Chapter 5: Case Studies and Comparative History

None of the inventories of the causes of democracy surveyed in Chapter 4 can be taken at face value because they summarize conclusions from disparate and poorly integrated sources of uneven rigor. Before giving credence to checklist items, it is essential to find out where they came from. The remainder of this book therefore evaluates the strengths and weaknesses of the methods and approaches employed by the scholars who have launched these propositions into our collective academic mind.

If there were a way to measure quantities of information there would be no doubt that the bulk of our knowledge of democratization comes from histories and case studies. It is ironic that non-comparative research so dominates a field known as “comparative” politics, but this is an irony that is longstanding and well known. The huge number of books and articles about the birth, death, or survival of democracy in dozens of countries in the twentieth century alone is more than any one scholar can digest. In fact, anyone unlucky enough to be buried under this small mountain of printed matter would surely be suffocated and crushed. To be sure, this literature does not satisfy all the criteria for good theory. I will argue in this chapter that histories and case studies produce a kind of knowledge that lacks generality and theoretical integration. However, this kind of knowledge is unsurpassed in its thickness; hence its bulk. In this chapter I will also survey and critique a smaller but still sizable body of literature that consists of comparative histories: detailed comparisons of the historical development of a (usually) small number of cases. Comparative history reaches out toward generality and theoretical integration, but still has enough characteristics in common with case studies and histories to warrant discussing them jointly.
Histories and Case Studies

Histories (written by historians) and case studies (written by political scientists) are different genres with zealously policed boundaries. In fact, one of my purposes in discussing them together is to highlight their differences. However, they also have two advantages in common. First, they are thick: multifaceted, detailed, conceptually rich, and multidimensional. (See Chapter 3 for a fuller definition of thickness.) Second, they analyze change over time in a way that makes especially solid causal inferences possible.

Histories and case studies are the best examples of a "thick" approach. Both paint multifaceted, multidimensional portraits of countries and do so in rich detail. Studies of democratization in single countries, for example, are so richly detailed that they do not attempt to explain anything as grand as "democratization." Instead, they explain a series of specific events that, taken together, amount to democratization. A typical history is R.L. Webb's Modern England (Webb 1980). England was clearly not a democracy at the book's opening in 1760 but had clearly become a (male) democracy 420 pages later in 1918. Yet no one event marks the democratization of England. Rather, democratization is the accumulation of small reforms: the separation of the bureaucracy from the King's household, the development of a prime minister as a first among equals, the toleration of other religions, the formation of principled political parties, the development of the principle of collective responsibility, the publication of parliamentary debates, the obligation of the monarch to appoint the ministers chosen by parliament, the elimination of the royal veto, elimination of prior censorship of the press, the equal apportionment of electoral districts, the enfranchisements of middle- and working-class men, the responsibility of the cabinet to the Commons, and the diminution of the legislative powers of the House of Lords. Webb's story of these reforms makes them impossible to understand apart from the personalities of monarchs and
politicians, personal rivalries and reputations, deaths of major figures, and the formation and
dissolution of cabinets, all played against the backdrop of wars, economic crises, economic
transformation and social and technological change.

Similarly, a case study of the consolidation of democracy in Venezuela, Daniel Levine's
*Crisis and Political Change in Venezuela*, recounts in detail crucial events of the first, short-lived,
democratic regime of 1945-1948 and the first decade of the second democratic regime that began in
1958 (Levine 1976). It also analyzes the formation of party factions and the origins of party splits,
explains negotiations over education and social policy, and characterizes the personalities of several
key political leaders. Like a history, it reconstructs events, focuses attention on decisions made by
individuals, and situates micro-level processes within macro-level institutional, economic, social, and
international contexts. Both Webb's and Levine's books, and the histories and case studies they
represent, deserve to be called "thick" in the best sense of the term.

Because they are thick and analyze change over time, histories and case studies share the
ability to support powerful causal inferences. Causal inference--attributing causation of an outcome
to some stimulus or stimuli--is hard. In fact, as Popper argued, we can never prove that a causal
relationship exists; the best we can do is to disprove other hypothetical causal relationships: an
indirect "proof," at best (Popper 1968). What makes causal inference particularly hard in political
science is the complexity of the political world. Recall the argument from Chapter 3 that politics is
complex; that events have many facets and that outcomes are the product of a long and densely
tangled chain of causation. This complex nature urges us to build complex, multifaceted
explanations. But the more elements there are to an explanation, the greater the danger that some of
them will be spurious or false, and the more difficult it becomes to disconfirm all the possible
explanations except the one we wish to "prove," indirectly. (Chapter 7 will develop this argument
Indirectly "proving" an argument about causes in politics therefore necessarily involves holding constant, or controlling for, as many alternative hypothetical causes as possible. Histories and case studies excel at this because they take advantage of change over time in a single case. In effect, what they do is compare each country to itself at an earlier time. This is a far more foolproof method than comparing one country to another, because every country has much more in common with itself in the recent past than it does with any other country at any time. The logic of inference is therefore that any feature that did not change from time 1 to time 2 cannot be a cause of an outcome observed at time 2. Because most features of a country do not change quickly, these static features can be ruled out as causes by this method and analysts can more confidently attribute causal force to the features that did change just before the outcome. The shorter the intervals between observations and the more finely the process is observed, the greater the confidence the inference inspires. A sweeping history of several centuries in one chapter probably inspires little confidence, but tracking events decade by decade begins to inspire confidence. Tracking them year by year or month by month is a vast improvement, and studies that manage to reconstruct events week by week or day by day begin to seem irrefutable. It becomes difficult to imagine an alternative story that would be consistent with all the details.

It is this historical dimension that grants histories and case studies of democratization their authority. For example, Levine's claim that moderating leadership helped consolidate Venezuelan democracy was bolstered by his accounts of attempts at radical mobilization that moderate party elites managed to isolate and defuse or marginalize (Levine 1976: ch. 5). Similarly, Webb can show that the Glorious Revolution of 1688, installing William and Mary as the monarchs of England, established the supremacy of Parliament over the King (Webb 1980). This kind of historical process-
tracing also enabled Alfred Stepan to show that President João Goulart's decision to mobilize landless peasants provoked Brazilian elites to conspire to overthrow him and that newspaper editorials helped legitimate the conspiracy (Stepan 1971:191-204). Comparisons within countries over time are the most powerful approach at our disposal for identifying the immediate causes of specific events.

Of course, there are some important differences between histories and case studies. Two complementary differences come to mind. The first is simply a matter of degree: most histories are thicker than most case studies in political science. There are exceptions, such as Jared Diamond's sweeping history of humanity, *Guns, Germs, and Steel*, which is necessarily thinner due to its scope. And case studies can be unusually thick, such as the Graham Allison classic *Essence of Decision*, which reconstructs decision-making during the Cuban Missile Crisis day by day, sometimes hour by hour (Allison 1971; Diamond 1997). Nevertheless, the general tendency is for histories, especially histories written by academics for academics, to be thicker than political science case studies. The latter may go into fine detail at points, but tend to leap from crucial event to crucial event, and not necessarily in chronological order. They do not sustain a continuous, detailed narrative.

This is not the only difference between these two genres, however. If it were, we would have to conclude that case studies are merely inferior histories. As Juan Linz once remarked to a convention audience, "If we do our work very well, we are *almost* historians." The other difference is that case studies in comparative politics are more narrowly and explicitly focused on explanation. Political science case studies almost always explicitly identify the independent variables and put the causal process front and center. They are organized to advance and support an argument about what caused what. They also, whenever possible, embed their arguments in a larger theoretical framework. They self-consciously present the case as a specific instance of more general rules. Case studies are,
by design, installments in the larger enterprise of theory-building. Histories also seek to explain, but it is just one of their purposes, which include recounting the facts completely and accurately, interpreting what events meant to people at the time, and simply telling a good story. Historians vary in their mix of purposes. Some emphasize the scholarly recounting, some tell better stories, and some are more didactic than others. But none are as single-mindedly focused on demonstrating that the outcome was a necessary consequence of the explanatory factors. Histories generously grant more of a role to chance, whim, accidents, mistakes, coincidences, and miscalculations, which make for a more colorful story. These are exactly the elements that render a theoretical explanation logically incomplete (see Chapter 3). As E.P. Thompson observed (even while defending the notion of a “logic of history”),

> Historical concepts and rules. . . display extreme elasticity and allow for great irregularity; the historian appears to be evading rigour as he disappears into the largest generalisations at one moment, while at the next moment he disappears into the particularities of the qualifications in any special case. This provokes distrust, and even laughter, within other disciplines (Thompson 2001:454).

Judged by the criteria of political science, histories are merely prolix, equivocating case studies.

By stereotyping histories as inferior case studies and case studies as inferior histories, I do not mean to imply that one is superior to the other; I merely mean to show that virtue in one discipline often appears to be a vice in another. The same is true for the vices that histories and case studies share when judged by the standards of other approaches in comparative politics. There are three shared vices in particular: myopia, an inability to generalize, and a tendency to capitalize on chance.

Myopia—already mentioned in Chapter 4—is the tendency to exaggerate the impact of short-
term, micro causes. This tendency is practically inevitable in any approach that involves close examination of cases. Mom was right: squinting causes near-sightedness. Looking at events in historical perspective, especially over closely-spaced intervals of time, necessarily privileges causal factors that change over those intervals and discredits causal factors that remain unchanged throughout the period of observation. Case studies of democratization therefore tend to emphasize what is dynamic in the short term: leadership, natural disasters, economic crises, and wars. This tendency has already been noted, in Chapter 4, in the critique of inductive elite theories of democratization, but examples can be found in most case studies.

