoff when in opposition is lower than when its party holds power because the opposition has fewer opportunities to exercise influence or line pockets. In the lower left cell, the minority faction is excluded from office, but the dominant faction of the party still rules. In this case, the minority continues to receive some benefits, since its policy preferences are pursued and party connections are likely to bring various opportunities, but members of the excluded minority receive none of the specific perquisites of office. The majority is also worse off, because exclusion gives the minority an incentive to try to unseat the majority. Combatting the minority is both risky and costly for the majority.

Factions form in single-party regimes around policy differences and competition for leadership positions, as they do in other kinds of regimes, but everyone is better off if all factions remain united and in office. This is why co-optation rather than exclusion characterizes established single-party regimes. Neither faction would be better off ruling alone, and neither would voluntarily withdraw

from office unless exogenous events changed the costs and benefits of cooperating with each other (and hence changed the game itself).¹⁹

In contrast to what happens in military and single-party regimes, the political fate of the close allies of a personalist dictator is tied to the fate of the dictator himself. “[I]nsiders in a patrimonial ruling coalition are unlikely to promote reform. . . . Recruited and sustained with material inducements, lacking an independent political base, and thoroughly compromised by corruption, they are dependent on the survival of the incumbent” (Bratton and van de Walle 1997, 86). Personalist dictatorships rarely survive for long after the death or ouster of the dictator, perhaps because dictators, in their efforts to defend themselves from potential rivals, so assiduously eliminate followers who demonstrate high levels of ability and ambition.

In personalist regimes, one leader dominates the military, the state apparatus, and the ruling party, if there is one. Because so much power is concentrated in the hands of this one individual, he generally controls the coalition-building agenda. Consequently, the game between factions in a personalist regime must be depicted as a game tree instead of a two-by-two matrix in order to capture the leader’s control over first moves.²⁰ As shown in figure 2.3, the leader’s faction has the initiative, choosing to share the spoils and perks with the rival faction or not. The choice I have labeled “hoard” can be interpreted either as limiting the opportunities and rents available to the rival faction or as excluding some of its members altogether. In the example shown in this figure, the amount of hoarding is small (the payoff to members of the rival faction for continued cooperation despite hoarding by the ruler’s faction is 6); perhaps members of the rival faction are not offered the choicest opportunities, or perhaps a few of its members are jailed but the rest continue to prosper. If the whole rival faction were excluded from all benefits, their payoff for continued co-

19. The economic shocks of the 1980s and 1990s changed these costs and benefits in many countries, reducing the incentive of potential rivals to cooperate with ruling-party leaders and thus destabilizing regimes. The game used here shows the incentives to cooperate during good times. A different game would be needed to capture the choices facing single-party cadres after serious exogenous shocks.

20. Two-by-two matrices, often used to depict simple prisoner’s dilemmas, battle-of-the-sexes games, chicken games, and so on, assume simultaneous decisions by the players or a lack of information about how the other has chosen. More complicated games, including those in which one player chooses first and the second chooses knowing how the first has chosen, have to be depicted using a game tree or equations.

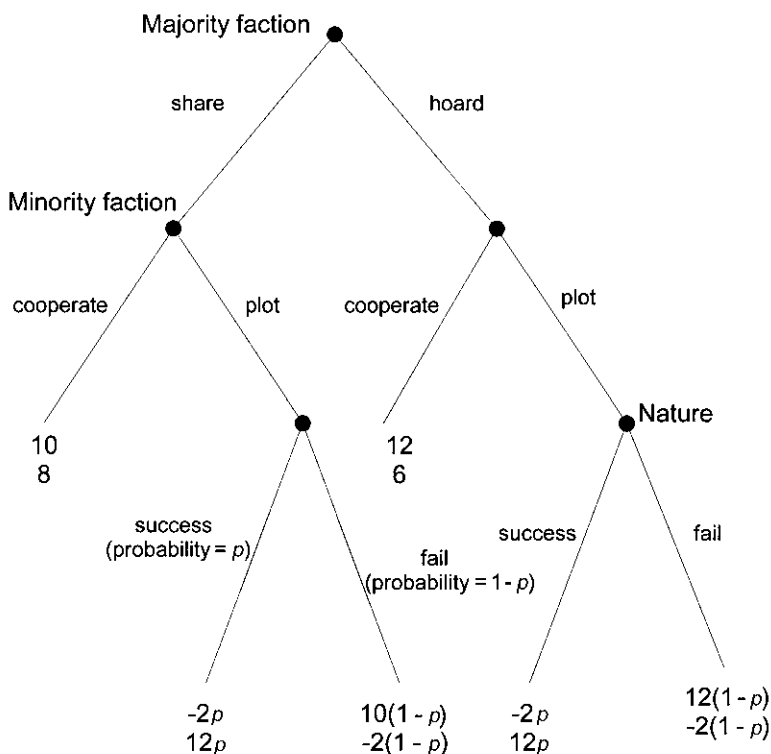


Fig. 2.3. Game between factions within personalist clique

operation would be much lower, but rarely lower than the payoff for refusing to cooperate.

After the leader's faction has chosen its strategy, the rival faction must decide whether to continue supporting the regime or not. During normal times, it has strong reasons to continue. Because its members "face the prospect of losing all visible means of support in a political transition, they have little option but to cling to the regime, to sink or swim with it" (Bratton and van de Walle 1997, 86).

Unlike in single-party regimes, the leader's faction in a personalist regime may actually increase benefits to itself by excluding the rival faction from participation. Where the main benefits of participation in the government come from access to rents and illicit profit opportunities, the payoff to individual members of the ruling group may be higher if these benefits need not be shared too widely. It may also be easier to keep damage to the economy below the meltdown threshold, and thus increase

the likelihood of regime survival, if the predatory group is relatively small. Hoarding by the leader's faction is thus likely. If the hoarding is not too extreme, as in figure 2.3, the rival faction is better off continuing to cooperate, and most of the time that is what they do.

If the rival faction withdraws its support and begins to plot the leader's overthrow, its members risk life, liberty, and property. The rewards of a successful overthrow are high, but so are the costs of detection, betrayal, or defeat. In the game, the uncertainty over the outcome of plots is shown as a play by Nature. The plot succeeds with probability p , usually a low number, and fails with probability $1 - p$. The rival faction decides whether to continue its support for the leader's faction by comparing its payoff for support with its expected payoff from a plot. Two considerations thus affect the choice: the benefits being derived from the status quo and the potential plotters' assessment of the risk of plotting. As long as the personalist ruler seems powerful enough to detect plots and defeat coup attempts, the rival faction will continue to cooperate if it gets some benefits from the regime. The leader's faction has an incentive to reduce the benefits to the rival faction to a level just above that needed to prevent plotting. This system is very stable as long as the ruler can distribute the minimum level of benefits needed to deter plotting and can maintain control over an effective security apparatus and loyal military. The situations in which these conditions become less likely are discussed below.

