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THE COMPARATIVE METHOD

Moving Beyond Qualitative and Quantitative Strategies

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Heterogeneity and Causal Complexity

"Social phenomena are complex." As social scientists we often make this claim. Sometimes we offer it as justification for the slow rate of social scientific progress. According to our collective folklore there are many, many variables—too many to specify—affecting the phenomena that interest us. Consequently, our explanations are often inadequate. This folklore implies that social phenomena are inordinately complicated and that it is surprising that anyone knows anything about social life.

Yet this depiction of social life does not fit well with experience. We sense that there is a great deal of order to social phenomena—that there is method to the madness. In fact, it is our strong sense that social phenomena are highly ordered that keeps us going. What is frustrating is the gulf that exists between this sense that the complexities of social phenomena can be unraveled and the frequent failures of our attempts to do so. The complaint that social phenomena are complex is not so much an excuse as it is an expression of this frustration.

This sense of-order-in-complexity is very strong in comparative social science because it is not difficult to make sense of an individual case (say, a general strike) or to draw a few rough parallels across a range of cases (a number of general strikes separated in time and space). The challenge comes in trying to make sense of the diversity across cases in a way that unites similarities and differences in a single, coherent framework. In other words, it is often impossible to summarize in a theoretically or substantively meaningful way the order that seems apparent across diverse cases.

The problem of identifying order-in-complexity has two general forms.

One is the identification of types of cases—the problem of constructing useful empirical typologies. Most Third World countries are economically dependent on the developed capitalist countries, for example, but in different ways (see Cardoso 1973, 1977). What are the characteristic forms of dependency? How many different forms are there? Such empirical typologies are important because they set boundaries on comparability. It would be unreasonable, for example, to expect a certain change in the world economy to have identical consequences in different types of dependent countries.

The other characteristic form of the problem of order-in-complexity concerns the difficulty involved in assessing causal complexity, especially multiple conjunctural causation. When an outcome results from several different combinations of conditions, it is not easy to identify the decisive causal combinations across a range of cases, especially when the patterns are confounded. Many different combinations of conditions, for example, may cause the leaders of a government to resign ("regime failure"). These combinations may vary both within and between countries. Yet there is certainly a describable order to these combinations, a patterning that is comprehensible, identifiable, and possibly predictive as well.

Though very different conceptually, these two characteristic forms of the problem of order-in-complexity parallel each other. The first concerns simplifying the complexity among combinations of characteristics of cases and then constructing a model of the types that exist. The second concerns simplifying the complexity among combinations of causes of an outcome (observable across a range of cases) and then constructing a model of these causal combinations. Because the two characteristic forms of the problem are parallel, I focus the discussion in this chapter on only one—the problem of deciphering causal complexity (especially multiple conjunctural causation). This problem has a definite advantage over the first because it is relevant to the general concern in social science for causation, which, in turn, is central to explanation. Parallels between the two problems are examined in later chapters, where I show that the solutions to these two problems provide complementary approaches to the general problem of deciphering order-in-complexity. I begin by discussing the relation between interests and complexity and then address the issue of causal complexity specifically.

INTERESTS, SIMPLICITY, AND COMPLEXITY

Whether any aspect of social life or social organization is simple or complex depends ultimately on the interests of social scientists (and, by implication,

the interests of their audiences). For example, it may be true in a probabilistic sense that children of divorced parents are more likely to drop out of school. This is a perfectly acceptable empirical generalization which presents one aspect of social life in a simple and straightforward manner. It may be entirely unsatisfactory, however, to an investigator (or school principal) interested in understanding how, from the perspective of dropouts, events seem to conspire to force them to quit school. Broken homes may be part of the general context for some of these (apparently) conspiring circumstances, but only a small part. The simple probabilistic relation between broken homes and dropping out is only one of several starting points for a more thorough investigation.