One good example is Malloy and Gamarra’s *Revolution and Reaction: Bolivia, 1964-1985*, which analyzes Bolivian politics during the period from the breakdown of the revolutionary regime to the tortuous democratic transition of 1980-1982 (1988:84-228). Although the authors (both political scientists) make it clear that chronic patrimonialism, corruption, human rights abuses, and an unsustainable development strategy undermined support for every government during this period, the factors that drive the narrative forward, from coup to election to coup to election, are specific short-term events: a hunger strike, a party split, an uprising by one or another military faction, pressure from the Carter administration, the failure of negotiations with Chile over an outlet to the Pacific Ocean, the power-hungry personality of Gen. Hugo Bánzer, and popular reactions to repression by the García Meza government. The authors conclude that “if [democracy] comes, it will be produced by serendipitous salidas [exit pacts] reflecting the pragmatic creativity of political elites and not grand solutions reflecting intellectual imports or architectural schemes” (Malloy and Gamarra 1988:225).

Another example—this time from a relatively stable case—is González and Gillespie’s analysis of the impact of presidentialism on regimes in Uruguay (González and Gillespie 1994). Their goal is
to attribute regime crises to the presidential features of the Uruguayan constitution, but the task is quite a challenge because Uruguay suffered only one coup (1973) and one semi-coup (1933) from 1918 to 1994 and presidentialism was a constant during that time. If a constant feature such as presidentialism caused breakdowns only twice in 77 years, then the explanation must be incomplete. Something else much explain why coups happened in those years and not others. González and Gillespie therefore fill in the gaps with short-term, specific events and actions: the death of President Oscar Gestido in 1967, his replacement by the more hard-line President Jorge Pacheco, a Tupamaro guerrilla offensive, repression of leftists, the loss of a governing majority in congress, Pacheco’s selection of an unpopular successor, a pact between the two largest parties to support a crackdown, accusations of corruption, and the political isolation of the president (González and Gillespie 1994:233-7). If the authors were not determined to blame presidentialism, it probably would not figure in the explanation at all.

In neither of these examples, nor in case studies generally, do scholars ignore or completely discount structural causes such as social structure, geography, constitutional design, or culture. Myopia is not a failure to see large, distant objects at all, but an inability to see them in focus or, mixing metaphors, to give them their proper weight. Case studies and histories tend to give too much weight to the dynamic particulars they can portray clearly and too little weight to the large, immobile features of the landscape in the background that they take for granted.

The specificity of these approaches also interferes with generalization. The difficulties for generalization are conceptual, theoretical, and empirical. Conceptually, if one were to try to generalize about democratization from the case of England, for example, it would first be necessary to translate the particularities of the English process into concepts that usefully describe processes in other countries. Some of the small reforms listed at the beginning of this chapter do not match up
easily with steps toward democracy in other countries. In particular, reforms that weakened the monarchy are irrelevant for countries that never had a monarch. One of the reforms that has the most traveling potential is the establishment of the responsibility of the cabinet to the Commons. If we substitute “lower or sole chamber of the national legislature” for “Commons,” we have a reform that is comparable to similar reforms in most other parliamentary democracies. However, it is still irrelevant for the many presidential democracies, which permit the legislature to dismiss the executive only in extraordinary situations. If we hold fast to the exact concepts used in case studies and histories, it becomes difficult even to claim that the concept of “democracy” has any useful and cross-nationally comparable meaning.

Theoretical specificity also hinders generalization. If concepts are too specific to travel, theories using those concepts do not travel well, either. But even a theory expressed entirely in generally-comparable concepts can be too specific for generalization if it combines a large number of propositions. Imagine, for example, a theory that predicts that democratic regimes will survive in countries that have a high standard of living, a small number of political parties, effective representation of business interests, have never been colonies, and are found on islands. Each proposition narrows the applicable domain of the hypothesis to an ever-more limited set of cases. Many countries could satisfy the first three propositions, but ruling out former colonies excludes the U.S., Australia, Canada, Iceland, and many others. Limiting the domain to islands shrinks the domain to Japan and Britain; and if we tack on the additional requirement of a homogeneous national identity, then only Japan is left! This happens even though all of the propositions are expressed in terms of general concepts.

General empirical confirmation is more difficult for the hypotheses that arise from case studies and histories because these studies involve implicit conceptual or theoretical specificity. The
authors of the case studies may never have intended for their propositions to be generalized, but other scholars who try to generalize them find that the same propositions do not “work” as well when applied outside of their original context. For example, many scholars have noted that the survival of democracy in India is a puzzle because democracy rarely survives in other poor societies with deep ethno-religious-linguistic cleavages. To explain this paradox, scholars have repeatedly given some credit to British colonial rule, arguing that the British educated India’s elite in its liberal values, created an efficient civil service, and gave Indians experience with limited self-government (Weiner 1987, 20; Lipset 1993, 5). Even scholars who are more critical of the British role argue that opposition to the British united Indians behind the Indian National Congress, which then neutralized many social cleavages for decades after Independence, enabling democracy to survive. Although some version of this hypothesis works well for India and also seems important for understanding the survival of democracy in the United States, Canada, Australia, New Zealand, and the English-speaking Caribbean, it does not help explain the failure of democracy in some other former British colonies such as Nigeria, Pakistan, Gambia, Zimbabwe, Ghana, Malaysia, Singapore, Kenya, and Zambia. The problem is conceptual: “British colonial rule” meant different things in different colonies. At a minimum, we must distinguish between colonies of settlement (as in North America and Australia) and colonies of conquest. There were also differences in the timing and length of colonization, degrees of self-government and exploitation, and the nature of the independence struggle that may help resolve the Indian paradox. Applying watered-down versions of a proposition without its original contextual qualifiers amounts to “concept-stretching” (Sartori 1970). We should not be surprised that hypotheses built from stretched concepts do not hold up when taken out of context.

Another illustration comes from case studies of oil producers arguing that dependence on oil
exports either prevents democratization or undermines any democratic regime that comes into being. This proposition gains support from non-democracies in the Middle East and from fragile democracies such as Venezuela and Ecuador, but it founders on the shoals of Britain and Norway. One could conclude from this that the proposition is false, but it is more likely the case that it is true in some contexts but not others. Terry Karl has argued, for example, that dependence on oil exports is antithetical to democracy only where the oil industry developed before a bureaucratic state became consolidated; in Britain and Norway, an efficient state bureaucracy (and democratic regime) developed long before North Sea oil was discovered (Karl 1997). Karl’s argument in effect made an implicitly specific theory explicitly specific, which made it clearer that it had a limited domain.

Propositions generated by case studies tend not to survive attempts at generalization because they are, in reality, conditional on other theoretical propositions that are taken for granted by scholars focusing on a single case, and therefore left implicit.

The three problems of generalization just described are all problems for formulating theories or hypotheses that are likely to be generally true. Generalization, however, also concerns inference: drawing general conclusions from limited evidence. Case studies and histories are also handicapped for this task, for one can never infer that a proposition is generally true because it is true in one case. Nevertheless, there have been attempts to argue that although case studies cannot confirm a proposition, they can disconfirm one. In 1975 Harry Eckstein argued that a single case could disconfirm a general hypothesis if it was a “crucial case”: one in which the hypothesis must hold true if it is true for any case (Eckstein 1975). More recently, Douglas Dion used Bayesian conditional probability theory to show that some hypotheses about necessary conditions could be generally disconfirmed if they were falsified in a small number of cases, even a single case. However, this is true only when one already knows that the condition of interest probably is necessary and that any
alternative explanations probably are not true (Dion 1998). In other words, a single case is unlikely to tell us something we do not already know. Therefore, Lieberson’s conclusions about the limits of small-sample testing still apply with special force to case studies: “Put bluntly, application of Mill’s methods to small-N situations does not allow for probabilistic theories, interaction effects, measurement errors, or even the presence of more than one cause” (Lieberson 1992). Actually, Eckstein admitted as much in 1975; he merely asserted that adequately measurable, deterministic, monocausal relationships were not so rare in comparative politics.

In all subsequent chapters of this book, I will summarize the findings about democratization uncovered by each approach. I will not do so with respect to histories and case studies, however, for the reason I have already given: they do not lend themselves to generalization. The conclusions of case studies of democratization in Britain, Ghana, Russia, Taiwan, Peru, or any other single case usually were not intended to be stated out of context. And even if some authors misguidedly presented them as general lessons, we should not interpret them as such because they could be taken as true only under implicit conditions that need to be made explicit. None of this should detract from the worth of case studies which, again, are the sources of our most rigorous explanations of the immediate causes of specific events. If you want to understand the birth or death of democracy in one country, read a case study. Better yet, read a history. But no one is now in a position to summarize the conclusions of these studies along with all the contextual conditions attached to each conclusion that are needed to avoid overgeneralization. If someone were in a position to do this, we would have a better grasp of the kind of general theory toward which we strive: a kind of theory that becomes more complex, not less, as it becomes more general.

The third vice that case studies and histories share—in addition to myopia and an inability to generalize—is a tendency to “capitalize on chance,” i.e., to confuse mere coincidences and spurious
relationships with causal relationships. This kind of confusion is always a danger when alleged causal
relationships are not tested with evidence different from the evidence that originally suggested the
relationship. A good illustration is the claim made by some specialists on U.S. politics that the
ingenious design of the U.S. constitution is responsible for the longstanding success of democracy in
the United States (Calabresi 2001; Diamond 1959). As long as attention is confined to the United
States, it is very hard to challenge this notion, especially when it is easy to come up with examples of
potentially regime-threatening conflicts that were resolved according to constitutional procedures.
One way to rule out coincidences would be to carry out a truly comparative analysis. Including Latin
American cases in the study would quickly cast doubt on the importance of the U.S. constitution,
because most Latin American constitutions since the 1820s have incorporated most of its major
features—presidentialism, bicameral legislature, supreme court, and in many cases federalism as
well—yet Latin America is known for the instability of its democratic regimes. The constitution alone
is clearly not a sufficient explanation (Riggs 1988).