By drawing on some rudimentary game theory, I have begun to develop insights into how the interests of cadres in different kinds of authoritarian regimes might play out in different contexts and how resilient to stress the cadres' loyalties to regime leaders might be. These insights motivate the analysis in the next section.

The Consequences of Differences in Interests

The interests described above provide a starting point for figuring out whether the splits and rivalries that exist within all kinds of governments will lead to regime breakdown. Because most military officers view their interests as following a logic similar to that of a battle-of-the-sexes game, they acquiesce in continued intervention regardless of whether military rule becomes institutionalized, the leader concentrates power in his own hands, or a

rival ousts the original leader. The officer corps will not, however, go along with disintegration of the military into openly competing factions. If elite splits threaten military unity and efficacy, some factions will opt for a return to the barracks. If the soft-line faction can make a credible first move in that direction, most other officers will go along.

Military regimes thus contain the seeds of their own destruction. When elite rivalries or policy differences intensify and these factional splits become threatening, a return to the barracks becomes attractive to most officers. For officers, there is life after democracy, because all but the highest regime officials can usually return to the barracks with their status and careers untarnished and their salaries and budgets often increased by nervous transitional governments (Nordlinger 1977; Huntington 1991).

Leaders of single-party regimes also face competition from rivals, but most of the time, as in personalist regimes, the benefits of cooperation are sufficient to ensure continued support from all factions. Leadership struggles and policy disagreements occur, but they do not affect the desire of most cadres to remain in office. For them, life after democracy would require some unpleasant changes in lifestyle. They would have to compete for the benefits they have become accustomed to monopolizing. During leadership struggles, most ordinary cadres just keep their heads down and wait to see who wins. Thus, leadership struggles within single-party regimes usually do not result in transitions.

The close allies of personalist dictators have even less reason to desert the ship in normal times. If the ship goes down, they are likely to go with it. As long as the dictatorship is able to supply some benefits and has a sufficiently competent repressive apparatus to keep the probability of successful plotting reasonably low, they will remain loyal.

These differences explain why the early transitions literature, drawing insights primarily from the transitions from military rule in Latin America, emphasized splits within the regime as causes for the initiation of democratization though later studies did not. In other parts of the world, where rule by the military as an institution is less common, factions and splits could be identified within authoritarian regimes, but they did not result in transitions. Instead, observers emphasized the importance of other factors in bringing down long-standing dictatorships: economic crisis (Haggard and Kaufman 1995), foreign pressure (Huntington 1991), and popular protest (Bratton and van de Walle 1992,

1997; Casper and Taylor 1996). In short, the theorization of intra-authoritarian politics makes it possible to subsume the findings of a number of studies with differing regional foci. In regions where the military led most of the authoritarian regimes that broke down, the first steps toward democratization could be traced to splits within the military leadership, but where single parties or personalist autocrats tended to rule, pressures of various kinds external to the ruling party or clique played larger roles.

The many studies of transitions, most of which draw essential insights primarily from one part of the world, bear some similarity to the parable about five blind men encountering an elephant. Each offers a useful and insightful description of the part of the elephant he touches, but cannot describe the whole. The early studies could not do so because they were trying to explain a process that had barely gotten under way, though of course they had no way of knowing how many countries democratization would eventually affect. Later studies either have made no attempt to survey all cases or, in their attempt to set their own region in the broader context, have misinterpreted studies of some of the most frequently examined cases in particular regions as being representative of the general experience of that region. To repeat two of the most basic pieces of advice in this book: lots of factual information is always good; and it is hard to explain an outcome that has not yet finished coming out.

Drawing Testable Implications from the Argument

In this section, I detail the derivation of testable implications from the analytic argument above. As is often the case in comparative politics, it is not feasible to test in a rigorous way the argument about cadre interests proposed here. To gather the necessary detailed information about the internal politics of a large number of authoritarian regimes would require learning many languages and traveling to many places. Although numerous books and articles have been written about authoritarian governments in the larger, more developed, and for other reasons more “interesting” countries, it is difficult to find even detailed descriptions of events in smaller, less developed countries such as Burkina Faso, Niger, and Laos, especially those in which democratization has not taken place. In situations like this, one must rely on tests of the *implications* of the argument, which can sometimes be done with less

detailed information than would be needed to test the argument itself. Testing implications in this kind of situation can make it possible to avoid the selection bias that would almost inevitably arise in an attempt to test the argument itself.

The argument sketched above claims that because officers see their interests in terms similar to a battle-of-the-sexes game, military regimes break down more readily than do other types of authoritarianism in response to internal splits, regardless of the cause of the splits. If that is true, we should expect military regimes to last less long, on average, than other forms of authoritarianism.

We should also expect economic crisis, which weakens support for all governments, to have a stronger disintegrating effect on military governments because of their underlying fragility. This suggestion might at first seem surprising, since most military governments hold no elections and tend to be more insulated from societal interests than other types of dictatorship. Thus, we might suppose them less vulnerable to pressures emanating from citizens unhappy with the regime's economic performance.

The cadre-interests argument, however, implies that officers may decide to step down even without the inducement of overt public pressure. Officers and cadres are aware of their government's economic performance, and they are linked to society via their families and friends. Typically, when officers perceive their government's performance as unsuccessful, some of them advocate intensifying the economic strategy being pursued while others advocate changing it. The backers of each policy prescription support the presidential aspirations of a different officer, and competition between them intensifies, sometimes leading to coups and countercoups. A split over economic strategy has the same effect as any other kind of split: if it threatens to get out of hand, most officers prefer to return to the barracks.

Observers such as Bratton and van de Walle (1997) note the importance of material inducements to loyalty in personalist regimes. We might suspect that where loyalty depends on the leader's ability to deliver individual benefits, economic crisis would cause regime breakdown, but that would be an insufficiently cynical view. Run-of-the-mill poor economic performance hurts ordinary citizens but does not preclude rewarding supporters. It takes a true economic disaster to do that. We should thus expect personalist regimes to be destabilized by economic catastrophe but, in comparison to military regimes, less affected by

ordinary poor economic performance. Recent African experience suggests that reforms reducing state intervention in the economy, and hence the rents and corruption opportunities often used to reward supporters, may be as destabilizing to personalist regimes as economic crisis itself.

Because officers tend to decide to return to the barracks for reasons relating to internal military concerns rather than being forced out of office by popular protest or external events, we should expect them to negotiate their extrication. When officers decide to withdraw from power, they enter into negotiations with civilian political leaders to arrange an orderly transition and to safeguard, if possible, their own interests after the transition. We should thus expect that military regimes will be more likely than other kinds of authoritarianism to end in negotiation.

