ABSTRACT
Various scholars have evaluated case-oriented comparative research from the perspective of large-N, variable-oriented research and found it lacking. In this chapter I turn the tables and evaluate large-N, quantitative research relative to the standards of case-oriented work. I focus on practical concerns addressed in case-oriented research which pose serious challenges to large-N, variable-oriented inquiry. The five practical concerns I address are: (1) the problem of constituting cases (defining and delineating the class of cases relevant to a particular investigation), (2) the problem of studying the causes of outcomes which are uniform across selected cases (“positive cases”), (3) the problem of delineating and defining negative cases which can be compared with positive cases, (4) the problem of studying multiple paths to the same outcome (multiple conjunctural causation), and (5) the problem of accounting for nonconforming cases. Case-oriented scholars use flexible analytic frames than can be modified in light of the knowledge of cases that researchers gain in the Course of their research. This aspect of the case-oriented approach makes it especially well-suited for concept formation and theory development.

INTRODUCTION
In this chapter I offer an extended response to John Goldthorpe's discussion of the merits of case-oriented investigation. Goldthorpe evaluates qualitative, case-oriented social science using the standards of quantitative, variable-oriented social science. He examines three problems: (1) the problem of small Ns, (2) the problem of the non-independence of observations, and (3) the problem of "black box" explanations. Goldthorpe does not offer solutions to these problems. He is content simply to argue that they plague macro-quantitative research and are not solved in case-oriented research. In essence, he appears to be more concerned with maintaining the preeminence of variable-oriented social science in the face of recent challenges by researchers advocating case-oriented methods than with solving problems. The implicit and sometimes explicit message in these discussions is that there are fundamental "problems" with the practice of case-oriented research and that this approach somehow must be made more rigorous.

It may come as a surprise to some that variable-oriented techniques need defending. After all, those who use conventional quantitative techniques align themselves with some of the most powerful social scientists in the world today. For example, economists, who are often aped by sociologists and political scientists, and practitioners of survey research hailing from many different disciplines, professions, and settings (including the world's most powerful governments and corporations).

Goldthorpe's defense of variable-oriented methods touches on many important methodological issues that certainly warrant careful consideration and debate. While I find much to disagree with his essay, I readily accept one of its basic premises - that there is a lot to be gained from a healthy dialogue between case-oriented and variable-oriented approaches. I also agree with Goldthorpe that there is much that can be improved in the conduct of case-oriented research. Rather than engage in tedious debates on issues that cannot be resolved to anyone's satisfaction in journal articles, I offer instead a new contribution to the dialogue between case-oriented and variable-oriented approaches. Rather than evaluate case-oriented work relative to
the standards of variable-oriented work, I translate some of the key concerns of case-oriented research to variable-oriented research and show the difficult methodological problems these concerns pose for the variable-oriented approach. In short, I seek to enrich the dialogue between case-oriented and variable-oriented approaches by turning the tables.

My goal in this chapter is not to repeat familiar appeals about uniqueness, holism, experience, meaning, narrative integrity, or cultural significance - the concerns most often voiced by qualitative, case-oriented researchers in debates about methods. Nor do I waste much time repeating the claim that the goals of qualitative research differ diametrically from those of quantitative research. After all, there is no necessary wedge separating the goal of "inference" - the key concern of quantitative approaches - from the goal of "representation" a common concern of qualitative approaches (Ragin 1994, pp. 47-52). Instead, I elucidate practical concerns that are at the core of case-oriented strategies and pose several important challenges to variable-oriented approaches. I do not claim that these difficulties throw insurmountable obstacles in the process of variable-oriented researcher neutralized through assumptions. My ultimate goal in raising these concerns is to make those who practice the variable-oriented approach at least more self-conscious and perhaps more rigorous as well. The concerns I address are centered in five overlapping domains: (1) the constitution of cases, (2) the study of uniform outcomes, (3) the definition of negative cases, (4) the analysis of multiple and conjunctural causes, and (5) the treatment of nonconforming cases.