Different evidence need not come from other countries. In principle, a scholar could base a
hypothesis on one set of observations from one country and then test it with a different set of
observations from the same country (King et al. 1994, 223-8). There are a great many within-case
research designs, which the field of American politics exploits to the hilt—surveys, comparisons of
states, analysis of time series, and so on—and all of these are available for case studies in comparative
politics as well. Still, an insuperable problem arises when the outcome and its alleged cause are
invariant. In order to test a hypothesis, the cause and the outcome must vary. This is the only way to
tell whether they covary, and covariance is an essential attribute of causal relationships. If changes in
Y do not correspond to changes in X, we can rule out a causal relationship; but if X and Y do not
change, we can never be sure. Maybe Y would have happened without X; maybe Y could change
even if X did not. Until X or Y changes, these possibilities are just hypothetical guesses. We can tell
different stories about their relationship—spin different theories—but it all remains hypothetical until
we can observe actual change.6

Because every country has many relatively constant, unchanging features, and because
academics are highly intelligent and creative people who can dream up causal connections among
them without breaking a sweat, case studies and histories tend to identify ostensibly important
invariant “causes” without providing us with any means for judging their relative importance, or
whether they are important at all. Why, for example, has Britain been a democracy without
interruption for so long? Is it because it is an island separate from the European mainland, which
lessened the need for a standing army that might also have been used to repress its own citizens? Is
it because the English variety of feudalism and the early establishment of a limited monarchy created
traditions and cultural expectations that quickly defeat attempts to encroach on democratic rule? Is it
because England was the first country to industrialize? All of these characteristics were acquired so
far in the past that today they are constants, so as long as democratic survival remains a constant as
well, it is very difficult to show that they are or are not causes of British democracy. Other examples
of invariant causes are geography, climate, or “national character.” Today we rarely turn to them for
our explanations, but all these factors lead to the same problem.

The same problem arises in assessing explanations for the long-term absence of democracy in,
for example, Egypt. Maybe it is the lack of any prior experience with democracy; maybe it is that
Egypt has a majority Muslem society; maybe it is the Egyptian state’s reliance on oil exports; maybe
it is Egypt’s colonial past. Again, there is no way to be sure, because neither these possible causes
nor the lack of democracy vary in Egypt. Similar arguments have often been made about entire
regions, not just countries: democracy is associated with Western Europe’s feudal past, Christianity,
or being in the “core” of the world economy, while weak or absent democracy is associated with non-European attributes such as Islam, Confucianism, or being in the “periphery” of the world economy (Huntington 1993; Wallerstein 1974). One could even extend this kind of reasoning to explain chronic regime instability in, for example, Peru. Democracy has rarely lasted long in Peru because it was an important part of the Spanish colonial empire; because a deep ethnic and linguistic cleavage divides the coast from the sierra; because a decades-long feud between the populist APRA party and the armed forces has never been resolved; or because El Niño periodically wreaks havoc with the economy and the environment.

Actually, hypotheses of this sort can be tested, but only if we have variation of some sort. It could be variation among subnational regions, as in Putnam et al.’s *Making Democracy Work* (Putnam, Leonardi, and Nanetti 1993); it could be variation over time, if a sufficiently large time span is considered; or it could be variation among individuals, if their opinions are relevant for the hypothesis and a survey can be done. It is also possible to break down the dependent variable to expose its inner clockwork—the causal mechanism—so that we can observe how its components move in synchrony. But these alternatives serve to reinforce the point that if cause and effect truly are static and unchanging, their relationship cannot be tested unless and until new evidence is found.

What can we say, then, about the kind of knowledge that we find in histories and case studies? First, they produce unparalleled descriptive knowledge in the form of concepts, facts, and narratives. Within the discipline, description is valued less than causal inference, but this is unjust. Good description is hard, it is an essential foundation for causal inference, and it is probably valued more by those outside the discipline than our attempts at building causal models (Schedler 2003). Furthermore, this descriptive knowledge tends to be custom-made for each case and therefore fits exceptionally well. Second, histories and case studies also attempt to model causes, but do so with
three inherent limitations. Their causal arguments are biased in favor of short-term, micro-level causes; the arguments are unlikely to translate easily to other times and places; and any static causes remain at the level of hypotheses rather than tested propositions. Histories and case studies are great ways to develop ideas about things that may matter generally, but cannot show that they do matter generally.

**The Nature of Comparative Histories**

Comparative histories are intended to overcome the limitations of histories and case studies while retaining some of their advantages. By analyzing and comparing a few cases in some detail, comparative histories try to avoid myopia, move toward generalization, and test hypotheses against fresh data. Compared to most other approaches, comparative history is a method that employs relatively thick concepts and develops relatively thick theory, albeit not as thick as the concepts and theories in case studies or histories. It tends to reject the goal of universality, but strives to generalize within geographically and historically bounded domains. Comparative historians often claim to be developing an integrated theory that is accumulating knowledge, as evidenced by a long tradition in which successive researchers address and revise the conclusions of their predecessors. Indeed, the roots of comparative history run so deep that it could be said to be the original approach of comparative politics. Comparative historians rightfully claim Tocqueville, Marx, Weber, and Durkheim as their ancestors. As structural-functionalism and behavioralism rose in the 1960s, comparative history temporarily fell into disfavor. However, inspired by Barrington Moore’s *The Social Origins of Dictatorship and Democracy* and joining in the reaction against structural-functionalism, comparative history achieved a resurgence. Moore's thesis that privileged classes would maintain a non-democratic regime unless they were swept away by a revolution was enthusiastically embraced by many scholars in the radicalized intellectual environment of the late 1960s (Laitin forthcoming).
Theda Skocpol, Charles Ragin, and others developed methodological justification for the approach in the 1980s, and by the 1990s comparative history became a more self-conscious and vigorous approach (Evans, Rueschemeyer, and Skocpol 1985; Ragin 1987).

Taking stock of comparative historical analysis in 2003, James Mahoney and Dietrich Rueschemeyer defined the approach by stating that "all work in this tradition . . . share[s] a concern with causal analysis, an emphasis on processes over time, and the use of systematic and contextualized comparison" (Mahoney 2003). This definition differentiates comparative history from, respectively, descriptive histories, cross-sectional comparisons of static observations, and efforts to develop universalistic theories. This definition characterizes well a series of books on democratization and state-building in Western Europe that took Moore's *Social Origins* (1966) as its starting point. The major works in this series include Skocpol's *States and Social Revolutions* (1979), Luebbert's *Liberalism, Fascism, or Social Democracy* (1991), Downing's *The Military Revolution and Political Change* (1992), and Ertman's *Birth of the Leviathan* (1997). This literature spread to Latin America with Collier and Collier's *Shaping the Political Arena* (1991) and has since given rise to several important books that transcend any single region and focus more narrowly on democratization: Rueschemeyer, Stephens, and Stephens's *Capitalist Development and Democracy* (1992), and Ruth Collier's *Paths Toward Democracy* (1999).

A superficial glance at the comparative historical literature suggests that it has developed in a cumulative way, gradually elaborating and repeatedly testing a coherent theory of democratization that applies to an expanding set of countries. Figure 5.1 sketches out the major regime types (or stages of regime-building) found in the Western European countries that have been the principal focus of this literature, and the time periods in which they predominated. The common starting-point for all of them was the Holy Roman Empire, founded in 936 A.D. When the Holy
Roman Empire began to disintegrate in the 15th century, it left behind many small feudal states. Some of these remained small feudal states for centuries; others were agglomerated into larger, consolidated national states. This is the first major division explained in this literature. The second bifurcation divided consolidated states that evolved into absolutist regimes in the 17th and 18th centuries from those that preserved a rough balance between crown and nobility, the rights of towns and guilds, and mutual obligations between landlords and peasants—-a situation that Downing calls "medieval constitutionalism." By the 19th and early 20th centuries, these states parted ways a third time, either establishing the responsibility of the executive to parliament and expanding the suffrage to become liberal democracies, or undergoing a social revolution. Many of the liberal democracies lasted only a short time before they became social democracies (in Luebbert’s terminology) or fell prey to fascism. Finally, democratic regimes either survived or gave way to authoritarian regimes. In some countries there were repeated cycles of democracy and authoritarianism. The Latin American countries covered in the works discussed here followed a different path until the 20th century. All of them were originally European colonies, but when they became independent in the early 19th century, their consolidation as national states was delayed by several decades of civil war or dictatorship. However, the Latin American cases fit well into Figure 5.1 from about 1900 onward, oscillating between democracy and authoritarianism.

The claim that comparative histories constitute a cumulative research program is bolstered by their overlapping historical coverage. Figure 5.2 superimposes on the framework of Figure 5.1 the domains covered by the eight comparative histories discussed in this chapter. Downing (1992) and Ertman (1997) cover approximately the same periods and propose answers to the same questions (among others): How did the remnants of the Holy Roman Empire become consolidated as states and why did some become absolutist regimes while others resisted absolutism? Moore
(1966) and Skocpol (1979), though published earlier, study the later political transformations of either absolutism or constitutionalism into social revolution, fascism, or liberal democracy. Luebbert (1991) focuses on transitions from constitutional or proto-democratic regimes to liberal democracy, social democracy, or fascism, ignoring social revolution, which was missing in his exclusively inter-war European sample. The Colliers (1991), in turn, omit social democracy and fascism (both missing from their Latin American sample) to focus on transitions between authoritarianism and democracy. The most sweeping study is that by Rueschemeyer, Stephens, and Stephens (1992), which covers the gradual early democratization of Europe, recent transitions in Latin America and the English-speaking Caribbean, and fascist interruptions of the early 20th century. Therefore, while each study considers a different slice of history, collectively these comparative histories provide continuous coverage of the last half-millenium in Western Europe and Latin America.