Because of the *internal* sources of fragility in military regimes, we should expect them to be overthrown by armed insurgents or ousted by popular uprisings only rarely. Demonstrations against them occur, but most of the time such demonstrations persuade factions of the military to initiate a transition before popular opposition develops into rebellion. Coups are common in military regimes, but they rarely end the regime. They are usually leadership changes, the analogue of votes of no confidence in parliamentary systems. Coups that bring a liberalizing military president to power often precede transitions in military regimes; such coups can be interpreted as first-mover strategies. They demonstrate that a shift in officer opinion has occurred and that a substantial faction prefers to return to the barracks.

In strong contrast to military officers, the leaders of personalist regimes generally fight tooth and claw to hang on to power. In Bratton and van de Walle's words, "They resist political openings for as long as possible and seek to manage the process of transition only after it has been forced on them" (1997, 83). If they are forced — by foreign pressure, for example — to negotiate with opponents, they renege on agreements at the first opportunity.²¹ Military governments rarely renege on the agreements they make, not because they cannot, but because agreements are made at a time when most officers want to return to the barracks.

The cadre-interests argument claims that in normal times, the

21. Note, for example, the way Mobutu of Zaire (now Congo), Eyadema of Togo, and various other long-ruling African leaders manipulated electoral rules and intimidated opponents after agreeing, under pressure from international aid donors, to initiate multiparty elections.

members of a ruling personalist clique have little reason to desert their leader or oppose the regime. We should expect to see elite desertions of the regime only if rents and opportunities can no longer be distributed to supporters or if the leader loses control over the security apparatus and armed forces, thus reducing the risk of plotting his overthrow. Loss of control of the security apparatus can happen for various reasons, but one obvious and usually insurmountable reason is the death or physical incapacity of the leader. Dead or incapacitated leaders are replaced in all political systems, but the demise of the leader does not usually end other forms of authoritarianism. Because control of the armed and security forces is usually concentrated in the dictator's hands in personalistic regimes, however, his death or serious illness often reduces the risks of opposition. A testable implication of this argument is that the death of the leader is more likely to lead to regime breakdown in personalist than in other types of authoritarian regimes.

According to the cadre-interests argument, most of the military prefers to return to the barracks in some circumstances. Even for those officers who would prefer to remain in government, the cost for most of resuming a more ordinary military career is low. The cost of losing office is higher for cadres in a dominant party, but not, on average, devastating. Many prominent politicians in post-transition democratic regimes were once cadres of the formerly dominant party. Although the cadres of a single-party regime cannot be expected to desert when times are good, if it looks as though the party's hegemony will soon end, those cadres who think they possess the skills to make a success of democratic politics and whose ambitions are frustrated within the ruling party can be expected to form or join opposition parties. Even those who remain in the ruling party to the bitter end need not despair of life after democratization. Many previously dominant parties continue to function as effective political actors after democratization (cf. van de Walle and Butler 1999). In fact, in a number of ex-communist and African countries, such parties have achieved executive office in the second free and fair election after democratization.

The members of personalist cliques, however, have fewer options. Joining the opposition prior to a transition can have very high costs, and many who desert the regime must go into exile in order to protect their lives and liberty. From exile, they may plot and organize, but few who remain at home are willing to risk

public opposition. Those who stick with the regime to the bitter end are much less likely to find a respected place in the post-transition political world than are the close supporters of single-party and military regimes. For these reasons, the end of a personalist regime is more likely to be violent in one way or another than is the end of a single-party or military regime. Thus, another testable implication of the cadre-interests argument is that personalist regimes should be more likely than other forms of authoritarianism to end in the assassination of the leader, popular uprising, armed insurgency, civil war, revolution, or armed invasion (cf. Skocpol and Goodwin 1994).

Violence and upheaval do not segue naturally into democratic elections; consequently, transitions from personalist rule should be more likely to end in renewed authoritarianism than are transitions from other forms of authoritarianism. Transitions accomplished by uprisings, invasions, or assassinations often allow the consolidation of power by those who overthrow the old regime. In contrast, negotiations during transitions usually set a time for elections and hammer out rules for how they will be conducted. Thus, competitive regimes are more likely to succeed military regimes than other forms of authoritarianism.

Like members of personalist cliques, cadres in single-party regimes have few reasons to desert in normal circumstances. Furthermore, because power is less concentrated in single-party regimes, they are less vulnerable to the death or illness of leaders. Thus, we should expect single-party regimes to last longer than either military or personalist regimes.

Because the dominant strategy of the ruling coalition in single-party regimes is to co-opt potential opposition, such regimes tend to respond to crisis by granting modest increases in political participation, increasing opposition representation in the legislature, and granting some opposition demands for institutional changes. They attempt to give the opposition enough to deter them from risky plots and uprisings while continuing to hang on to power.

In the most common kind of regime crisis—one caused by poor economic performance leading to antiregime demonstrations—the ruling elite in any kind of authoritarian regime tends to divide into intransigents and moderates as they struggle to respond. In military regimes, that division itself tends to persuade many officers that the time has come for a return to the barracks. In personalist regimes, the ruling coalition narrows as

the intransigents circle the wagons and exclude moderates from access to increasingly scarce spoils. Former regime moderates may then join the opposition because they have been excluded from the distribution of spoils (cf. Bratton and van de Walle 1997). Ruling parties, however, attempt to distract citizens from their economic grievances by granting them modest political rights. This strategy works only sometimes, but it works often enough to extend the average life span of single-party regimes.

Hypotheses Derived from Implications of the Argument

To summarize, we can list a number of expectations about what we would see in the real world if the basic logic of how elite politics works in different kinds of authoritarian regimes were correct. Compared to other kinds of authoritarianism,

- military regimes survive less long;
- military regimes are more quickly destabilized by poor economic performance;
- military regimes are more likely to end in negotiation;
- military regimes are more likely to be followed by competitive forms of government;
- personalist regimes are more likely to end when the dictator dies;
- personalist regimes are more likely to end in popular uprising, rebellion, armed insurgency, invasion, or other kinds of violence;
- personalist regimes are more likely to be followed by new forms of authoritarianism;
- single-party regimes last longest, on average.

Testing the Implications: "Measurement"

Since this is a book on research methods, I include here many details about case selection and classification that might ordinarily appear in the appendix of an article or book. To test the implications discussed above, I have collected basic information about all authoritarian regimes (except monarchies) lasting three or more years that existed or began between 1946 and 1996, in countries with a population of more than a million that became independent before 1990. Authoritarian regimes already in existence in 1946,

such as those in the Soviet Union, Mexico, and Turkey, are included, with their length of time in office calculated from the time they actually took power. Countries that became independent after 1945 enter the data set at the time of independence (if authoritarian). Countries that have achieved independence since 1990 because of the breakup of the Soviet Union and other communist states (and that remain authoritarian) have not been included, because the inclusion of a fairly large number of countries with severely truncated regimes might have biased conclusions.