THE CONSTITUTION OF CASES (What is a case?; What is this a case of?)

Case-oriented researchers see cases as meaningful but complex configurations of events and structures. They treat cases as singular, whole entities purposefully selected, not as homogeneous observations drawn at random from a pool of equally plausible selections. Most case-oriented studies start with the seemingly simple idea that social phenomena in like settings (such as organizations, neighborhoods, cities, countries, regions, cultures, and so on) may parallel each other sufficiently to permit comparing and contrasting them. The clause, "may parallel each other sufficiently," is a very important part of this formulation. The qualitative researcher's specification of relevant cases at the start of an investigation is really nothing more than a working hypothesis that the cases initially selected are in fact alike enough to permit comparisons. In the course of the research, the investigator may decide otherwise and drop some cases, or even whole categories of cases, because they do not appear to belong with what seem to be the core cases. Sometimes, this process of sifting through the cases leads to an enlargement of the set of relevant cases and a commensurate broadening of the scope of the guiding concepts. For example, a researcher might surmise in the course of studying "military coups" that the relevant category could be enlarged to include all "irregular transfers of executive power."

Usually, this sifting of the cases is carried on in conjunction with concept formation and elaboration. Concepts are revised and refined as the boundary of the set of relevant cases is shifted and clarified. Important theoretical distinctions often emerge from this dialogue of ideas and evidence. Imagine, for example, that Theda Skocpol (1979) had originally included Mexico along with France, Russia, and China at the
outset other study of social revolutions. The search for commonalities across these four cases might prove too daunting. By eliminating Mexico as a case of social revolution in the course of the research, however, it might prove possible to increase the homogeneity within the empirical category and, at the same time, to sharpen the definition of the concept of social revolution.

This interplay of categorization and conceptualization is a key feature of qualitative research. However, King and associates (1994) strongly discourage this practice, arguing that it is not appropriate to "add a restrictive condition and then proceed as if our theory, with that qualification, has been shown to be correct." They offer the following example of their concern (1994, p. 21, original italics):

If our original theory was that modern democracies do not fight wars with one another due to their constitutional systems, it would be less permissible, having found exceptions to our "rule", to restrict the proposition to democracies with advanced social welfare systems once it has been ascertained by inspection of the data that such a qualification would appear to make our proposition correct.

They state subsequently (1994, p. 22, original italics) that "we should not make it [i.e., our theory] more restrictive without collecting new data to test the new version of the theory." Unfortunately, this well-reasoned advice puts an end to most case-oriented research as it is practiced today. When the number of relevant cases is limited by the historical record to a mere handful, or even to several handfuls, it is simply not possible to collect a "new sample" to "test" each new theoretical clarification.

Both Goldthorpe and King and associates (1994) recommend switching to a different unit of analysis, say sub-national units or time periods, to enlarge the number of "cases" relevant to an argument formulated for larger units' However, most case-oriented comparative social scientists do not find this practice satisfactory. They study the cases they do because these cases are historically, politically, or culturally significant in some way. Typically, the shift to smaller units (i.e., to sub-national units or to time periods) entails an unavoidable reformulation of the research question which, in turn, severely undermines the substantive value of the study. Researchers end up asking questions dictated by methods or by data availability, not by their theoretical, substantive, or historical interests.

In fairness to both Goldthorpe and King and associates (1994), it is important to note that their primary concern is theory testing, not concept formation, elaboration, and refinement. Neither King and associates nor Goldthorpe would object to the common practice of using knowledge of the empirical world - however it may have been gained - to build better concepts and thus, ultimately, stronger theories. Still, it is worth pointing out that in their perspective theory testing is the primary, perhaps sole, objective of social science and researchers should organize their research efforts around this important task. It is as though Goldthorpe and King and associates and others start with the assumption that social scientists already possess well-developed, well-articulated, testable theories. Nothing could be further from the truth. In case-oriented research, the bulk of the research effort is often
directed toward constituting "the cases" in the investigation and sharpening the concepts appropriate for the cases selected.