Despite the considerable overlaps, these works contain too many fundamental differences to treat them as a fully integrated research program. Most fundamentally, they attempt to explain different outcomes. Obviously, only the works that extend into the years since about 1920 (when most European countries extended the suffrage to all adult males) really attempt to account for what we now consider democratic regimes. Moore and Skocpol barely reach this period, and only for some of their cases. Downing and Ertman seek to explain only institutions or traditions that were precursors to democracy, such as the balance between crown and nobility and the rights of towns and peasants. But the differences in outcomes are not solely due to different historical foci. Even authors dealing with the same period seek to explain different outcomes, and democracy is not always the outcome of chief interest. While Downing is primarily concerned with representative assemblies and other institutions that predisposed Europe toward democracy, Ertman is equally interested in explaining the type of regime (constitutional or absolutist) and the type of state
Moore was interested in three different kinds of "revolutions": the bourgeois revolution (allegedly leading to democracy), the "conservative revolution from above" leading to authoritarianism, and the peasant revolution leading to communism. Skocpol was primarily interested in explaining social revolutions; democracy was for her just one of several possible outcomes in a residual category of non-revolution. Of all the scholars of more recent periods, only Rueschemeyer, Stephens and Stephens are exclusively interested in explaining democracy. Luebbert distinguished between liberal democracy and social democracy—economic systems as well as political regimes—and contrasted both with fascism. The Colliers, in *Shaping the Democratic Arena*, are ultimately interested in explaining democratic survival (as an expected outcome of institutionalized party systems that incorporate labor in a peaceful way), but only in the final chapters. Along the way, they spend considerable effort explaining prior outcomes—the characteristics of parties, the mode of incorporation of labor, the nature of labor laws, and the strength of labor and the oligarchy. In her later (1999) book, Ruth Collier is actually interested in accounting for variations in the role of labor in democratization rather than for democratization per se.

Comparative histories have also disagreed about which actors matter. Skocpol took Moore to task for merely paying lip service to the notion of the state as an autonomous actor (Skocpol 1973). The same criticism could be made of Luebbert and perhaps Ruth Collier’s (1999) book. Skocpol, Downing, Rueschemeyer et al., and Ertman emphasize foreign states as important actors; Moore, Luebbert, and the Colliers do not. Similarly, authors disagree about the democratic orientations of key actors. Moore (like most of his predecessors and contemporaries) associated democracy with the interests of the bourgeoisie; if the bourgeoisie is defined to include the gentry and the urban middle class, Downing and Ertman would agree. Luebbert argued that only some of
the middle class supported democracy, and only in some cases, as the outcome depended on the middle class’s choice of allies. The central argument of Rueschemeyer, Stephens and Stephens, however, is that the working class was a far more consistent friend of democracy than the middle class, except in South America. This dispute was the inspiration for Ruth Collier’s book, which identified seven different roles played by labor in democratization, some of which were supportive and others, not.

Comparative histories have varied most with respect to the set of explanatory factors or variables that they emphasize. The only factor shared by all of these works is the assumption that the economic interests of social classes played an important role. Barrington Moore’s work is the purest expression of this; the others add other variables to social class. As noted before, Skocpol added state interests and international relations to the mix, initiating a tradition of centering comparative historical analyses on class-class, class-state, and state-state relations (Mahoney 2003:151). Luebbert, however, went beyond the class cleavage to consider pre-industrial religious, regional, linguistic, and urban-rural cleavages. Downing emphasized, in addition to class, medieval institutions and traditions, geography, military technology, trade, and colonial resources. The other works refer to a still richer variety of explanatory factors, including commodity specialization, immigration, leadership, modes of incorporation of labor, civil war, short-term economic performance, transnational political ideas and polarization, the nature of the antecedent regime, and coalition politics.

Although these works do not present a coherently integrated body of theory, they can be said to share a “metatheory”—a looser set of assumptions—as James Mahoney has noted (Mahoney 2003:132-7). All comparative historical researchers make three fundamental assumptions, and a subgroup makes a few more. One fundamental assumption is that big events such as revolution,
state-building, and democratization can be explained. Not all social scientists agree with this. Barbara Geddes, for example, has argued that big events are too complex to explain and that we must focus on the microfoundations of politics if we are to make any progress (Geddes 1997). A related assumption is that big events have big (structural) causes. Most scholars who refer to "structures" mean class structure, but other social structures such as religious or ethnic cleavages can be included, and I would argue that other large-scale, slowly-changing features such as physical geography, commodity specialization, and state capacity qualify as structures as well. Comparative historians do not claim that all causes are structural, but their belief that some causes are structural sets them apart from those who are exclusively interested in the effects of mutable institutions, culture, or strategic calculations of elites.

A third fundamental assumption is that it is meaningful and useful to treat social classes as actors. Some scholarly traditions long ago decided that "class" is not a useful concept except in the rare cases where a social class is conscious, cohesive, and conspiratorial (Parry 1969: ch. 2). Comparative historians tend to assume that members of classes do have a common economic interest, that they are aware of their shared interests, and that they collectively pursue their common interests. However, unlike primitive Marxians, comparative historians typically qualify these assumptions in two respects. First, they tend to divide classes into more precise, and therefore more homogeneous, categories. Instead of writing about capitalists in general, they distinguish among agricultural landlords, industrialists, and finance capital, and often sub-categories of these. Instead of the bourgeoisie, they distinguish among the rural middle class, urban professionals, merchants, civil servants, and so on, often to the point of reducing classes to a set of occupations. Second, comparative historians now tend not to assume that collective action in pursuit of common interests comes naturally. Therefore, important supplementary actors in their analyses are class-based.
organizations such as labor unions, political parties, and interest groups, which mobilize their members in the defense of the interests that they define on behalf of the class. These two modifications of the class-analytic approach do make it more useful. Taken to the extreme, it would become indistinguishable from the interest-group politics approach, but most comparative histories remain at a slightly more abstract level.

A sub-group of comparative historians shares several additional assumptions that help define the approach. After a long intellectual evolution from the Marxian dogma that the state is merely "a committee for managing the common affairs of the whole bourgeoisie," most contemporary comparative historians now assert that states are autonomous actors with interests of their own, apart from the interests of a dominant class (Marx and Engels 1932). States guard their territories from foreign encroachment and try to maintain public order, and to these ends they raise armies and revenues, staff bureaucracies, and gather information about their societies in order to make their task of governing easier and more efficient. This group also recognizes the impact on domestic politics of international forces such as war, trade, immigration, and revolutions in other countries. This international perspective may seem natural for scholars who take an expansive, long-term, structural view of politics, but it is not notable in the work of Barrington Moore or Gregory Luebbert, so it is most properly a sub-group characteristic.

The earlier comparative histories tended to employ John Stuart Mill’s “methods of similarity and difference” to test or buttress their claims. Those using Mill’s method of difference would choose cases that had as much as possible in common except for their differing outcomes, in effect holding the common characteristics relatively constant. These scholars could then argue that the different outcomes would be best explained by the remaining characteristics that differed. The method of similarity follows the opposite logic: choose cases that experienced similar outcomes
despite being different from one another in as many respects as possible. Using the method of similarity, scholars argue that the similar outcomes must be attributed to the reduced set of remaining similarities. Somewhat confusingly, Mill’s “method of difference” is the same as Przeworski and Teune’s equally well-known “most similar systems” design, and his “method of similarity” is identical to their “most different systems” design. Nevertheless, the logic is the same: differences explain differences and similarities explain similarities. This was the common logic of most comparative histories before 1990.

Since about 1987—the year of publication of Charles Ragin’s *The Comparative Method*—comparative histories have preferred a path-dependent model of politics, which is incompatible with Millian comparative logic. Peter Hall has insightfully described this approach as one that sees "the world not as a terrain marked by the operation of timeless causal regularities, but as a branching tree whose tips represent the outcomes of events that unfold over time" (Hall 2003:385). This approach is built around the key concepts of endogeneity and interactions. Comparative historians since 1990 have believed that outcomes occur in sequences; therefore, outcomes at one time become causes of other outcomes at later times. This is what it means for variables to be "endogenous": they are both causes and effects. There are interactions when causes combine to produce an outcome; and because the effect of one cause depends on the presence of another cause, it is not useful to speak of the independent impact of either cause separately. Recent comparative histories emphasize both endogeneity and interactions. As Hall notes, "[t]he prototypical contention is that the impact of \( x \) will depend on whether it occurs before or after \( w \)” (Hall 2003:385).

This is a much more complex kind of model than a simple list of variables that lead to an outcome, and more complex than models with either endogeneity or interactions but not both. Figure 5.3 contrasts a path-dependent model with simpler models. Model 5.3a is a basic 2-variable
causal model in which continuous variables A and B cause outcome Y. The same model could be written in equation form as \( Y = A + B. \) Model 5.3b contains an interaction between A and B that helps cause Y; or, in equation form, \( Y = A + B + A \times B. \) Model 5.3c incorporates endogeneity by distinguishing between an initial outcome \( Y_1 \) and a subsequent outcome \( Y_2. \) (Typically A and B would vary with time as well.) Model 5.3c also incorporates the assumption that different variables matter at different stages. In this case, C helps cause \( Y_1 \) but not \( Y_2, \) and D helps cause \( Y_2 \) but not \( Y_1. \) Representing endogeneity algebraically requires a separate equation for each outcome: in this case, \( Y_1 = A + B + C \) and \( Y_2 = Y_1 + A + B + D. \) Model 5.3d combines the endogeneity of Model 5.3c with the interactions of Model 5.3b. It can be written as \( Y_1 = A + B + A \times B + C \) and \( Y_2 = Y_1 + A + B + A \times B + D. \)

Model 5.3d meets Hall's criteria for path dependence, but he made his definition so general that it includes strategic bargaining models and some complex statistical models. To narrow the definition to the comparative histories discussed in this chapter, a further stipulation is necessary: the models must have nominal outcomes and at least one nominal cause. To represent this feature in Figure 5.3e, we must alter the notation. Therefore, B and \( Y_1 \) are no longer continuous variables, but dichotomies. Now the upper-case "B" signifies that condition B is present, and the lower-case "b" signifies that condition B is absent; similarly for \( Y_1 \) and \( y_1 \) ("not \( Y_1 \)”) and for \( Y_2 \) and \( y_2 \) ("not \( Y_2 \)”). In this example, \( Y_1 \) occurs only if A and B are both present in the first time period; if either A or B is absent (signified by “a” or “b”), then the outcome is “not \( Y_1 \)”, i.e., \( y_1. \) (The branching from A to B to Y, touching every combination of values, substitutes for the multiplicative interactions in other models.) In the second time period, however, \( Y_2 \) is determined by a different rule. If \( Y_1 \) had already been achieved, then \( Y_2 \) occurs if either A or B is present; but if \( Y_1 \) had not been achieved, then \( Y_2 \) occurs only if both A and B are present. This argument can be expressed mathematically with
Boolean logic:

If \([Y_1 \text{ and } (A \text{ or } B)] \text{ or } [\neg Y_1 \text{ and } A \text{ and } B]\), then \(Y_2\);

If \([Y_1 \text{ and } \neg A \text{ and } \neg B] \text{ or } [\neg Y_1 \text{ and } (\neg A \text{ or } \neg B)]\), then \(\neg Y_2\).