The purpose of the three-year threshold is to distinguish regimes from temporary authoritarian interventions and periods of chaos. Regimes are defined as sets of formal and informal rules and procedures for selecting national leaders and policies. Under this definition, periods of instability and temporary “moderating” military interventions (Stepan 1971) are considered interregnums, not regimes. That is, they are periods of holding customary rules in temporary abeyance, struggle over rules, or transition from one set of rules to the next. The three-year threshold is simply a way of excluding such periods from the data set. This cutoff was chosen — after considerable empirical investigation of very short-lived authoritarian interludes — because it introduced the least misclassification into the data. A lower threshold would lead to the inclusion of a few moderating interventions and interventions that never managed to establish a new set of rules. The military governed during most of these interregnums. If they were included in the data set, the findings I report below would be stronger. A higher threshold would lead to the exclusion of some authoritarian governments that have been included in other literature on transitions.

I use a dichotomous measure of regime type (authoritarian versus not authoritarian) to identify cases for inclusion in the study, because the hypotheses I want to test require being able to identify the endpoints of regimes. Zachary Elkins (2000) argues that continuous measures of regime type are better, and for some purposes they are. In this study, however, they would add nothing. The argument makes no predictions about whether regimes are moving incrementally toward somewhat more press freedom or allowing minority parties a few seats in the legislature. It does make predictions about the conditions that cause regimes to end. To test those, I need to be able to identify unambiguously when an authoritarian regime has ended. I could have dichotomized an

available continuous indicator such as the Freedom House index rather than “measuring” the cases myself, but Freedom House indices are made up of measures of citizens’ political and civil rights. Since I define regimes as the sets of rules for choosing leaders and policies, Freedom House and other commonly used measures of democracy do not seek to measure the concept I have in mind.

Measurement decisions should derive from definitions of concepts in the theory being investigated and from the needs of particular tests of hypotheses. The usefulness of different indicators depends on their purpose and cannot be judged in the abstract.

In this data set, most decisions about whether governments were sufficiently authoritarian to deserve inclusion were easy, but a few were not. A significant complication was that norms for defining countries as democratic vary by region. Few Latin Americanists would classify Mexico as democratic before 1997, and some would not do so until the PRI finally lost the presidency in 2000. Among Africanists, however, Botswana, in which the ruling party has never lost control of the executive and at least two-thirds of the seats in parliament, is always called democratic. Needing a single standard to apply across regions, I classify regimes as authoritarian if opposition parties have been banned or subjected to serious harassment or institutional disadvantage, or if the ruling party has never lost control of the executive and has controlled at least two-thirds of legislative seats in all elections before 1985. Once a regime is labeled authoritarian, I do not consider it fully democratized until one turnover of executive power has occurred. Where it appears that conclusions might be affected by the stringency of these criteria, I also show results using less demanding rules.²²

A basic point to be made about using concepts with contested definitions (such as authoritarianism) in research is that the concrete criteria used for classifying cases or observations need to be clear. The researcher must take care to apply the same criteria to all cases. Where these “coding” decisions are complicated or

22. These regime type classifications are similar to those of Huntington (1991), and my “coding” judgments are very close to his. My decision rule for determining whether a political system had crossed the threshold to democracy is essentially the same as that of Przeworski and Limongi (1997). The biggest difference between my classification scheme and that of Linz and Stepan (1996) is that I collapse what they call “sultanistic” and “civilianized” regimes into one category — personalist.

require careful judgment, I suggest using a written coding scheme and reporting the classification of all cases along with the research findings.²³ If one suspects that the concrete criteria used to assign cases to categories will be controversial or that decisions about classification drive results, one should also show results using alternative classificatory criteria.

The rationale for the stringent classification rule used here is that a party (or clique) that has concentrated great power in its hands over the years can, like the current Malaysian government, very quickly and easily reinstate strict limits on opposition when threatened. Such a regime contains few institutionalized limitations on the power of rulers, even if the rulers have not previously felt the need for repressive measures and hence have not relied on them. The consequence of this rule is that a few cases that are sometimes considered democratic—notably, Botswana, Tanzania, Malaysia, and Taiwan (before the election in 2000)—are classified as single-party regimes here. Classifying these countries as democracies would reduce the average life span of single-party regimes by about a year.

To classify authoritarian regimes as military, single-party, personalist, or hybrids of these categories, I relied on the following criteria. Military regimes were defined as those governed by an officer or retired officer, with the support of the military establishment and some routine mechanism by which high-level officers could influence policy choice and appointments. Single-party regimes were defined as those in which the party had some influence over policy, controlled most access to political power and government jobs, and had functioning local-level organizations. Regimes were considered personalist if the leader, who usually came to power as an officer in a military coup or as the leader of a single-party government, had consolidated control over policy and recruitment in his own hands, in the process marginalizing other officers' influence and/or reducing the influence and functions of the party. In the real world, many regimes have characteristics of more than one regime type. When regimes had important characteristics of more than one pure regime type, especially when the area specialist literature contained disagreements about the importance of military and party institutions, I put them in hybrid categories.

In all cases, I attempted to rely on a regime's actual rules for

23. Devising a "coding" scheme is discussed in more detail in chapter 4.

selecting leaders and making allocative decisions rather than formal designations of regime type. In practice, many regimes have characteristics of more than one of these classifications, and many move from one category to another over time even though the same person holds the highest office.

Dictators sometimes succeed in transforming the regimes they lead from one kind to another. As noted above, the transition from military to personalist occurs frequently. I did not count these transformations as regime changes, since that would artificially reduce the length of what we in everyday language call regimes, and one of the implications I want to test involves length of survival. If an early period of uncertainty or transition was followed by consolidation of a different regime type, I assigned the regime to the category in which it seemed to stabilize. Some cases, however, had to be assigned to intermediate categories.

In deciding whether a regime led by the single leader of a single party should be classified as personalist or single-party, I gave more weight to the party if it existed prior to the leader's accession to power, especially if it had organized the fight for independence, a revolution, or some equivalent mass movement, rather than being formed by the leader after his accession; the heir apparent or the successor to the first leader already held a high position in the party and was not a relative or a member of the same tribe or clan as the leader; the party had functioning local-level organizations that did something important, such as distributing agricultural credit or organizing local elections; the party either faced competition from other parties or held intra-party competitive elections for some offices; and party membership was more or less required for government jobs. I gave the party less weight if its membership seemed to be almost all urban (with little or no grassroots organization); its politburo (or equivalent) served as a rubber stamp for the leader; all members of the politburo and assembly were in effect selected by the leader; its membership was dominated by one region, tribe, clan, or religion (in heterogeneous societies); and the dictator's relatives occupied high offices.