Another simple example comes from the study of face-to-face interaction. Certain patterns of interaction in dyads (asking more questions, for example) are related to the distribution of power. This is a straightforward generalization from empirical data. The fact that this simple, probabilistic relationship exists does not mean, however, that it is pointless to study the variety of situations in which the relation is reversed (with the more powerful person in the dyad displaying an interaction style usually characteristic of less powerful individuals) or to try to generalize about these exceptions. The fact that a general pattern exists does not negate the value of trying to unravel the intricacies of situations in which the relationship is reversed.

The direct relation between interests and the degree of complexity of social phenomena is even more apparent in comparative social science. Several macrosocial theories, for example, argue that international inequality is maintained, in part, by the economic dependence of underdeveloped countries on developed countries. Drawing on these theories, a number of researchers have documented a weak but consistently negative cross-national relationship between economic dependence (such as degree of specialization in the export of primary commodities) and economic growth (rate of increase in GNP per capita). Thus, interest in a global argument about international inequality has inspired general tests of the relationship, and a simple cross-national pattern has been confirmed, though not overwhelmingly. (See Bornschier and others 1978 and Rubinson and Holtzman 1981.)

Other perspectives argue, however, that dependency and GNP per capita growth are not necessarily incompatible and that several countries have experienced "associated-dependent development" (Cardoso 1973). Note that this perspective is more an elaboration of the first (which argues, in effect, that dependency uniformly stunts economic development) than a rejection. The second argues that dependency and growth are compatible in a context

of severe (and possibly increasing) internal inequality and regime repressiveness (see Bradshaw 1985). Several studies have documented cases of associated-dependent development and have shown that it forms a complex of traits consistent with theoretical expectations (see Evans 1979). In this second line of research, detailed study at the case level was mandated because the goal was to document associated-dependent development as a relatively complex totality in the modest number of cases where it has occurred.

The contrast between these two schools of thought and the picture they present of the relation between interests and complexity is clear. The first line of research, which dictates relatively little concern for complexity, views underdeveloped countries as a more or less homogeneous mass and applies a single, variable-oriented causal model to the entire population with some success. The second line of research, by contrast, dictates greater concern for complexity and views the underdeveloped world as heterogeneous—a set containing several distinct populations. Neither view is incorrect. Ultimately, the degree to which a set of observations or cases is one population or many depends on the interests of the investigator and those of the intended audience.

The close connection between interests and complexity in comparative research is also evident in many comparativists' predilection for studying cases that register "extreme values" on important dimensions of cross-national variation. Comparativists often argue that cases with extreme values are qualitatively different from other cases and that this quality justifies close attention to their complexity, despite their relative infrequency. The example of countries experiencing social revolutions versus countries experiencing milder forms of social turmoil is useful here. (The argument applies equally well to other infrequent but important large-scale social phenomena.) The fact that some elements of a revolution are present—albeit in muted form—in nonrevolutionary cases does not change the fact that a social revolution is an unusual combination of circumstances. In disaggregated form, the different components of a revolution (which might be present in different countries at different times—for example, executive instability in the United States during the Watergate period) are not revolutionary because it is the *whole* these components form when combined that gives them their revolutionary character. The fact that a few superficial commonalities exist across revolutionary and nonrevolutionary cases does not detract from the importance of social revolution as a theoretical category with considerable cultural and political significance—a phenomenon demanding the special attention of social scientists.

Some comparativists argue further that cases registering extreme values deserve detailed attention because they provide especially pure examples of certain social phenomena. (See, for instance, Durkheim in *Elementary Forms of the Religious Life*.) Dumont (1970), for example, argues that the Indian caste system provides a unique opportunity to study human social stratification in its purest known form. Anthropologists (such as Harris 1978 and 1985) frequently justify their selection of cases on these grounds, usually with the goal of showing that emergent cultural patterns that may seem bizarre or extreme in some way have important practical value and should therefore be understood in a larger context.