The first practical concern can now be summarized in succinct terms: In much case-oriented research cases usually are not predetermined, nor are they "given" at the outset of an investigation. Instead, they often coalesce in the course of the research through a systematic dialogue of ideas and evidence. In many qualitatively oriented studies, the conclusion of this process of "casing" may be the primary and most important finding of the investigation. Consider the serious practical problem this poses for conventional quantitative analysis: the boundary around the "sample of observations" must be relatively malleable throughout the investigation, and this boundary may not be completely fixed until the research is finished. Thus, any statistical result (say, the correlation between two variables across cases) is open to fundamental revision up until the very conclusion of the research because the cases that comprise the sample may be revised continually before that point. Quantitative analysis of the relationships among variables presupposes a fixed set of relevant observations.

THE STUDY OF UNIFORM OUTCOMES (selection-bias?)
Because the constitution and selection of cases is central to qualitative inquiry, case-oriented researchers may intentionally select cases that differ relatively little from each other with respect to the outcome under investigation. For example, a researcher might attempt to constitute the category "anti-neocolonial revolutions," both empirically and conceptually, through the reciprocal process just described. At the end of this process his or her set of cases might exclude both lesser uprisings (e.g., mere anti-neocolonial "rebellions") and mass insurrections of varying severity that were successfully repressed. In the eyes of the variable-oriented researcher, however, this investigator has committed a great folly - selecting cases that vary only slightly, if at all, on the outcome, or dependent variable.

The first and most obvious problem with this common practice - in the eyes of the variable-oriented scholar - is the simple fact that the dependent variable in this example, anti-neocolonial revolution, does not vary substantially across the cases selected for study. All cases selected display, more or less, the same outcome - anti-neocolonial revolutions. Variable-oriented researchers tend to equate "explanation" with "explaining variation". If there is no variation in the outcome, they reason, then there is nothing to explain. From the perspective of statistical analysis, therefore, the case-oriented investigation of anti-neocolonial revolutions just described may seem to lack even the possibility of analysis or research design. It appears to be an analytic dead end.

The second problem with this common case-oriented practice is known, to statisticians, as "selecting on the dependent-variable". Assume (1) that the category "anti-neocolonial revolutions" encompasses cases with the highest scores (say, 90 through 100 on a 100 point scale) of the more general variable "level of mass insurrection". And, (2) that this dependent variable has a strong positive correlation with measures of foreign capital penetration, say, the proportion of fixed capital that is owned by transnational corporations. No doubt, the cases of "anti-neocolonial revolt" identified by the qualitative researcher would all have high levels of foreign
capital penetration. However, within the relatively narrow range of mass insurrection that encompasses "anti-neocolonial revolution" (i.e., countries with scores over 90), there may be no apparent relationship between level of foreign capital penetration and the level of mass insurrection. Instead, the relationship between these two variables might be visible only across the entire range of variation in the dependent variable, level of mass insurrection, with scores ranging from near zero to 100. For this reason all researchers are advised, based on sound statistical arguments, to examine the entire range of variation in broadly defined dependent variables and thereby avoid this analytic sin.

These statistically based criticisms are well reasoned. However, they are based on a very serious misunderstanding of case-oriented research. The first response to these criticisms concerns the theoretical status of the categories elaborated through case-oriented research. The fact that anti-neocolonial revolutions all have very high scores on the variable "level of mass insurrection" does not alter the possibility that anti-neocolonial revolutions are fundamentally (i.e., qualitatively) different from other forms of insurrection and therefore warrant separate analytic attention. Social scientists study the phenomena they study because these phenomena are often culturally or historically significant. The fact that some phenomena (e.g., anti-neocolonial revolutions) can be re-construed as scores on more general variables (e.g., mass insurrection) does not negate their distinctive features or their substantive importance.