It is the qualitative nature of the variables that produces the “branching trees” typical of path-dependent models, as in figure 5.3e. In such models, we cannot pin down a consistent effect of A, B, or \(A\times B\) on \(Y_2\) because they may or may not be associated with \(Y_2\), depending on whether \(Y_1\) was previously achieved or not. Figure 5.1 implies a good example of a path-dependent argument: if a consolidated national state forms, then other conditions can eventually produce democracy there; but if there is no consolidated national state, then these other conditions do not produce the same democratic outcome. However, as in Figure 5.3e, outcomes need not branch away forever. In many path-dependent models, divergent paths can later converge. In this way, path-dependent models often identify multiple paths to a single outcome.

**Evaluation of the Comparative Historical Approach**

The preceding section described the nature of comparative history as an approach that is, by varying degrees, thinner than case studies and histories but thicker than most other approaches in comparative politics. It is also more theoretically integrated than most case studies, but only loosely integrated in the sense of sharing some metatheoretical assumptions; and therefore less integrated than approaches that derive propositions from a common well of formal theory or that repeatedly test the same hypotheses with slight modifications and different samples. Comparative history also aims for an intermediate level of generality, shunning both particularism and universalism in favor of the middle-range aspiration of generalizing about a century or two on a continent or two. Therefore, on all three criteria for a good theory--thickness, integration, and generality--comparative history is a methodological compromise. Its advocates claim that this makes the approach the best of all worlds,
at least the best that we can actually achieve. As with any compromise, however, it is also possible that we are left with worst of all worlds. A careful evaluation of the approach is therefore in order.

Comparative history is a very good approach for identifying and testing possible causes of specific events in specific cases. Unfortunately, it is not as rigorous in doing this as case studies and histories are, as they bring greater conceptual richness and much more evidence to bear on such questions. Comparative histories do have one advantage over case studies, however. They are less myopic: more likely to call attention to structural, macro causes.

However, comparative histories are less successful than sometimes claimed at moving beyond specific events to develop middle-range theories. In their attempt to generalize to the middle range, comparative histories typically encounter five problems. (1) Their concepts are slippery and inconsistently applied. (2) They do not adequately integrate the theories developed for small domains into a single theory for a larger domain. (3) The page limits on what presses will publish and readers will read constrains the number of cases and the number of variables that any comparative historian can include. (4) Comparative histories almost always end up with more variables than cases, rendering any testing indeterminate. Finally, (5) comparative historians always amend their theories as they go along and never test the amended theory with different evidence, and this habit virtually guarantees capitalizing on chance. I will discuss each problem as it manifests itself at three levels: in the typical work of comparative history, where the problems are often minor; in the collective body of comparative historical research, where problems are most serious; and in the potential for comparative history as an approach.

The rich conceptualization that is typical of case studies and histories becomes a burden when scholars attempt to compare cases. It becomes impossible to respect the uniqueness of each case and necessary to work with simpler, less precise concepts. Comparative historians must write
about the upper chamber of parliament instead of the British House of Lords; about the working class instead the nitrate miners of northern Chile. Moving up "the ladder of abstraction" is a tricky business that requires the scholar to define an umbrella concept that captures the common characteristics of diverse times and places without ignoring other characteristics that may be causally relevant. It is hard to do this well, so comparative historians often fall short in one of three ways. First, some define a concept vaguely and then use it even where it does not really fit, which Sartori (1970) called "concept stretching." Ertman, for example, cites historical research showing that earlier comparative historians wrongly classified early modern England as a patrimonial state. In fact, England had a more highly developed bureaucracy than Prussia did (Ertman 1997:??). Second, some scholars define a concept in a much thinner, essentialist way but then tack on different causally relevant qualifications in some of their cases. In this situation, the concept hides important variations, creating an illusion of general applicability. For example, Brian Downing classified both France and Brandenburg-Prussia as having developed "military-bureaucratic absolutism," but also says that one reason that absolutism survived in Prussia while succumbing to revolution in France was that France's absolutism was less absolute (Downing 1992:127-32). Third, some comparative historians simply use vague concepts, leaving it up to the reader to guess how they should be applied. Readers can search in vain for a concrete definition of concepts such as "a rough balance between the crown and the nobility" (Moore), "economic dependence" (RSS), "bureaucratic state" (Ertman), "bourgeoisie" (Moore, Skocpol), "labor-repressive agriculture" (Moore), or "solidarity of peasant communities" (Skocpol), even though the truth of core arguments depends on how these concepts are defined.

This problem is found even in individual works, which we should expect to be internally consistent. There is quite a bit of variation in conceptual clarity in the genre. Ruth Collier's Paths
Toward Democracy (1999) stands out as unusually clear and consistent in its definitions, while Moore's Social Origins (1966) is frustratingly muddied. However, the slippery or inconsistent use of concepts is much more of a problem for comparative history as a whole due to the lack of agreement among its practitioners. One need not take an outsider's word that this problem exists. Any comparative history selected at random will contain at least one lengthy discussion of concepts and classification that disputes the decisions made by other comparative historians. Thus we find that Ruth Collier classifies Weimar Germany as an episode of successful democratization while Moore, Luebbert, and Ertman consider it a case of rising fascism (Collier 1999). Skocpol considered Moore's concept of "labor-repressive" agriculture not to be useful and criticized Moore for using his key concept of the "bourgeois impulse" in inconsistent ways that glossed over important differences in the strength of the bourgeoisie (Skocpol 1973:??). And what are we to make of the fundamental disagreement between Ruth Collier and Rueschemeyer, Stephens and Stephens? The latter argue that the working class was the primary actor advancing the cause of democracy in Western Europe, while Collier argues that labor organizations were not important actors in early democratization episodes in Greece, France, Portugal, Spain, Switzerland, Norway, or Italy (Collier 1999: 23-32; Reuschemeyer, Stephens, and Stephens 1992). The difference is a conceptual disagreement about whether the inclusion of the working class must be accomplished before the outcome deserves to be called "democratization." Rueschedmeyer et al. believe not, so they disqualify early steps toward democracy that Collier considers relevant.

The second problem faced by comparative histories is the difficulty of integrating their findings with other comparative research. This problem affects both the integration of several comparative histories into a larger, cumulative, comparative-historical research program and the integration of the findings from each case into a single work of comparative history. The integration
problem at the level of the research program is best understood as a difficulty in drawing a sample that is comparable to the larger population of cases. If the cases selected for comparison are comparable to all the relevant cases that could have been chosen, then one would be justified in treating the findings of the comparison as complete, finished hypotheses ready for application to other cases in the larger population. This is sometimes called the principle of “unit homogeneity”: the same cause has the same effect in all the cases being analyzed (King, Keohane, and Verba 1994:91-4). But if the selected cases are systematically different from the larger population in any relevant way, then the comparisons will suffer from selection bias. In a large sample with cases selected at random (in a public opinion survey, for example), this is not a problem because the sample differs only randomly, not systematically, from the population. The non-comparable aspects of the sample can be assumed to cancel one another out, so that on average the sample is representative of the population from which it was drawn. Unfortunately, a few dozen cases are needed in order for the benefits of randomization to be realized, so this solution is not available to comparative historians (King, Keohane, and Verba 1994:124-8). For them, selection bias is unavoidable. This should be especially obvious with regard to comparative histories that focus (as most do) on a particular geographic region or a particular period of history precisely because it is systematically different. There is a price to pay for the advantages of middle-range theory.

Fortunately, selection bias is—in principle—correctable, but only if we know the nature of the bias. There are several possibilities. First, the selected cases may have some fixed advantage or disadvantage in achieving the outcome; for example, Latin American countries since the mid-1970s tend to be more democratic than non-Latin American countries at the same level of socioeconomic development in this period (Coppedge 2004). If we know why these countries have this bonus or penalty, we can add an extra wrinkle to the theory to take it into account. But even if the reason is
unknown, it is better to know and acknowledge such systematic differences than to pretend that they do not exist. Taking these differences into account makes sample comparable to the population, at least in this respect. Another kind of bias occurs when the same cause has a more powerful or a weaker effect in the selected cases than it does in other cases. A good example is the belief that U.S. foreign policy has a more powerful impact on Central America than it does on the Southern Cone of South America. Again, if we knew the reason for this varying effect—say, the size and distance of each country from the U.S.—then we would have an improved theory that would travel more easily beyond the region. Even lacking a good explanation, it is better to state that a process is different in this region than in that one. This leaves us with a “multiple paths” kind of theory that applies equally well to both regions. Finally, it is possible that the selected cases are harder or easier to explain than the ones not selected: the outcomes just vary more. The more apparently random variation there is in the outcomes, the more likely it is that the impact of a cause will get lost in the noise, leading scholars to conclude, falsely, that the cause has no effect. In such situations, the appropriate correction is to adjust the standards for each set of cases so that a big change in the less predictable cases is treated as equivalent to a small change in the more predictable cases. For some purposes, for example, a failed coup attempt in Northern Europe could be equivalent (i.e., as traumatic, as severe a regime crisis) to a successful coup in Sub-Saharan Africa.