In general, attention to complexity is justified whenever it is argued that a certain historical outcome (say, the Sandinista Revolution in Nicaragua) or set of similar outcomes (say, anti-neocolonial revolutions) is historically or culturally significant in its own right and therefore demanding of social scientific interpretation. The interpretation of important historical events and outcomes (which includes a wide array of macrosocial phenomena ranging from brief episodes of collective action to the rise of the West) is one of the defining features of comparative social science—one of its special missions. Furthermore, this type of interpretation is a primary avenue for the dissemination of social scientific knowledge. While general statements about major dimensions of macrosocial variation and their interrelation (that is, the stuff of variable-oriented comparative social science) are important, the reach of these general statements beyond a purely academic audience is limited by their abstract character (see Ragin 1985).

Interest in complexity is most apparent whenever comparative social scientists address specific historical outcomes, especially when they examine the causes of similar outcomes in different contexts. It is difficult to specify historical causation across a range of cases, however, because such causation is often conjunctural. I turn now to a general discussion of the issue of multiple conjunctural causation as it relates to comparative research.

CAUSAL COMPLEXITY

Virtually all everyday events show causal complexity. A funny joke told in the wrong setting can fall flat. Some compliments come off like insults; some insults come off like jokes. Certain behavioral patterns in some individuals are seen as virtues; in others they are seen as vices. In all these examples context plays an important part. This is because human understanding of causation and of events in general is fundamentally holistic. (See

Nisbett and Ross 1980.) Parts are not viewed in isolation but in context of the whole they form. To change one or more elements often changes how the whole is perceived or understood, which, in turn, has an impact on the meaning of each individual part. (For a more sophisticated treatment of these and related issues, see Goffman 1974.)

Examples of causal complexity at the macrolevel abound. A president's popularity may increase as the result of military intervention in other areas of the world; it can also plummet. News about higher interest rates can cause the stock market to go up or down, depending on other economic news. Appeals to patriotic sentiment by political leaders are sometimes quite effective, depending on the timing and character of the appeal and the specific mix of national symbols used in the appeal. But they often fall flat. It is hazardous to predict when an appeal to patriotism will work. In short, the prediction of collective sentiments, mass behavior, and aggregate trends in general is a risky business. We are awash with political and economic advisors and consultants precisely because of the causal complexity of national-level events and processes.

Most national-level events of interest to comparativists show a great deal of causal complexity. Some polities in the Third World, for example, are thought to be stable because they are democratic (Costa Rica, for example), but others are thought to have failed because of the instability that was magnified by the adoption of democratic procedures (certain countries of sub-Saharan Africa, for instance). The specific contribution of democracy to political stability depends on context. Another example: sometimes a prolonged deterioration in socioeconomic conditions demoralizes people and makes them apathetic (see Gurr 1970). In other circumstances it may make them revolutionary (see Walton 1984). Still, there are instances of mass mobilization occurring in the absence of important socioeconomic change. Another example: in some countries ethnic inequality fuels ethnic political mobilization (as in Wales), while in others there is ethnic political mobilization without dramatic ethnic inequality (as in Belgium). Ethnic political mobilization can result from a variety of seemingly unrelated causes. There is no universal explanation of this or most other large-scale events common to contemporary nation-states.

Whenever social scientists examine large-scale change (such as the collapse of a polity, the emergence of an ethnic political party, or the rapid decline in support for a regime), they find that it is usually *combinations* of conditions that produce change. This is not the same as arguing that change

results from many variables, as in the statement "both X_1 and X_2 affect Y," because this latter type of argument asserts that change in either causal variable produces a change in Y, the dependent variable.

When a causal argument cites a combination of conditions, it is concerned with their intersection. It is the intersection of a set of conditions in time and in space that produces many of the large-scale qualitative changes, as well as many of the small-scale events, that interest social scientists, not the separate or independent effects of these conditions. Such processes exhibit what John Stuart Mill (1843) called "chemical causation." The basic idea is that a phenomenon or a change emerges from the intersection of appropriate preconditions—the right ingredients for change. In the absence of any one of the essential ingredients, the phenomenon—or the change—does not emerge. This conjunctural or combinatorial nature is a key feature of causal complexity.