The second response to these criticisms is the simple observation that most case-oriented investigators would not be blind to the fact (in the hypothetical example) that countries with anti-neocolonial revolutions have unusually high levels of foreign capital penetration. Indeed, the very first step in the qualitative analysis of anti-neocolonial revolutions, after constituting the category and specifying the relevant cases, would be to identify the possible causal conditions they share - their commonalities. Their high levels of foreign capital penetration no doubt would be one of the very first commonalities identified. It is not a causal fact or that would be overlooked because of its lack of apparent correlation with the intensity of anti-neocolonial revolutions within the relatively narrow range of outcomes selected for study.

The issue of "uniform outcomes", therefore, concerns the function and importance of what statisticians call constants in case-oriented analysis. Often the outcome (i.e., the "dependent variable") and many of the explanatory factors in a case-oriented analysis are constants - all cases have more or less the same values. In the example just presented, anti-neocolonial revolutions (the uniform outcome) occur in countries with uniformly high scores on one causal variables (foreign capital penetration) and probably with relatively uniform values on other causal variables as well. While using constants to account for constants (i.e., the search for commonalities shared by similar instances) is common in case-oriented work (and in everyday life), it is foreign to statistical techniques that focus exclusively on relationships among variables - that is, on causal conditions and outcomes that must vary across cases.
THE DEFINITION OF NEGATIVE CASES

The discussion so far has brought us to a debate recognizable to many as the controversy surrounding the method of analytic induction, a technique that follows Mill's method of agreement. The method of agreement looks only at positive cases (that is, cases displaying an effect) and assesses whether or not these positive cases all agree in displaying one or more causes. The usual statistically based objection to this practice is that only two cells of a two-by-two cross-tabulation (presence/absence of a cause by presence/absence of an effect) are studied and that, therefore, causal inference is impossible. After all, what if many of the negative cases (that is, cases not displaying the effect) display the same causal factors (e.g., in the example just elaborated, a high level for foreign capital penetration)?

This criticism appears sound. However, it is very important to recognize that this criticism assumes a pre-existing population of relevant observations, embracing both positive and negative cases, and thus ignores a central practical concern of qualitative analysis - the constitution of cases, as described above. From the perspective of case-oriented qualitative analysis, the cross-tabulation of causes and effects is entirely reasonable as long as these analyses are conducted within an appropriately constituted set of cases. For example, it would be entirely reasonable to assess whether or not the emergence of "multiple sovereignty" in anti-neocolonial revolutions is linked to the prior existence of democratic institutions. However, this analysis would be conducted only within the duly constituted category of anti-neocolonial revolutions. The statistically-based critique of analytic induction and the method of agreement ignores this essential precondition for conventional variable-oriented analysis, namely, that relevant cases must be properly constituted through a careful dialogue of ideas and evidence involving the reciprocal clarification of empirical categories and theoretical concepts.

From the perspective of case-oriented analysis, to cross-tabulate the presence/absence of causes of anti-neocolonial revolution with the presence/absence of anti-neocolonial revolution, it first would be necessary to constitute the category of relevant negative cases (for example, the category "countries with a strong possibility of anti-neocolonial revolutions"). Before doing this, of course, it would be necessary to examine actual anti-neocolonial revolutions closely and identify their common causes, using theory, substantive knowledge, and interests as guides. In other words, the investigator would have to constitute the empirical category "anti-neocolonial revolutions" and identify common causal factors (using the method of agreement or some other method appropriate for the study of uniform outcomes) before attempting to constitute the category "countries with a strong possibility of anti-neocolonial revolutions" and then proceed with conventional variable-oriented analysis of the differences between positive and negative cases (see also Griffin et al. 1996).

Thus, conventional variable-oriented analysis assumes the very thing that case-oriented analysis typically considers most problematic - the relevant population of cases, including both positive and negative instances. The many simple-minded critiques of the method of agreement (and analytic induction) fall apart as soon as it is recognized that the constitution of populations is a theory-laden, concept-intensive process. Further, as I have argued, the constitution of relevant "negative cases" typically rests on the careful prior constitution of "positive cases." Even once both
positive and negative cases have been identified, the boundaries of the relevant population of cases may still be adjusted further in the course of case-oriented research.