In all these situations, it would be preferable to identify explicitly how the cases selected for a comparative history differ systematically from the rest of the world. When this is done, then we have some guidance about how to integrate the findings of one comparative history with the findings of other comparative histories. Failure to do so biases the conclusions of a study in unpredictable ways, rendering effects more or less optimistic than they would be in a larger sample, causes more or less powerful, and confidence in conclusions either inflated or deflated.
Without the explicit guidance needed to make the different sets of countries comparable, the best we can do is circumscribe the relevance of the findings to just the times and places actually studied. Although this is enough to satisfy most comparativists today, it is important to keep in mind how far it falls short of the covering-law model of explanation, which requires universal laws. If an explanation is limited to particular times and places for arbitrary (i.e., unspecified) reasons, then it is an incomplete explanation, at best. Of course, it is unrealistic to expect comparative politics, still in its infancy as a science, to produce complete explanations. Producing incomplete explanations that work well for certain middle-range regions and periods is without doubt a useful first step along the road to complete and therefore universal theories. In one influential view, what we learn by testing is not so much whether a theory is true or false but “how much of the world the theory can help us explain” (King, Keohane, and Verba 1994:101).

However, even at this early stage it is essential for comparative historians to make the reasons for their selection decisions explicit. Otherwise, even the smallest differences in case selection make it impossible to cumulate knowledge in a coherent research program. We have already seen that the major comparative historical works involving democratization have used different, though overlapping, sets of cases. This makes it all the more important for every author to point out how the selection might have influenced the results. Downing (1992) is a good model for others to follow. He explicitly identifies many differences between Europe and other regions:

To say that European social, political, and economic history is markedly different from that of the rest of the world is to say nothing new. The West was the first to develop innovative agricultural techniques, large-scale capitalist production, and a system of states. Europe was also the first—and, unfortunately, virtually the last—to develop democratic political systems that featured institutional checks on political monopoly, varying but frequently increasing
degrees of political representation, chartered rights of citizenship, and the rule of law
(Downing 1992:18).

He then offers brief case studies of Russia, Japan, and China to show that these non-western
countries “never developed constitutional government as found in late medieval Europe. Structural
configurations conducive to constitutionalism in the West, rough balance between crown and noble,
contractual-feudal military organization, and lord-peasant dynamics were absent or weak” (Downing
1992:53). Moore limits his conclusions to large, powerful states to the exclusion of smaller,
dependent states on the grounds that “the decisive causes of their politics lie outside their own
boundaries” (Moore 1966:xiii). This is brief and debatable, but at least he addressed the issue.

Many other authors discuss this issue in only the most cursory ways, if they address it at all.
The Colliers (Collier and Collier 1991:5), for example, merely note that “The present study parallels
the concerns of various analysts of Europe who have viewed the incorporation of the working class
as a pivotal transition within this larger process of societal change,” without identifying the
differences between the European and Latin American experiences. All of their discussion of case
selection deals with selection within Latin America. Skocpol (1979:40-2, 302, n97) includes a
section entitled, “Why France, Russia, and China?” but misses the point. She defends the selection
of these three cases from different regions and periods against charges that they are not comparable
to one another but neglects to address how they might not be comparable with other cases.
Elsewhere she does recognize that “there always are unexamined contextual features of the
historical cases that interact with the causes being explicitly examined in ways the comparative
historical analysis either does not reveal, or must simply assume to be irrelevant” (Skocpol 1979:39)
but does not attempt to identify these features in her analysis. Given the vague and inconsistent
justifications for case selection, we cannot assume that different comparative histories study
comparable sets of cases. This is an obstacle to the cumulation of findings in the comparative historical research program.

Achieving comparability across temporal domains is as important as comparability across spatial domains. Comparative historians should tell us not only what is systematically different about the territories they study, but also what is different about the historical period. This kind of guidance is necessary for unifying theories inspired by different waves of democratization. For example, it probably matters a great deal that “first-wave” democratization in Britain took place without a model to follow (until 1776). Democracy was not a familiar goal to achieve, but a set of evolving institutions and practices that people invented as they went along. Transitions after World War II had many models to emulate, and this is probably an important reason that transitions are much faster now.

Individual works of comparative history must also demonstrate the comparability of the cases they include. Because cases in one region during one period are relatively similar, this is usually less difficult than ensuring the comparability of one set of cases to all other sets of cases. However, since comparative historians are primarily interested in accounting for the differences among the cases they have selected, they do a much better job of addressing this problem. Perhaps comparative historians could be faulted for not accounting for the differences among their cases exhaustively. In practice, they present a simple set of explanatory factors that most efficiently sorts the cases into the possible outcomes. An exhaustive analysis would identify all the differences among cases and would leave us with multiple, sometimes competing, explanations. However, this would be expecting each comparative history to do all the work required of the entire collective comparative-historical research program over a long period of time; it is expecting too much.

Nevertheless, contemplating the demands of rising to this challenge exposes the third
problem that comparative historians encounter: the practical limits to the complexity of any analysis. Even if a scholar or teams of scholars were interested in coming closer to exhaustive explanations of differences among a larger number of cases, no press would publish their massive writeup. And even if a press would publish it, few readers would actually read it. There is therefore an unavoidable tradeoff between the thickness of an analysis and the size of the domain it can cover. We can see this tradeoff in the works discussed in this chapter. Using crude indicators of domain size—the number of countries studied—and thickness—pages per country—we find *Shaping the Political Arena* at one extreme, with 877 pages for 8 countries, or an average of 110 pages per country. (Case studies, of course, are even thicker: several hundred book pages per country.) Skocpol and Moore are also at the high end, with 6-8 countries and about 70 pages per country. With 11-13 cases, Luebbert and Ertman slip to 28-38 pages per country. At the low extreme, Ruth Collier’s set of 22 countries and Rueschemeyer, Stephens and Stephens’s set of 34 countries weigh in at only about 11 pages per country. The larger the domain, the thinner the analysis.

The fourth problem of comparative history is indeterminacy. In a single-case study, many variables are held constant over time and the only variables necessary are those that explain variation within that case. In a comparative history, more variables are needed to account for cross-national differences. Comparative histories face the real danger of having more variables than cases—the “many variables, small N” problem. This is a problem because it virtually guarantees capitalizing on chance—mistaking spurious or coincidental associations for causal relationships. The relationships can be tested, but any tests would yield indeterminate conclusions.

If the cases had been selected at random (an important qualifier to which I will return below), the probability that the finding of a comparative historical analysis could have been produced by chance could be checked with Fisher’s Exact test. (The more familiar Chi-square test is
actually a large-sample approximation of this test. Some readers will be familiar with the Fisher test in 2 X 2 tables. The same test, although computationally difficult, can be performed with larger tables.) I have done this test for most of the works discussed in this chapter by summarizing their findings in tables, which are in the appendix to this chapter. Table 5.1 presents the results of the Fisher tests for the major comparative histories. Only two of them–Ertman and Luebbert–satisfied the conventional p<.05 criterion for statistical significance, mostly because the other tables were “sparsely populated,” as indicated in the “cases per cell” column. Chance results are harder to rule out when the table is large (i.e., when there are complex explanations for complex outcomes), the cases are few, or there are many exceptions to the pattern. Therefore, comparative histories, which tend to have complex explanations for few cases, are prone to chance results.

Table 5.1 suggests that even if the cases were chosen at random, it would not be possible to distinguish results this strong from sheer coincidence. But of course comparative historians do not select their cases at random; random selection is inappropriate when the sample is small. There are various alternative criteria for selecting cases for testing when the sample is small, some better than others. According to King, Keohane, and Verba (1994:142), “The most egregious error is to select observations in which the explanatory and dependent variables vary together in ways that are known to be consistent with the hypothesis that the research purports to test.” There are two ways to do this: selecting cases to fit the hypothesis, or selecting a hypothesis to fit the cases. I doubt that many comparativists deliberately select cases to fit a hypothesis and then present the comparison as a test; if they did, it would be research fraud. However, I think it is likely that some comparativists unconsciously select cases to fit their hypothesis. The temptation to present confirming evidence and sweep inconvenient cases under the rug is difficult to resist. Moreover, it is fine to do this as long as such evidence is presented as illustrations of how a hypothesis might work; but this is not
Comparative historians do, however, select hypotheses to fit their cases. In fact, they not only do it; they admit it and praise it as methodological virtue itself. Mahoney and Rueschemeyer (2003:13), for example, write that comparative historians can "...move comfortably back and forth between theory and history in many iterations of analysis as they formulate new concepts, discover novel explanations, and refine preexisting theoretical expectations in light of detailed case evidence." Actually, there is methodological virtue in selecting hypotheses to fit cases, but it is a virtue for building theory and generating hypotheses. Staying close to cases is probably the most efficient way to develop explanations that work well for the cases being studied. But selecting hypotheses to fit cases is not a virtue for testing. As far as testing is concerned, it is a corrupt practice. To the extent that scholars deliberately select hypotheses to fit their cases, or unconsciously select cases to fit their hypotheses, Fisher tests, which assume random selection, inflate the significance of the comparative evidence for comparative historical theories. Seen in this light, the strong significance levels for the Ertman and Luebbert models in Table 5.1 are not meaningful. Most comparative historians should drop the pretense that there is any truly comparative testing going on in their work.