The conjunctural nature of social causation is not the only property of social phenomena that makes them complex. Typically, there are several combinations of conditions that may produce the same emergent phenomenon or the same change. The comparison of many large-scale changes, for example, often leads to the conclusion that for a given type of outcome (say, the formation of regionally based ethnic political parties) there are many causally relevant intersections of conditions. In one set of cases, for example, a coincidence of ethnic inequality, a high degree of government centralization, and increased domination of regional economies by multinational firms may explain the emergence of ethnic parties. In another set, a coincidence of ethnic equality, decentralized government, and an increased migration of members of the numerically dominant ethnic group into regions containing minority ethnic groups might explain the formation of ethnic parties. In the first set, it is the lure of separatism that spurs ethnic mobilization. In the second, it is the infringement by the majority group on formerly ethnic turf that stimulates ethnic mobilization. These two combinations of conditions certainly would not exhaust all instances of regionally based ethnic political mobilization. Other combinations might be identified, and the specification of other causal combinations might further the identification of different types of ethnic mobilization. The point is not the number of causal combinations or types but the fact that the same general outcome—ethnic political mobilization—may result from various combinations of causes.

That social causation is often both multiple and conjunctural is consistent with commonsense notions about how the world works. The key considera-

tion is the whole—how different conditions or parts fit together. The problem that social scientists face is to unravel the empirically relevant causal combinations. In other words, once the possibility of multiple conjunctural causation is admitted, it is necessary to determine how different conditions fit together—and in how many different combinations—to produce a given outcome. The identification and interpretation of these causal configurations (or causal complexes) allows the investigator to delineate the different empirical processes and causal mechanisms relevant to a specific outcome.

Thus, social phenomena are complex and difficult to unravel not because there are too many variables affecting them, although the number of causal variables is certainly important, but because different causally relevant conditions can combine in a variety of ways to produce a given outcome. In short, it is the combinatorial, and often complexly combinatorial, nature of social causation that makes the problem of identifying order-in-complexity demanding.

THE ANALYSIS OF CAUSAL COMPLEXITY

Causal complexity is not easily unraveled, paradoxically, because of the relatively limited diversity of empirical social phenomena. The similarities and differences among nonexperimental cases confound attempts to specify social causation unambiguously. If social scientists could create social phenomena displaying all the different combinations of causal conditions and then observe outcomes (that is, if they could conduct experiments), it would be a simple matter to explicate the decisive causal combinations. Obviously, this is not possible, so they have developed research strategies appropriate for nonexperimental data. Before addressing nonexperimental strategies, I examine the experimental design standard that inspires nonexperimental approaches.

The ideal social scientific comparison is identical in structure to the simple experiment. In a simple experiment an investigator compares an experimental group, which has been subjected to an experimental treatment, with a control group, which differs from the experimental group in only one respect—it does not receive the treatment. Only one factor, the treatment, is allowed to vary; all other conditions are held constant or randomized. If significant posttreatment differences between the experimental and control group emerge, these differences are credited to the experimental or treatment variable, and a tentative cause–effect sequence is established.

Experimental design has an unrivaled directness and simplicity, and it is immune to some of the inferential errors that affect other methods. Of course, experimental research is confronted with a host of threats to its validity (see Campbell and Stanley 1966; Cook and Campbell 1979), but it is more capable of deciphering causal complexity than other techniques. This is because it allows the investigator to manipulate causes directly—to manufacture a basis for making comparisons.