THE EXAMINATION OF MULTIPLE AND CONJUNCTURAL CAUSES

After constituting and selecting relevant instances of a phenomenon like "anti-neocolonial revolutions" and, if possible, defining relevant negative cases as well, the case-oriented investigator's task is to address the causal forces behind the phenomenon, with special attention to similarities and differences across cases. Each case is examined in detail- using theoretical concepts, substantive knowledge, and interests as guides - in order to answer the question of "how" the phenomenon of interest came about in each positive case and why it did not in the negative cases - assuming they can be confidently identified. While it is standard practice for case-oriented researchers to search for constants (e.g., high levels of foreign capital penetration) across positive cases in their attempts to identify the causal forces behind an outcome (e.g., anti-neocolonial revolutions), the typical case-oriented inquiry does not assume or even anticipate causal uniformity across positive cases. On the contrary, the usual expectation is that different combinations of causes may produce the same outcome. That is, case-oriented researchers often pay special attention to the diverse ways a common outcome may be reached.

When examining similarities and differences across cases, case-oriented researchers usually expect evidence to be causally "lumpy." That is, they anticipate finding several major causal pathways in a given body of cross-case evidence. A typical finding is that different causes combine in different and sometimes contradictory ways to produce roughly similar outcomes in different settings. The effect of any particular causal condition depends on the presence and absence of other conditions, and several different conditions may satisfy a general causal requirement - that is, two or more different causes may be equivalent at a more abstract level. Thus, causal explanations in case-oriented research often have the form: "When conditions A, B, and C are present, X causes Y, however, if any one of these conditions (A, B, or C) is absent, and X - is also absent, then Z causes Y. "This argument is multiple and conjunctural in form because it cites alternate combinations of causal conditions. The hypothetical causal argument just presented essentially states that there are four combinations of conditions that result in the outcome Y. It can be formulated using Boolean algebra (see Ragin 1987) as follows:

\[ Y = ABCX + ABcxZ + AbCxZ + aBCxZ \]

(Upper-case letters indicate the presence of a condition; lower-case letters indicate its absence; multiplication indicates causal conjunctures; addition indicates alternative causal pathways.)

The search for patterns of multiple conjunctural causation, a common concern of case-oriented researchers, poses serious practical problems for variable-oriented research. To investigate this type of causation with statistical techniques, it is necessary to examine high-level interactions (e.g., four-way interactions in the causal argument just described). However, these sophisticated techniques are very rarely used by variable-oriented researchers. When they are, they require at least two
essential ingredients: (1) a very large number of diverse cases, and (2) an investigator willing to contend with a difficult mass of multi-collinearity. These techniques are simply not feasible in investigations with small or even moderate Ns, the usual situation in comparative social science. When Ns are small to moderate, causal complexity is more apparent, more salient, and easier to identify and interpret; yet it is also much less amenable to statistical analysis.

In his essay Goldthorpe (1996) laments the inability of case-oriented methods to reveal the "relative strengths of different effects or combination of effects." However, multiple conjunctural causation, as sketched here, challenges the very idea of "relative strengths." It is not possible to assess a variable's "unique" or separate contribution to the explanation of variation in some outcome unless the model in question is a simple additive model. To isolate a single causal factor and attempt to assess its separate or "independent" impact across all cases, a common concern in multivariate statistical analysis, is difficult in research that pays close attention to causal conjunctures. When the focus is on causal conjunctures, the magnitude of any single cause's impact depends on the presence or absence of other causal conditions. The impact of X on Y in the causal statement just presented, for example, requires the co-presence of conditions A, B and C. Of course, it is possible in the case-oriented approach to assess which cases (or what proportion of cases) included in a study follow which causal path. Indeed, linking cases to causal pathways and assessing the relative importance of different paths should be an essential part of case-oriented comparative research.