To classify a regime led by an officer as either military or personal, I leaned toward military if relationships within the junta or military council seemed relatively collegial; the ruler held the rank of general or its equivalent; the regime had some kind of institutions for deciding succession questions and for

routinizing consultation between the leader and the rest of the officer corps; the military hierarchy remained intact; the security apparatus remained under military control rather than being taken over by the leader himself; succession in the event of the leader's death was in hierarchical order; the officer corps included representatives of more than one ethnic, religious, or tribal group (in heterogeneous countries); and the rule of law was maintained (perhaps after rewriting the laws). I treated the following as evidence of greater personalism: seizure of executive office by an officer who was not a retired or active duty general (or the air force or navy equivalent); disintegration of military hierarchy; dissolution of military councils and other military consultative institutions; the forced retirement or murder of officers within the leader's cohort or from tribes or clans other than the leader's; the murder or imprisonment of dissenting officers or of soldiers loyal to them; the formation of a party led by the leader as an alternative base of support for himself; and the holding of plebiscites to legitimize the leader's role. See appendix A for a summary of the regime classification criteria.

Most of the time it was not hard to distinguish between military and single-party regimes, though a few cases, especially in the Middle East, were problematic. Probably the most difficult decisions in this data set involved the current Egyptian regime and post-1963 Syria. Egypt posed a problem because the regime that took power in 1952 has gone through a series of changes. In my judgment, it began as a military regime under Naguib and the Free Officers but was transformed when Nasser consolidated his personal power beginning in 1954. Though the military continued to support the regime, Nasser—and Sadat to an even greater extent—increasingly marginalized it (Springborg 1989). Beginning under Nasser, efforts were made to create a single party; this party achieved some real importance in the mid-1960s but was then undermined by Nasser (Waterbury 1983; Richards and Waterbury 1990). The Nasser period thus seems primarily personalist. Under Sadat, the party became more important, though his government also retained large personalist elements (Hinnebusch 1985). The dominant party has played a more important role as the regime has gone through a modest liberalization. In the Syrian case, some experts refer to the period after 1963 as a Ba'athist regime (Ben-Dor 1975; Perlmutter 1969; Richards and Waterbury 1990), while others emphasize the personal power of Hafez al-Asad until his death (Hopwood 1988; Ma'oz 1986, 1988; Rabino-

vich 1972). As in the Egyptian case, the military is an important supporter of the regime but seems to have been excluded from most decision making. The best way to deal with these difficult cases seemed to be to put them, along with the regimes of Suharto in Indonesia, Stroessner in Paraguay, and Ne Win in Burma (or Myanmar), into a triple hybrid military/personalist/single-party category. The second section of appendix A lists all the regimes used in the data analysis and their classifications.

How long an authoritarian regime lasts is not always obvious. The beginning is usually clear, because dictatorships start either with an illegal seizure of power or with a change in rules—such as the banning of opposition parties—that in effect eliminates meaningful competition for the top national office, though opposition parties may be allowed minority representation. But the end of an authoritarian regime may be less clearly demarcated. I counted an authoritarian regime as defunct if either the dictator and his supporters had been ousted from office or a negotiated transition resulted in reasonably fair, competitive elections and a change in the party or individual occupying executive office. Where ousters occurred, I used that date as the endpoint. Where elections occurred, I used the date of the election, but I did not include the case unless the winner of the election was allowed to take office. Elections did not have to be direct, but the body electing the executive had to be made up mainly of elected members. Cases in which elections deemed free and fair by outside observers have been held but have not led to a turnover in personnel are not treated as transitions because, until they actually step down, we do not know if long-ruling parties such as the United Malay National Organization (UMNO) or the Revolutionary Party of Tanzania (CCM) really will relinquish power if defeated.²⁴ The 1992 Angolan elections were deemed free and fair by outside observers, but few would have called Angola a democracy in subsequent years. Several of the countries in which long-ruling parties have won officially free and fair elections, however, probably have taken irreversible steps toward democracy. Since observers disagree about the classification of these “free and fair” countries, tests should be done classifying them first as continuing authoritarian regimes and then as

24. In a study of transitions in Africa, van de Walle and Butler (1999) show that a strong relationship exists between executive turnover and scoring at the democratic end of the Freedom House scale, which suggests an additional reason for not treating democratization as complete until a turnover in power has occurred.

authoritarian regimes that ended at the time of the “free and fair” election. In this data set, these reclassifications make no substantive difference in the results.

Some of the most difficult classification decisions involved judgments about whether successive authoritarian governments should be considered one regime (defined as a set of formal and informal rules and procedures for choosing leaders and policies) or not. Authoritarian regimes often follow one another, as, for example, the Sandinista regime followed the Somozas in Nicaragua. Data sets that simply identify regimes as authoritarian or democratic create the impression that authoritarian regimes are more stable and longer-lived than they really are, because they fail to note that one has broken down and another taken its place. This problem may undermine some of the findings in a series of studies by Adam Przeworski and coauthors on the relationship between regime type and growth (e.g., Przeworski and Limongi 1993; Przeworski et al. 2000). In putting together their data set, they simply coded each country as democratic or not in December of each year. If a country was coded authoritarian two years in a row, the regime was considered to have survived, regardless of whether one authoritarianism had been replaced by another or a democracy had been formed and then overthrown during the intervening year.²⁵

I relied on a number of decision rules to avoid this problem. Where a period of democracy intervened between two periods of authoritarianism, I counted the authoritarianisms as separate entities. Where one kind of authoritarian regime succeeded another, as with Somoza-Sandinista, I counted them as separate. Some of these decisions were much more difficult. In a number of cases, periods of collegial military rule were succeeded by one officer’s consolidation of his personal power. These I classified as single regimes undergoing consolidation, unless there was persuasive evidence that the support base of the regime had changed. Where a coup — especially if accompanied by a change in clan or tribal dominance or a substantial move down the military hierarchy (e.g., a coup by sergeants against a government led by the

25. This coding decision does not affect their main finding about the robustness of democracy at high levels of development, but it does undermine conclusions about the effect of economic performance on authoritarian stability, since ousters of dictatorships followed by renewed authoritarian rule within the year are coded as on-going regimes.

high command)—led to the change in most of the leadership, I counted it as a regime change. Where one individual who was already part of a governing junta overthrew another but most of the rest continued, I counted it as a single regime.

In any study, but especially when the project involves complicated or contested decisions about how to classify cases, it is important to carry out the analogue of sensitivity analysis in statistics. That is, one should reclassify the cases and see if it affects conclusions. This might involve including or excluding cases from the data set, as in the decision about how many years a dictatorship has to survive in order to be classified as a regime. If I had followed the usual practice of including every period of authoritarianism that lasted a year or more, military regimes would appear even more fragile than they do in the results below, because most of these very short interventions are military. The three-year threshold seems to me theoretically correct in that it derives from the definition of a regime as a set of rules, but it is also a methodologically conservative decision. If the empirical investigation turns out to support the argument even though the most short-lived military interventions have been excluded from the data set, then we can have greater confidence in the argument, because changing that decision rule would only strengthen the findings.