Many features of social life confound attempts to unravel causal complexity when experimental methods cannot be used. Three are especially relevant to this discussion because they concern issues of multiple and conjunctural causation. First, rarely does an outcome of interest to social scientists have a single cause. The conditions conducive to strikes, for example, are many; there is no single condition that is universally capable of causing a strike. Second, causes rarely operate in isolation. Usually, it is the combined effect of various conditions, their intersection in time and space, that produces a certain outcome. Thus, social causation is often both multiple and conjunctural, involving different combinations of causal conditions. Third, a specific cause may have opposite effects depending on context. For example, changes in living conditions may increase or decrease the probability of strikes, depending on other social and political conditions (Snyder 1975). The fact that some conditions have contradictory effects depending on context further complicates the identification of empirical regularities because it may appear that a condition is irrelevant when in fact it is an essential part of several causal combinations in both its presence and absence state.

Natural scientists attempt to establish causes that are either necessary or sufficient or both necessary and sufficient. In situations where causation is multiple and conjunctural, there may be no necessary or sufficient conditions for an outcome of interest. For example, if outcome Y occurs following the combination of X_1 and X_2 or the combination of X_3 and X_4, then none of the single conditions, X_1 through X_4, is either necessary or sufficient to produce Y. This possibility complicates the observation of causal relations in nonexperimental settings because investigators typically are not able to observe all logical combinations of the relevant causal conditions. Yet comparative social scientists are often confronted with phenomena that display this type of causation.

Multiple conjunctural causation can be assessed directly only in experimental designs. Suppose a researcher believes that three factors (X_1, X_2, and X_3) are causally relevant to Y and has strong reason to suspect that different

TABLE 1: Experimental Design Exhausting Logically Possible Combinations of Three Treatments

	Treatments		
	x_1	x_2	x_3
Group 1:	absent	absent	absent
Group 2:	present	absent	absent
Group 3:	absent	present	absent
*Group 4:	present	present	absent
Group 5:	absent	absent	present
Group 6:	present	absent	present
*Group 7:	absent	present	present
Group 8:	present	present	present

*Groups showing change in outcome variable (y).

combinations of these factors cause Y. The presence of any one factor alone is not thought to be sufficient; only certain (as yet unspecified) combinations of factors are capable of causing Y. Determining the relevant combinations of conditions is a simple matter if an experimental design is feasible. In this example the experimenter would set up eight different experimental groups and apply different combinations of the treatment variables, as shown in Table 1. The investigator would examine Y under each of the eight conditions to see which combinations of X's cause Y. If Y were to occur only in groups 4 and 7, for example, the investigator would conclude that if X_2 is accompanied by X_1 or X_3, but not by both, then Y will result. (In this example, X_2 is a necessary but not a sufficient condition for Y.)

The beauty of experimental design is that it is a simple matter to examine combinations of conditions and determine the specific combinations that are causally relevant. Thus, causal complexity, which is a key characteristic of social life, is unraveled. Decisive comparisons can be made because all the relevant combinations of conditions are manufactured by the investigator. In the hypothetical study described here, Y is caused by X_2 when it is combined with either X_1 or X_3, but not when it is combined with both. There are three decisive comparisons that establish this finding: the comparison of group 4 with groups 2 and 3, which establishes that X_2 and X_1 must be combined to produce Y; the comparison of group 7 with groups 3 and 5, which establishes that X_2 and X_3 must be combined to produce Y; and the comparison of

groups 4 and 7 with group 8, which establishes that when X_2 is combined with both X_1 and X_3, then Y does not result. (Other comparisons are also important, but these are the most decisive.) In each of the key comparisons an experimental group is compared with other groups differing in only one causally relevant condition.

Of course, social scientists rarely ask questions that can be addressed with experimental methods. Their questions are usually shaped by the events around them, and social scientists often are called upon to interpret events (or simply desire to do so), including the social and historical forces that have shaped contemporary social arrangements. For example, some social scientists are interested in the conditions that lead to different types of collective action. What conditions cause peasants to rebel? What conditions cause workers to go on strike? What conditions cause citizens to feel nationalistic or cause members of an ethnic minority to organize ethnic political parties? Obviously, experimental methods are not applicable to these questions. It is impossible to manipulate conditions affecting large masses of people, and social scientists must be content to study naturally occurring (that is, "non-experimental") data. Yet there is good reason to believe that the causes of these phenomena are both multiple and conjunctural and therefore require experiment-like analyses. Only when naturally occurring data approximate experimental designs is it possible to decipher the order-in-complexity that seems apparent in these phenomena.