Some will object that there is plenty of testing in comparative histories. In fact, I have already argued that comparative histories share the advantage of case studies in testing hypotheses about the immediate causes of specific events. Designing Social Inquiry admonishes us to multiply observable implications, to search for evidence that might disconfirm any of the many things that must be true if the theory is true (King, Keohane, and Verba 1994:28-9), and I agree that comparative histories have a home court advantage in this respect, even if case studies and histories have an even greater advantage. But this is testing of hypotheses about each case. What is missing is comparative testing of more general propositions that apply not just to one case but to all the cases in
the sample and to the larger population that the sample represents.

The only solution to this problem is to test the propositions that emerge from a comparative history with a different set of cases. In principle it would be possible for a comparative historian or a team of comparative historians to work out a model in one sample and then test its implications in a different, sufficiently large, and fairly selected sample. A few comparative histories seem to do this, including Moore (1966), Skocpol (1979), and Rueschemeyer et al. (1992). However, the fifth and final problem with comparative history is that its practitioners keep amending the theory while the "testing" is going on. If the theory building never stops, the testing cannot begin. Capitalist Development and Democracy, for example, could be said to develop its hypotheses in Western Europe and then test them in Latin America and the Caribbean. However, they modify their core thesis when applying it to South America where, it turns out, the middle class sometimes played a more important role in democratization than the working class. By making this amendment, the authors used the South American cases to develop new theory instead of testing the theory they already had.

One of the better comparative testing efforts is Ruth Collier's Paths Toward Democracy (1999), most of which is an out-of-sample test of the RSS thesis that the working class played a leading role in democratization. She finds the thesis wrong in a great many cases, and this is a fine test. However, this is the exception that proves the rule, because Collier then proceeds like other comparative historians: she amends the hypothesis drastically by defining seven distinct patterns of democratization, speculates about their possible causes, and never tests them all systematically or with different cases.

The logic of comparison for testing is weak in comparative histories. Most of their value for testing lies in the individual case studies, and even these are inferior to case studies and histories. As a method for generating hunches and building theory, comparative history may well be the best of
both worlds. But with respect to testing, it is the worst of both worlds.

Nevertheless, comparative histories have made useful contributions to the study of democratization. They have churned up relevant descriptive information, or at least brought it to the attention of non-historians. They have developed highly plausible explanations for the unique democratization paths taken by major countries. They have called attention to macro or structural factors, such as class conflict, technological change, international events, and war, that case study authors may have downplayed or taken for granted. They have cumulatively developed a framework or meta-theory in which the material interests of rising social classes spur conflict that culminates in the achievement of democracy. Comparative histories may have left the testing to others, but they have played a tremendous leading role in theory building.
Table 5.1: Fisher Tests of Comparative-Historical Models

<table>
<thead>
<tr>
<th>Author</th>
<th>Number of Cases</th>
<th>Explanatory Categories (rows)</th>
<th>Outcome Categories (columns)</th>
<th>Cases per Cell</th>
<th>Probability of a Chance Result</th>
</tr>
</thead>
<tbody>
<tr>
<td>Skocpol</td>
<td>6</td>
<td>6</td>
<td>2</td>
<td>0.50</td>
<td>.933</td>
</tr>
<tr>
<td>The Colliers (Heritage Period)</td>
<td>8</td>
<td>4</td>
<td>2</td>
<td>0.67</td>
<td>.393</td>
</tr>
<tr>
<td>Rueschemeyer, Stephens and Stephens</td>
<td>10</td>
<td>6</td>
<td>4</td>
<td>0.42</td>
<td>.271</td>
</tr>
<tr>
<td>Stephens (timing of initial democratization in Latin America)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Moore</td>
<td>7</td>
<td>6</td>
<td>4</td>
<td>0.29</td>
<td>.188</td>
</tr>
<tr>
<td>Rueschemeyer, Stephens and Stephens</td>
<td>10</td>
<td>6</td>
<td>2</td>
<td>0.83</td>
<td>.134</td>
</tr>
<tr>
<td>Stephens (rise of authoritarianism in Europe)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Downing</td>
<td>7</td>
<td>5</td>
<td>3</td>
<td>0.47</td>
<td>.077</td>
</tr>
<tr>
<td>Ertman (simple version)</td>
<td>13</td>
<td>4</td>
<td>4</td>
<td>0.81</td>
<td>.001</td>
</tr>
<tr>
<td>Luebbert</td>
<td>15</td>
<td>4</td>
<td>4</td>
<td>0.94</td>
<td>.000</td>
</tr>
</tbody>
</table>

Note: The table form of each author's model is in the appendix to this chapter. Ruth Collier's (1999) model of the impact of labor on democratization considers several hypotheses but does not integrate them into a single model. The probabilities of a chance result must be calculated for each specific table. A number of web-based calculators will do this for small tables. I used the one at http://www.physics.csbsju.edu/stats/exact_NROW_NCOLUMN_form.html.
Appendix to Chapter 5

This appendix summarizes the arguments and outcomes of the major comparative historical studies of democratization in tabular form. Each column of a table corresponds to one of the outcomes being explained; each row corresponds to the combination of explanatory factors that allegedly explain the outcome in a case or set of cases. The counts of the number of cases in each cell were used to calculate the probabilities in Table 5.1. Fisher's test sums the probabilities associated with all the other possible tables with the same dimensions and the same number of cases that show a stronger association between the causes and effects. Conventionally, if this sum of probabilities is less than .05, we feel confident in rejecting the possibility that the apparent association was a chance result.

Summary of Arguments and Outcomes in Skocpol (1979)

<table>
<thead>
<tr>
<th>Causal Conditions*</th>
<th>Social Revolution</th>
<th>No Social Revolution</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>ULBCATrISMV</td>
<td>France</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>ULBCATrISMV</td>
<td>Russia</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>ULBCatrISmv</td>
<td>China (by 1911)</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>ULBCATRismv</td>
<td>Prussia (west of Elbe)</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>ULBCATRismv</td>
<td>Prussia (east of Elbe)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>ULBCatrismv</td>
<td>Japan</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>ULbcATRismv</td>
<td>England</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>N</td>
<td>2</td>
<td>4</td>
<td>6</td>
</tr>
</tbody>
</table>

*Key:
Capital letters indicate the condition was present; lower-case letters indicate the condition was absent.
U: There was a powerful landed upper class.
L: The upper class had leverage over the state.
B: The state was semi- or highly bureaucratic.
C: The state was centralized.
A: Agricultural productivity was increasing.
T: There was a transition to capitalist agriculture.
R: The transition to capitalist agriculture included core regions.
I: International pressures were at least moderate.
S: Smallholding peasants own at least 30 percent of the land.
M: There is a strong peasant community.
V: Villages are fairly autonomous from central control.

Source: Skocpol (1979), parts A and B of Table 1, 155-6.
Summary of Arguments and Outcomes about the Timing of Initial Democratization in Latin America in Rueschemeyer, Stephens and Stephens (1992)

<table>
<thead>
<tr>
<th>Causal Conditions</th>
<th>None</th>
<th>Early</th>
<th>Medium</th>
<th>Late</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>No export expansion, labor-intensive agriculture, state incorporation, late industrialization</td>
<td>Paraguay</td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Export expansion into labor-intensive agriculture, state incorporation, medium timing of industrialization</td>
<td></td>
<td>Brazil</td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Export expansion into labor-intensive agriculture, incorporation by clientelistic parties, late industrialization</td>
<td></td>
<td>Colombia</td>
<td>Ecuador</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>Export expansion into non-labor-intensive agriculture, incorporation by clientelistic parties, early industrialization</td>
<td></td>
<td>Argentina, Uruguay</td>
<td></td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>Mineral exports, incorporation by radical mass parties, medium timing of industrialization</td>
<td></td>
<td>Chile</td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Mineral exports, incorporation by radical mass parties, late industrialization</td>
<td></td>
<td>Peru, Venezuela</td>
<td>Bolivia</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>Mineral exports, revolution, early industrialization</td>
<td></td>
<td>Mexico</td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>N</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>2</td>
<td>11</td>
</tr>
</tbody>
</table>

Source: Author's interpretation of Rueschemeyer et al.(1992), Table 5.1, p. 164, and the surrounding text.

Note: For the Fisher test, I excluded the row corresponding to Mexico because I could not perform a Fisher test for a 7 X 4 table.
Summary of Arguments and Outcomes about Europe in Rueschemeyer, Stephens and Stephens (1992)

<table>
<thead>
<tr>
<th>Causal Conditions*</th>
<th>Authoritarian Breakdown</th>
<th>Democratic Survival</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>LABDr</td>
<td>Germany, Austria</td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>LAbdr</td>
<td>Italy</td>
<td>Australia</td>
<td>2</td>
</tr>
<tr>
<td>LABdr</td>
<td>Spain</td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>LAbdR</td>
<td></td>
<td>United States</td>
<td>1</td>
</tr>
<tr>
<td>LabdR</td>
<td></td>
<td>Britain</td>
<td>1</td>
</tr>
<tr>
<td>labdR</td>
<td></td>
<td>France</td>
<td>1</td>
</tr>
<tr>
<td>labdr</td>
<td></td>
<td>small European countries, Canada, New Zealand</td>
<td>3</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>4</td>
</tr>
</tbody>
</table>

Source: Rueschemeyer et al. (1992), Table 4.4, p. 144.

*Key:
Capital letters indicate the condition was present; lower-case letters indicate the condition was absent.
L: The landed upper class was politically very significant.
A: The landed upper class was historically engaged in labor-repressive agriculture.
B: The bourgeoisie was strong enough to be politically very significant, but not more powerful than the landed upper class.
D: The bourgeoisie was the dependent partner in the coalition.
R: There was a revolutionary break from the past.