The reclassification of cases could, alternatively, involve moving them from one category to another on one of the variables. For this project, I classified a number of rulers who are often described as military—for example, Barrientos in Bolivia and Ershad in Bangladesh—as personalist because, although they were officers and came to power in coups, the military was not their primary constituency; they organized civilian support and held popular elections to legitimate their rule. If I were to eliminate this criterion for discriminating between personalist and military rule, a certain number of cases would move from the personalist to the military category. The changes would not affect conclusions about the length of military rule, because most of the cases that would be affected were quite short-lived. The reclassification would, however, increase the number of regimes classified as military that ended in violence, and this could affect conclusions about another of the implications of the cadre-interests argument not tested here: that personalist regimes are more likely than others to be violently overthrown.

TABLE 2.1. Durability of Different Types of Authoritarian Regime

Regime Type	Average Length of Rule (years) ^a	Average Age of Surviving Regimes ^b	Percent Regimes Surviving in 2000
Military	9.5 (33)	10.0 (2)	5.7
Military/personalist	11.3 (12)	12.7 (3)	20.0
Personalist ^c	15.5 (46)	18.0 (12)	20.7
Single-party hybrids ^d	19.6 (14)	25.2 (6)	30.0
Single-party (stringent transition criteria) ^e	29.0 (21)	34.0 (13)	38.2
Single-party (less stringent transition criteria)	27.9 (24)	35.4 (9)	27.3
Triple hybrid	33.0 (3)	43.5 (2)	40.0

Note: Regimes maintained by foreign occupation or military threat are excluded. Number of observations on which averages are based is shown in parentheses.

^aIncludes only regimes that had ended by December 2000.

^bIncludes regimes in existence in 1946, or that have come to power since then, that still survived at the end of 2000.

^cThe Rawlings government in Ghana held elections deemed free and fair by international observers in 1996 (and elections boycotted by the opposition in 1992), and voters reelected Rawlings. Many then considered Ghana democratic, but by the criteria used for this study its transition was completed in 2000. If Ghana were classified as having made a transition in 1996, this change would have no effect on the average length of personalist regimes.

^dCategory includes both military/single-party and personalist/single-party regimes.

^eSix countries in this category have held elections deemed free and fair but nevertheless returned the ruling party to power. The results if these countries are classified as having democratized at the time of the first free and fair elections are shown in the next row.

Testing One of the Implications

Here I describe a test of the first implication above: that military regimes should be expected to survive less long than other kinds of authoritarianism.²⁶ Preliminary evidence bears out the expectation that sources of fragility endogenous to military regimes cause life spans shorter on average than those of other forms of dictatorial rule. Table 2.1 shows the average life spans of both the pure and hybrid regime types. Among regimes that had ended by December 2000, military regimes lasted on aver-

26. Tests of some of the other implications of the cadre-interests argument are reported in Geddes (1999a).

age 9.5 years,²⁷ personalist regimes 15.5 years, and single-party regimes (excluding those maintained by foreign occupation or threat of intervention) 29 years.²⁸

Another way to assess the durability of different regime types is to compare their current survival rates. As shown in column 3 of table 2.1, the proportion of surviving military regimes is quite low. Only 5.7 percent of those that once existed still survive. In contrast, 20.7 percent of personalist regimes remain in existence, and 38.2 percent of single-party regimes still survived in 2000 if stringent transition criteria are used to determine regime endpoints (27.3 percent if less stringent criteria are used).²⁹ Military regimes that had come to power by 1997 and still survived had lasted an average of 10 years by 2000. Single-party regimes that remained in power, on the other hand, had lasted an average of 34 years (35.4 if less stringent transition criteria are used).

Although these differences in the average length of different types of regime are quite large, we cannot be sure that they are really caused by regime type. Military regimes are more common in Latin America, where levels of economic development are relatively high, and personalist regimes are most common in Africa, where countries tend to be poorer. It might be that the stronger demand for democracy by citizens of more developed countries accounts for the shorter duration of military regimes. Alternatively, it might be that military regimes last less long because they are responsible for worse economic performance

27. Reminder: authoritarian interludes lasting less than three years have been excluded from the data set. The military ruled during most of these interludes. If they were included, the average length of military rule would be reduced. Nordlinger, who did not exclude them from his calculations, found that military regimes last five years on average (1977, 139).

28. Regimes maintained in power by direct foreign occupation or the threat of military intervention have been excluded from the calculation of average life span here and from the statistical analysis below because their longevity depends on external events. The excluded regimes are those in Afghanistan, 1979–92; Bulgaria, 1947–90; Cambodia, 1979–90; Czechoslovakia, 1948–90; German Democratic Republic, 1945–90; Hungary, 1949–90; and Poland, 1947–89. The average length of these regimes is 34 years.

29. The stringent criteria for determining the end of an authoritarian regime require not only that competitive elections be held but also that the executive change hands. The less stringent criteria count authoritarian regimes as ended if competitive elections are held and are considered free and fair by outside observers, regardless of who wins.

than other kinds of authoritarianism. To test for these possibilities, I have carried out statistical tests of the effect of regime type on the probability of regime breakdown, controlling for level of development, growth rate, and region.

I use a hazard model to assess these rival arguments. Hazard models are used in medical research and other areas to predict the survival of individuals with certain conditions, given various treatments. This type of model also seems appropriate for explaining the survival of a different kind of entity. A logit model produces the same substantive results, so they do not depend on the particular specification.

To rule out the possibility that the apparent relationship between regime type and length of time in office might really be caused by level of development, the statistical analysis includes an indicator of development, the natural log of GDP per capita. Since a number of studies have found that current economic performance affects the likelihood of regime breakdown, an indicator for growth is also included in the models as a control variable. The measure of growth used is change in GDP per capita in the prior year. I use the prior year because credit or blame for the prior year's economic performance is unambiguous. In years in which a transition takes place, the outgoing regime might be responsible for only part of the year's performance. Furthermore, economic performance is often erratic in transition years. It can plummet in response to government instability, but it can also improve rapidly during the euphoria that sometimes accompanies a transition. Thus, the previous year's growth seems a better indicator of the regime's recent economic performance. Adam Przeworski and Fernando Limongi (1997) also found, after trying a number of possibilities and lags, that growth during the prior year was the best predictor of regime change.

Economic data are from the Penn World Tables, the longest time series for the largest number of countries I have been able to find. For most countries, it covers 1950–92, which means that regime years prior to 1951 and after 1992 are excluded from the statistical analysis. In addition, no economic data are available from Albania, Cambodia, Cuba, North Korea, Libya, Vietnam, and South Yemen, and there are some years missing from a few other countries. Since the period covered is quite long and I cannot think of any reason to believe that transitions during the years covered would be different from those in the years immedi-

ately before and after, I do not think the years excluded introduce bias into the results.