In variable-oriented research the assessment of relative importance is conducted across all cases and involves computing partial relationships, which, in turn, are constructed from bivariate relationships. (To compute a multiple regression, for example, only a matrix of bivariate correlations, along with the means and standard deviations of all the variables, is required.) However, bivariate relationships can give false leads, even when they are partialled. Note, for example, that condition X in the Boolean equation just described must be present in some contexts and absent in others for Y to occur. A conventional statistical analysis of the bivariate relationship between X and Y might show no relationship (i.e., a Pearson's r of 0).

Simply stated, the fourth practical concern of case-oriented researchers is causal heterogeneity. Because they conduct in-depth investigations of individual cases, case-oriented researchers are able to identify complex patterns of conjunctural causation. While researchers interested only in testing general theories might find this level of detail uninteresting, in-depth study offers important insight into the diversity and complexity of social life, which, in turn, offers rich material for theoretical development and refinement.

"DETERMINISM" AND THE TREATMENT OF NONCONFORMING CASES

Because Ns tend to be relatively small in comparative research, it is possible to become familiar with every case. Also, each case selected for examination may be historically or culturally significant in some way and thus worthy of separate attention. For these reasons, case-oriented researchers often account for every case included in a study, no matter how poorly each may conform to common causal
patterns. Thus, researchers hope to find interpretable pattern of multiple conjunctural causation, but they also anticipate finding causal specks-cases that do not conform to any of the common causal pathways. Causal specks are usually not discarded, even though they may be inconvenient. Suppose, for example, that Iran offers the only instance of anti-neocolonial revolution with a strong religious slant. Do we simply ignore this important case? Relegate it to the error vector? Call it a fluke?

The variable-oriented critics of case-oriented work argue that accounting for every case is equivalent to trying to do the impossible - explaining "all the variation" - and that this trap should be avoided. They argue that researchers instead should stick to well-known and well-understood probabilistic models of social phenomenon. This criticism of case-oriented research has two important bases. The first is that explanations that "account for every case" are deterministic, and there is simply too much randomness in human affairs to permit deterministic explanations. The second is that the effort to "explain all the variation" may lead to the inclusion of theoretically trivial causal variables or, even worse, to the embrace of theoretically incorrect causal models, understandings that take advantage of the peculiar features of a particular "sample" of cases.

These arguments can be addressed with a simple example. As is well known, the typical comparative study has a paucity of cases relative to the number of variables. This feature, in fact, could be considered one of the key defining characteristics of comparative research. Consider the typical contrast: A quantitative study of voting with 3,000 voters and 15 main variables has a statistically "healthy" ratio of 200 observations per variable (200:1). A comparative study of Third World countries with violent mass protest against the International Monetary Fund, by contrast, might have about 20 cases and 30 independent variables, an "unhealthy" ratio of 2:3. Anyone who has attempted sophisticated statistical analysis of small Ns knows that with 20 cases and 30 independent variables, it is possible to construct many different prediction equations that account for 100 percent of the variation in a dependent variable (say, the longevity of the mass protest against the IMF). No special effort is required to "explain all the variation" in this hypothetical variable-oriented analysis. The researcher would not have to "take advantage" of the "sample" or of any of its "peculiar" (i.e., historically or culturally specific) features. The high level of explained variation in this hypothetical variable-oriented study is a simple artifact of the ratio of cases to explanatory factors just as it would be in a case-oriented study of the same evidence.

No one would describe a statistical model derived in this manner deterministic simply because of the level of explained variance achieved (100%). A truly deterministic argument should involve explicit theorizing and explicit statements about the nature of the determinism involved. I know of no case-oriented or variable-oriented researcher who has proposed such an argument, even though it is always possible for researchers using either research strategy to explain "all the variation" in some outcome.