Note: For the Fisher test, I excluded the row corresponding to the United States because I could not perform a Fisher test for a 7 X 2 table. I also treated the small European countries as a single case because the authors do not differentiate them.
### Summary of the Impact of Party System Types on Coups from the Colliers (1991)

<table>
<thead>
<tr>
<th>Type of Party System</th>
<th>Coup</th>
<th>No Coup</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Integrative</td>
<td>Mexico, Venezuela</td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>Electorally Stable but Socially Conflictual</td>
<td>Colombia</td>
<td>Uruguay</td>
<td>2</td>
</tr>
<tr>
<td>Stalemated</td>
<td>Peru, Argentina</td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>Multi-party Polarizing</td>
<td>Brazil, Chile</td>
<td></td>
<td>2</td>
</tr>
</tbody>
</table>

Note: The above is just one argument from the Colliers' (1991) analysis of the Heritage period, but it is the one that is most relevant for democratization. Their work advances many other arguments about the Heritage period and the preceding Reform, Incorporation, and Aftermath periods.

### Summary of Arguments and Outcomes from Downing (1992)

<table>
<thead>
<tr>
<th>Causal Conditions</th>
<th>Military-Bureaucratic Absolutism</th>
<th>Loss of Sovereignty</th>
<th>Preservation of Constitutionalism</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>High warfare, high domestic military mobilization</td>
<td>Brandenburg-Prussia, France</td>
<td></td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>High warfare, low domestic military mobilization</td>
<td>Poland</td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Low warfare, low domestic military mobilization</td>
<td></td>
<td></td>
<td>England to 1648</td>
<td>1</td>
</tr>
<tr>
<td>High warfare, medium domestic military mobilization aided by wealth, alliances, and geography</td>
<td></td>
<td></td>
<td>England 1688-1713, Netherlands</td>
<td>2</td>
</tr>
<tr>
<td>High warfare, low military mobilization but aided by access to foreign resources</td>
<td></td>
<td></td>
<td>Sweden</td>
<td>1</td>
</tr>
</tbody>
</table>

Source: Downing (1992), Table 2, p. 242.
Summary of Arguments and Outcomes in Ertman (1997)

<table>
<thead>
<tr>
<th>Causal Conditions</th>
<th>Patrimonial Absolutism</th>
<th>Bureaucratic Constitutionalism</th>
<th>Bureaucratic Absolutism</th>
<th>Patrimonial Constitutionalism</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Geopolitical competition before 1450, administrative local government</td>
<td>France, Spain, Portugal, Naples, Tuscany, Savoy, and Papal states</td>
<td></td>
<td></td>
<td></td>
<td>7</td>
</tr>
<tr>
<td>Geopolitical competition before 1450, participatory local government, active parliament</td>
<td>Britain</td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Geopolitical competition after 1450, administrative local government</td>
<td></td>
<td>German states</td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Geopolitical competition after 1450, participatory local government</td>
<td>Sweden</td>
<td>Denmark</td>
<td>Hungary and Poland</td>
<td></td>
<td>4</td>
</tr>
<tr>
<td>N</td>
<td>7</td>
<td>2</td>
<td>2</td>
<td>2</td>
<td>13</td>
</tr>
</tbody>
</table>

Source: This is the simple model presented early in the introduction to Ertman's (1997) book. By the end of the chapter, however, Ertman revised the model by distinguishing among parliaments that were weak (Denmark), parliaments that were initially strong but later weakened (Sweden), and parliaments that were strong and remained strong (Hungary and Poland). This change associated each set of causal conditions with a unique outcome. However, expanding the table from 4 X 4 to 7 X 4 cells does not improve the significance of the "test."
## Summary of Arguments and Outcomes in Luebbert (1987)

<table>
<thead>
<tr>
<th>Causal Conditions</th>
<th>Pluralist Democracy</th>
<th>Social Democracy</th>
<th>Traditional Dictatorship</th>
<th>Fascism</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Early industrialization, dominant liberal parties, Liberal-Labor coalitions, and mass support for democracy</td>
<td>Britain, France, Switzerland</td>
<td></td>
<td></td>
<td></td>
<td>3</td>
</tr>
<tr>
<td>Late industrialization, greater Socialist party success, previously mobilized agricultural sector</td>
<td></td>
<td>Netherlands, Belgium, Denmark, Sweden, Norway, Czechoslovakia</td>
<td></td>
<td></td>
<td>6</td>
</tr>
<tr>
<td>Late industrialization, greater Socialist party success, Socialist recruitment of agricultural proletariat, united bourgeois parties</td>
<td></td>
<td></td>
<td>Austria, Finland, Hungary</td>
<td></td>
<td>3</td>
</tr>
<tr>
<td>Late industrialization, greater Socialist party success, Socialist recruitment of agricultural proletariat, divided bourgeois parties</td>
<td></td>
<td></td>
<td>Germany, Italy, Spain</td>
<td></td>
<td>3</td>
</tr>
<tr>
<td>N</td>
<td>3</td>
<td>6</td>
<td>3</td>
<td>3</td>
<td>15</td>
</tr>
</tbody>
</table>

### Summary of Arguments and Outcomes in Moore (1966)

<table>
<thead>
<tr>
<th>Causal Conditions</th>
<th>Democracy</th>
<th>Fascism</th>
<th>Communism</th>
<th>Peaceful Change</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>Commercialized agriculture, strong bourgeois allied with reformist landed upper class, peasantry weakened</td>
<td>Britain</td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Commercialized agriculture, strong bourgeois impulse, peasants sweep aside landed upper class</td>
<td>France</td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Commercialized agriculture, strong bourgeois impulse, no peasantry</td>
<td>U.S.</td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Commercialized agriculture, moderately powerful bourgeois allied with landed upper class</td>
<td>Japan</td>
<td></td>
<td></td>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Commercialized agriculture, weak bourgeois, landed upper class swept away by peasant revolution</td>
<td>Russia, China</td>
<td></td>
<td></td>
<td></td>
<td>2</td>
</tr>
<tr>
<td>Little commercialization of agriculture</td>
<td></td>
<td></td>
<td></td>
<td>India</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td>3</td>
<td>1</td>
<td>2</td>
<td>1</td>
<td>7</td>
</tr>
</tbody>
</table>

Source: Because there have been many conflicting interpretations of Moore’s arguments, I relied on his own simple summary in the preface: Moore (Moore 1966: xiv-xvii). I excluded Germany because Moore devotes no separate chapter to it and included India because there is a chapter on it. Chi-square results for this table would be significant if the first three rows were collapsed into one. I did not do this because Moore takes care to differentiate the British, French, and American paths; he would not be comfortable lumping them all together as similar cases of commercialized agriculture with a strong bourgeois impulse, ignoring the role of the peasantry.


Schedler. 2003. ?????


Endnotes

1. Some consider the definition of “a case” problematic. See Charles C. Ragin and Howard S. Becker (1992). I think that in practice and in context of a particular project, defining a case is not a problem. In the present book, a case is a country observed during a period of time.

2. Author’s personal observation at an APSA meeting in the mid-1990s. As a participant on a roundtable, Linz had been critical of some attempts at theorizing. The quoted remark was his reply to an audience member who challenged him by asking, "Should we just give up on theory and become historians?"

3. Actually, Mom had it backwards as far as eyesight is concerned: squinting is caused by myopia (as I know well from personal experience). In comparative politics, whether a preference for micro-level explanations fosters a preference for case studies or the constraints of case studies encourage micro-level explanations, the approach and the findings are closely associated.

4. I would not allege that the authors were “determined to blame presidentialism” if I did have personal knowledge of it. Both authors were advisees of Juan Linz, as I was, and all of us found his critique of presidentialism persuasive and saw evidence of its pernicious effects even in relatively successful presidential democracies such as Uruguay and Venezuela. This does not mean that we were biased and wrong. Tests of the critique of presidentialism are discussed in detail in Chapter 10.

5. For an excellent summary and critique of this literature, see Michael L. Ross (2001).

6. Hypothetical reasoning is unavoidable in any kind of research, even large-sample statistical comparisons (Fearon 1991). In regression, we must suppose that no omitted variables are correlated with the variables in the model. However, this strikes me as less fanciful than the supposition required in case studies: that there is an entirely new case that is just like the observed case save in one respect, and that we can know the consequences of this small difference.

7. Their definition is useful, although they intend it to be broad enough to include statistical time-series analysis and cultural analysis, both of which I discuss in separate chapters because they raise distinct sets of methodological issues.

8. Two important works in a similar vein are typically omitted from lists like this one: Reinhard Bendix (1964) and Charles Tilly (1975). This is a bit puzzling, but it could be argued that Bendix's work is more descriptive than causal and that Tilly's book is more concerned with state-building than democratization. They tend to be classed with structural-functionalism or modernization theory despite many arguments that are similar to those of Moore and his heirs.

9. Actually, the Colliers also include one case of social revolution–Mexico–but spend most of their time on the post-revolutionary period.
10. The intellectual evolution passed through a stage of holding that states are "relatively autonomous"--sometimes acting against capital in order to guarantee the continued existence of capital. See Nicos Poulantzas (1973:255-321).

11. As James Mahoney observes, "...Moore’s specific hypotheses were stated in a relatively vague manner that has made it difficult to for subsequent scholars to evaluate his claims empirically. ... [S]cholars have had difficulty summarizing Social Origins, leading to a rather diverse set of interpretations regarding the main arguments of the book. ..." (Mahoney 2003:137-8).

12. Actually, as noted earlier in this chapter, multiple paths are required only when the condition on which the effect depends is conceived of in categorical terms. If it is thought of as a continuum, then an interaction between two variables is a more precise model.

13. Here I count the Papal states and the 20 German territorial states as a single entities because Ertman always discusses them as undifferentiated blocs. This gives him a total of 13 cases.