The countries left out of the data set, however, differ from those included. Most had or have single-party or personalist/single-party regimes. Their regimes have lasted an unusually long time (excluding Cambodia, 32.1 years on average). Dictators still rule in five of seven, and nearly all the countries are very poor. If they were included in the data analysis, they would probably further strengthen the coefficient for the effect of single-party regime and reduce the effect of level of development on the probability of regime stability. Since the data set is large, however, and not very many cases have to be left out, I do not think their exclusion has much effect on conclusions.

Region is used as a quasi-fixed effects estimator.³⁰ Fixed effects estimators are used to hold constant aspects of history and culture that might affect the outcome of interest but that cannot be directly measured. I have used region to hold constant some of the possible effects of colonial history and cultural heritage.

Because regime types are nominal categories, they are entered into the model as dummy variables: if the regime is, for example, military, it is coded "one"; otherwise, it is coded "zero." The left-out regime type is personalist, the middle category in terms of longevity. Thus, the hazard ratios reported should be interpreted as referring to differences between the effect of the type of regime associated with a particular ratio and the effect of personalist regimes.

Hazard ratios have a simple intuitive interpretation. Ratios above one mean that the variable associated with them increases the probability of regime collapse. In the first column of table 2.2, the hazard ratio for military regime is 2.81, which means

30. Usually, country dummy variables are used as fixed effects estimators, but they could not be used to analyze this data set because they cause countries with only one regime to be dropped from the analysis. In this data set, half the countries have had only one authoritarian regime, either because one stable regime remained in power for several decades or because the country is usually democratic and had only one postwar authoritarian interlude. A more serious problem than the loss of cases per se is that regimes in the cases with only one regime, are, on average, unusually long-lived, and they are especially likely to be single-party regimes. The use of country fixed effects estimators eliminates 60 percent of the single-party regimes from the analysis. When the analysis was done using country fixed effects estimators, the coefficient for the effect of military regime was artificially strengthened (since the longest military regimes were eliminated), and the effect of single-party regime was greatly weakened (since most of the single-party regimes were eliminated, leaving an unrepresentative set of mostly African cases).

that, all else being equal, military regimes are nearly three times as likely to break down as personalist regimes. Hazard ratios between zero and one mean that the variable reduces the probability of breakdown. In column 1, the hazard ratio for single-party regimes, .39, means that, all else being equal, single-party regimes have about 40 percent the chance of collapsing that personalist regimes do.

As can be seen in column 1, military regimes break down more readily than all other types. The hazard ratio for the military regime variable is substantively large and statistically significant. The two intermediate regime types, military/personalist and single-party hybrid (in which personalist/single-party regimes predominate, since there are very few military/single-party regimes), are, not surprisingly, not very different from personalist regimes. Single-party regimes, however, are more resilient than personalist regimes to about the same extent that military regimes are less resilient, and this difference is also statistically significant. Finally, the triple hybrid regimes, which combine characteristics of single-party, personalist, and military regimes,

TABLE 2.2. Effect of Regime Type on Authoritarian Survival (Weibull regression, log relative-hazard form)

Dependent Variable: Regime Collapse (hazard ratios)

	Model 1	Model 2	Model 3
Military	2.81**	2.83**	10.26**
Military/personalist	1.31	1.15	2.07
Single-party hybrids	1.24	1.47	3.44*
Single-party	0.39**	0.38*	0.59
Triple hybrid	0.04**	0.00	0.00
Log GDP per capita	0.53*	0.54*	0.40
Growth GDP per capita	0.02*	0.02*	0.004**
Asia	1.22	0.97	0.19
Central America, Caribbean	0.99	0.98	0.23
Eastern Europe ^a	0.18*	0.16	0.00
Middle East	7.46	3.29	0.40
North Africa	0.42*	0.15	0.01**
South America	3.44	3.95	0.35
Sub-Saharan Africa	0.61	0.41	0.05**
Percent Muslim		1.01*	1.02**
Dependence on oil			1.02
Dependence on minerals			1.00
N of observations	1,694	1,627	861

Note: Left-out regime category is personalist; left-out region is Southern Europe.

^aExcludes regimes maintained by foreign intervention.

* Statistically significant at .05 to .01; **statistically significant at .01 or better.

are the strongest of all. An alternative to the substantive interpretation of this category is that it simply serves the purpose of controlling for five very long-lived and unusual regimes that might otherwise inflate the apparent longevity of single-party regimes.

The control variables used in the regression also show some interesting effects. As the level of development rises, authoritarian regimes, like democratic ones, become more stable. This finding is consistent with that of John Londregan and Keith Poole (1990, 1996), who found that the best predictor of coups, in both democratic and authoritarian regimes, was poverty. It raises some questions, however, about traditional demand-centered explanations for the relationship between increased development and democracy. It is inconsistent with the idea that the citizens of more affluent countries are more likely to demand democratization. Rather, it suggests that when authoritarian governments manage the economy well over the long term, regime allies remain loyal and citizens remain supportive, or at least acquiescent. That interpretation is reinforced by the very strong negative effect of short-term economic growth on the probability of regime breakdown. In other words, both long- and short-term economic performance affects authoritarian stability.

In light of various arguments about the effects of religion, culture, and colonial heritage on the development of democratic values, it is somewhat surprising that most of the region variables show little effect. The left-out region here is southern Europe (Portugal, Spain, and Greece), and we might have expected the regions most culturally distinct from Europe to exhibit differences in the likelihood of regime transition. The only two regions with statistically significant hazard ratios in model 1, however, are Eastern Europe and North Africa. Since the governments kept in place by the threat of Soviet intervention have been excluded from these tests, the East European region contains only the Soviet Union, Romania, and Yugoslavia (Albania had to be excluded because of missing economic data). Controlling for level of development, growth, and regime type, regimes in these countries were unusually resilient. There are internal reasons for this resilience, but in the context of the full data set, this region dummy variable in effect controls for the very unusual longevity of the Soviet regime and prevents it from inflating the apparent effect of the single-party regime type.

The unusual resilience of authoritarian regimes in North

Africa leads to speculations about other possible causes of authoritarian stability that might have been left out of these tests. Several studies have shown that countries with large Islamic populations are less likely to be democratic, and Michael Ross (2001) shows that oil wealth is associated with authoritarianism. North African exceptionalism might be caused by Islam or by oil wealth, though it should be noted that the hazard ratio for the Middle East is not only insignificant but suggests the opposite effect.