Consider, for example, the following hypothetical examination of the causes of peasant revolts in different areas within a single country. Assume there are four causes to consider across six different regions, with different combinations of causes appearing as in Table 2.

There are no experiment-like contrasts among the six regions because all pairs of regions differ on at least two of the four causes. When this pattern exists, it is difficult to draw any strong conclusion. For example, data from regions 3 and 6 indicate that land hunger combined with an absence of commercialization of agriculture may be important to peasant revolts. But region 4 had the opposite pattern on these two variables and also experienced a revolt. Regions 3 and 4 both combine peasant communalism and few middle peasants, suggesting that peasant revolts are more likely in traditional peasant communities lacking an upwardly mobile class of middle peasants. But region 6 has the opposite values on these two variables and experienced a revolt. Examination of the four regions with revolts suggests that if any two of four conditions are present, then a peasant revolt is likely. But region 2

TABLE 2: Hypothetical Regional Data Showing Distribution of Causes of Peasant Revolts

Region	Revolt	*L*	*C*	*P*	*M*
1	no	no	no	no	no
2	no	yes	yes	yes	yes
3	yes	yes	no	yes	no
4	yes	no	yes	yes	no
5	yes	yes	yes	no	no
6	yes	yes	no	no	yes

L = Land hunger
C = Commercialization of agriculture
P = Peasant communalism
M = Middle peasants

had all four conditions present, and a revolt failed to occur. In short, it would be unwise to draw any strong conclusions from these data. The diversity of causal patterns among these cases is too limited to permit sound conclusions based on the data.

CURRENT ALTERNATIVES TO EXPERIMENTAL DESIGN

The observations offered above concerning the limited applicability of experimental designs to most social science data are certainly not new. The discussion serves primarily to establish what most American-trained social scientists, both comparativists and noncomparativists, consider to be the ultimate standard in social science methodology: the precision and causal certainty of experimental design. (See also Lieberson 1985.) Social scientific statements about empirical phenomena are thought to be sound to the extent that the demands of experimental design (which could be considered a methodological ideal type) have been met. The closer the approximation to the type of comparison fundamental to experimental design, the more sound the statement of empirical regularity.

Obviously, social scientists rarely come close, and some argue that social scientists should simply acknowledge the limitations of their efforts and give up the experimental design standard. While it might be possible to abandon the standard, comparison still provides the primary basis for empirical generalization. As Swanson (1971 : 145) notes, "thinking without comparison is

unthinkable"—and comparison, at its social scientific best, involves experiment-like contrasts. Is it possible to ask the questions that social scientists ask and still retain experiment-like comparison as an ideal? There have been two basic responses to this question. Each response constitutes a research strategy; both research strategies have long histories.

The first strategy has been for comparatively oriented social scientists to use case-oriented methods, also known as *the* comparative method (see Chapters 1 and 3; Smelser 1973; Ragin 1983), qualitative historical methods (Ragin and Zaret 1983), the method of systematic comparative illustration (Smelser 1976), and logical methods (Gee 1950; see also Skocpol and Somers 1980), to name only a few of the many labels that have been applied. Investigators who use this strategy usually work only with small, theoretically defined sets of cases, and they compare cases with each other as wholes to arrive at modest generalizations, usually about historical origins and outcomes, concerning relatively narrow classes of phenomena.

Some have argued that this tradition follows in the footsteps of Weber, and German historiography more generally, and that it is primarily an interpretive tradition. While there is a good deal of truth to the claim that the tradition is Weberian (Ragin and Zaret 1983), this strategy is usually not merely interpretive but also causal-analytic. To characterize this tradition as predominantly interpretive implies that the experimental design standard is irrelevant—that a concern for historical essences and particularities removes any need for experiment-like comparisons.