The more important issue here is the fact that many different models will perform equally well, not that it is possible to "explain all the variation." For example, suppose that with 20 cases and 30 independent variables, it is possible to derive eleven
different prediction equations, each with only five predictors, that account for 80 percent of the variation in the dependent variable. Which one should the investigator choose? The key question, of course, is the plausibility of the explanations implied by the different equations. Faced with the possibility of achieving a very high level of explained variation with many different prediction models, the variable-oriented researcher is usually stymied. The issue of plausibility cannot be resolved by running more and more equations or by plumbing the depths of probability theory. Instead, it is usually necessary to assess the relative plausibility of equivalent prediction models by going back to the cases included in the study and trying to determine at the case level which model makes the most sense. In other words, having a surplus of explanatory variables - the usual situation in comparative social science often necessitates case-oriented analysis.

Thus, when there are more independent variables than cases, the problem is not one of "determinism," where determinism is equated with 100 percent explained variation. This so-called determinism is a simple artifact of the ratio of independent variables to cases and has nothing to do with the researcher's arguments. On the contrary, the problem here is one of extreme indeterminism the fact that there may be many different models that do equally well. The best antidote for a multiplicity of equally predictive models (indeterminism) is more knowledge of cases. All researchers should be wary of models, especially simple models, that "explain every case." They should check each case to see if the model in question offers a plausible representation of the case. Most case-oriented investigators do not explain all their cases with a single model (even when the model incorporates multiple conjunctural causation).

More typically, they confront nonconforming cases and account for them by citing factors that are outside their explanatory frameworks (a procedure endorsed by Goldthorpe). The specifics of each case are not irrelevant to social science, even when knowledge of specifics has only limited relevance to theory. Consider an example developed by King and associates (1994): Weather fluctuations intensify a flu epidemic, especially among lower strata, on election day. The Labor Party thus suffers a poor turnout and loses an election it should have won. This example is a wonderful demonstration of both randomness and of our potential for identifying the play of such forces in producing nonconforming outcomes. For those interested in what happened or in winning elections (i.e., path dependency), this bit of knowledge might be very important. For those interested in studying shifts in the link between class and party support, it may simply be an annoyance (i.e., error).

The practical issue here is that "error" is usually conceived very differently in case-oriented and variable-oriented research. In qualitative research, error is a prod: Investigators try to account for every case in their attempt to uncover patterned diversity. Cases often deviate from common patterns, but these deviations are identified and addressed. Investigators make every effort to identify the factors at work in nonconforming cases, even when these factors are outside the frameworks they bring to the research. In variable-oriented research, by contrast, the "error" that remains at the end of an investigation may embrace much more than it does in qualitative research. It includes randomness, omitted variables, poor measurement, model misspecification, and other factors, including, in some research, ignorance of the cases studied.
CONCLUSION
Case-oriented and variable-oriented researchers are joined by their common objective of constructing representations of social phenomena from evidence and by their use of common concepts and analytic frames to facilitate this fundamental objective. In practice, however, cross-case qualitative research, adopts a very different approach to the work of building representations from evidence. The practical concerns sketched in this paper present a bare outline of several distinctive features of the process of case-oriented research, from the constitution of cases to the examination of uniform causes and outcomes, and from the analysis of multiple conjunctural causes to the explanation of nonconforming cases.

The case-oriented approach poses important challenges to variable-oriented research, challenges which, if answered, would make variable-oriented research more rigorous. For example,

(1) in most variable-oriented research, the sample of relevant observations is usually set at the outset of a study and is not open to reformulation or redefinition.

(2) In most variable-oriented research, the operation of causal conditions that are constant across cases is obscured.

(3) In most variable-oriented research, it is difficult to examine multiple conjunctural causation because researchers lack in-depth knowledge of cases and because their most common analytic tools cannot cope with complex causal patterns.

(4) Finally, in most variable-oriented research, ignorance of cases may find its way into the error vector of probabilistic models.

Of course, the practical concerns of case-oriented research may be difficult to address in the variable-oriented approach. It is still reasonable to hope, at a minimum, for greater appreciation of the special strengths of different ways of constructing social scientific representations of social life.