The results of tests of these possibilities are shown in columns 2 and 3 of table 2.2. For the hazard model used in column 2, the percentage of the population that is Islamic was added as an additional control variable. The first thing to notice is that the inclusion of this control variable has virtually no effect on the relationship between regime type and breakdown. Those relationships look as strong as ever, and the hazard ratios for growth and level of development are also unaffected. The absence of change increases our faith in the importance of the variables of interest.

The effect of percent Muslim population on the probability of breakdown would be quite interesting in its own right if we believed it. It is statistically significant, though the effect is the opposite of that expected. As Muslim population increases, the probability of regime collapse becomes more likely. This finding does not, of course, mean that democratization is more likely in Islamic countries. Authoritarian regimes collapse and are followed by other authoritarian governments. If this finding were to be replicated in additional tests, it would disconfirm the idea that authoritarianism is more stable in Islamic countries because of an affinity between Muslim culture and authoritarian values. The results so far have to be considered quite tentative, however, because of the exclusion of monarchies from the data set. Since most of the extant monarchies are both long-lived and in predominantly Muslim countries, their inclusion might well cause the disappearance of this finding.

Adding dependence on oil and minerals to the model (column 3) reduces the number of observations by about half and causes the hazard ratios to bounce around quite a bit. Statistical significance is harder to achieve in the smaller data set. Because of peculiarities in the set of cases for which data on oil exports are available, findings from the third model should probably

not be taken too seriously.³¹ For what they are worth, however, the hazard ratio for military regimes is still statistically significant and remarkably large, growth still reduces the likelihood of breakdown, and percent Muslim has about the same effect as before. Single-party regime and level of development still reduce the probability of breakdown but have lost statistical significance. Dependence on oil apparently has no effect on authoritarian stability, contrary to much that has been written in the literature on rentier states, though here also the exclusion of monarchies renders the conclusion suspect. In short, these findings are mostly consistent with expectations drawn from the cadre-interests argument, though if we had faith in the quality of the data on which the findings are based, the loss of statistical significance for the single-party variable would be cause for concern.

To summarize the findings, the hypotheses about the average duration of different types of authoritarian regime have been mostly confirmed by statistical analysis, holding constant the most obvious challenges to the apparent relationship. Growth was found to have the expected effect of reducing the probability of regime breakdown. Higher levels of development also probably reduced the likelihood of authoritarian breakdown. In short, empirical investigation of the first implication of the cadre-interests argument about authoritarian breakdown has failed to disconfirm expectations.

A series of methodological observations can be made about the empirical test described above. The first observation, though obvious, may need restating: this was a test of an implication of the argument, not of the argument itself. The implication is quite simple and, once the data had been collected, easy to test. Nevertheless, its confirmation adds to the persuasiveness of the argument, and if the other implications listed above also proved consistent with reality, we would be pretty much convinced that the argument captures a key aspect of the explanation of the breakdown of authoritarian regimes.

31. For nearly all African countries, most years after 1983 are missing, which means that most African transitions are missing. In other words, the missing cases are almost all from the poorer half of the data set, which is probably the reason that level of development loses significance. The later years of a considerable number of long-lived African single-party regimes are also left out.

Second, in order to test the implication, evidence had to be gathered about a large number of cases, although the kind of evidence needed was not complicated. It is not always necessary or feasible to include the whole universe of cases, as I did here. If examining the whole universe is infeasible, however, the whole universe nevertheless needs to be identified so that cases from within the universe can be selected at random or in some other way that does not bias conclusions. (Figuring out the domain of an argument is discussed in chapters 3 and 4.) Cases should be selected to ensure that the outcome of interest varies across them. (Case selection is treated in detail in chapter 3.) Cases that have been studied repeatedly by other scholars are, on average, larger, more developed, and more geopolitically important than the cases that have not been studied, and conclusions based on experience in such cases are therefore unlikely to be representative of the whole group.

The number of cases used to test an argument needs to be reasonably large, since it is very hard to be sure that a result has not been caused by chance events when only a few cases are examined. The more arguments one can think of other than the one of interest that could also explain an apparent relationship, and the more factors one thinks need to be held constant in order to exclude the effects of irrelevant forces on the relationship of interest, the more cases need to be examined. The possibility of spurious correlation — that is, the possibility that what appears to be a relationship between some cause and effect really results from some outside factor that causes both of them — can rarely be dismissed without using statistics, and not always even then.

All data sets, whether gathered by the researcher or taken off the shelf, contain some missing data and some mistakes. The researcher should always think carefully about how the missing cases differ from the cases included in the study and how their inclusion, if it could be managed, would affect conclusions. When using an off-the-shelf data set known or rumored to contain mistakes, the researcher should try to figure out whether the mistakes are likely to affect conclusions. The Freedom House democracy indicator, for example, is rumored to contain a pro-Washington bias, especially during the early years. For some purposes, this bias would not matter, but for others it might seriously undermine the credibility of findings. One would want to use a different indicator of democracy in the latter situation.

Conclusion

This chapter began with the claim that one of the practices hindering the accumulation of theory in comparative politics is the way we usually go about trying to explain compound outcomes such as democratization. I argued that greater progress could be made toward actually understanding how such outcomes occur by examining the mechanisms and processes that contribute to them, rather than through inductive searches for the correlates of the undifferentiated whole. Coherent deductive arguments can be devised to explain constituent processes, and hypotheses derived from the arguments can be tested.

I attempted to demonstrate the usefulness of this approach with an extended example. After identifying seven constituent processes of the large, complicated phenomenon of democratization, I proposed a deductive argument based on the individual interests of regime insiders to explain why elite splits play a larger role in some instances of authoritarian breakdown than others, and why some authoritarian regimes initiate political liberalization in the absence of societal pressure to do so. Although this argument as a whole is not testable, it was a simple matter to derive implications of the argument that could potentially be falsified.

The only impediment to testing these hypotheses was the need to gather an appropriate data set. Data gathering was a major and time-consuming effort, but once the data had been gathered, it was possible to show not only that predicted differences existed, but that they were quite large and statistically significant.

I make no grand claims for the cadre-interests argument itself. It may not be true. It is possible that when other variables are taken into consideration, the relationship that seems apparent now between regime type and longevity will disappear. Even if true, the argument explains only one element of the compound process of regime transformation. I do claim, however, that an argument from which an implication has been tested on evidence from a large number of cases is more likely to prove of lasting value than untested arguments induced from a handful of cases. And once data have been gathered, more implications can be tested. If those tests also conform to expectations generated by the argument, our confidence that the argument is true will increase. I also claim that to tack down an explanation of one

process that contributes to a compound outcome of great theoretical and real-world importance such as democratization would constitute serious intellectual progress.

While inductive explorations of instances of transition may have been the only possible research strategy at the beginning of the current wave of democratization, we now have enough basic information to move on to theory building. I have tried to show here that the theoretical edifice can best be built one deductive brick at a time, testing as many of them as possible using evidence from a large number of cases.