Considering only extreme examples of case-oriented investigation, it is true that this type of inquiry often involves a different way of seeing social phenomena. The best work in this tradition, however, the work that is most relevant to the concerns of social scientists, does not stop with historical interpretation. Two tasks are usually apparent: interpreting historically significant or decisive social phenomena and determining the causes of important categories of social phenomena (such as the origins of different types of modernizing revolutions, as in Moore 1966).

The case-oriented strategy attempts to approximate experimental rigor by identifying comparable instances of a phenomenon of interest and then analyzing the theoretically important similarities and differences among them. This approach provides a basis for establishing modest empirical generalizations concerning historically defined categories of social phenomena. Of course, there is rarely a sufficient variety of cases to prove or disprove causal arguments. Typically, several possible explanations can be supported

in a given set of cases. The limited variety of cases imposes a necessary indeterminancy. Thus, the investigator must support his or her chosen explanation by citing surrounding circumstances and, more generally, by interpreting cases. This attention to the details of individual cases engenders a rich research dialogue between the investigator and the evidence.

The second strategy also has deep intellectual roots, which can be traced back to Comte and Durkheim (see Ragin and Zaret 1983), but it has recently received a strong boost from mainstream social science methodology, especially quantitative methods. The second strategy typically is not concerned with accounting for historically defined phenomena, such as modernizing revolutions or peasant rebellions. It is concerned with formulating broad generalizations about societies and other large-scale social organizations. Unlike the first strategy, which is oriented toward explaining specific cases or historically defined categories of social phenomena, the second strategy is more concerned with variables and their relationships. Its primary goal is to test abstract hypotheses derived from general theories concerning relationships between features of social units such as societies conceived as variables.

A preference for generality over specificity enhances the compatibility of the second strategy with the goals of mainstream social science which, in turn, has allowed the use of mainstream methods, especially techniques of statistical control. This strategy attempts to approximate the rigor of experimental methods through statistical manipulation. The effects of competing and confounding variables are "removed" or "partialed" in estimating the effect of each variable. In this way conditions are "controlled," and a basis for generalizing about confounded causes is manufactured mathematically. (These procedures and the logic of statistical control in nonexperimental research in general are critically evaluated in Lieberson 1985.)

Note that in this strategy it is possible to manufacture a basis for generalizing about causes only by making simplifying assumptions about their operation. These assumptions sometimes are not necessary, but they greatly simplify the task of examining empirical data and the problem of summarizing and presenting the general patterns of covariation that exist among diverse cases. Statistical techniques are biased toward simplifying complexity through assumptions because the assumptions are often built into the procedures themselves. Thus, these techniques do not decipher causal complexity but eliminate perplexing elements of it.

A common (and sometimes testable) assumption, for example, is that causes are additive. One problem with this assumption is that it asserts that

the effect of a cause is the same in all contexts—regardless of the values or levels of other causal variables. This assertion directly contradicts the idea, held dear by many case-oriented investigators, that causation, especially historical causation, is often multiple and conjunctural. (This issue is addressed in greater detail in Chapters 3 and 4.) Assumptions that are built into statistical models have a profound effect on the nature of the research dialogue—the interaction between the investigator and the evidence—that develops in the variable-oriented approach. The dialogue centers on the issue of specifying the "correct" model. The identity, diversity, and particularity of cases tend to be obscured.

In the next two chapters, I examine these broad strategies in detail. I pay special attention to the way scholars in both traditions have attempted to approximate features of experimental design. It is important to point out that in many respects I present exaggerated versions of these strategies and that many variants and combinations exist. In fact, the best comparative work usually combines these two strategies in some way (see Chapter 5). After all, for most comparativists the problem is not choosing strategies per se, but doing good comparative work. Following my presentation of the basics of these broad strategies, I discuss several ways these strategies have been combined. Finally, I present a middle road between the two which integrates important features of both. This integration is the foundation for my elaboration of Boolean techniques of qualitative comparison.