Canons of Research Design in Qualitative Analysis

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After a wave of publications on comparative methodology in the late 1960s to mid-1970s[1] and the subsequent abatement of discussion, we are now again in a period of great interest in issues pertaining to qualitative and small-N research.[2] As a sign of the vitality of this literature, the new works have focused on a large and diverse set of issues, such as case selection, conceptual stretching, process tracing, the role of historical narratives in causal inference, and multiple conjunctural causation. Indeed, few issues that affect the conduct of research have not been touched by this literature. However, there is a downside to the manner in which this debate has unfolded. While more and more issues have been put on the table and more and more suggestions have been advanced, the contributors to this literature have done little to clarify how all the various methodological proposals fit together or add up to a coherent set of methodological guidelines.[3] A primary concern, consequently, is that students of comparative politics who turn to this growing literature in search of practical advice will become bewildered and discouraged, and hence will ignore it completely.

This article seeks to make this new literature more valuable and helpful by offering an extended discussion of what is probably the single most important work among the recent spate of books and articles on comparative methodology: Gary King, Robert O. Keohane, and Sidney Verba's (KKV) *Designing Social Inquiry: Scientific Inference in Qualitative Research* (1994). The reasons for focusing on this book are many: it presumably is now the most widely read book on methodology;[4] it is the most ambitious attempt to formulate a comprehensive agenda for the field of comparative methodology; and its basic premise—that guidelines for qualitative research can be derived from the conventions used in quantitative research—has

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provoked much debate and forced many authors to articulate how they themselves stand vis-à-vis KKV's arguments. In short, *Designing Social Inquiry* has shaped and crystallized extant methodological debates and thus is the logical point of departure in an effort to take stock of these debates.

In order to use *Designing Social Inquiry* and the debate it has generated as a basis for an assessment of the state of the methodological literature, this article first provides a summary of KKV's book. This summary goes beyond what KKV themselves do by offering a new synthesis of their rich discussion. Even though KKV's book has clear pedagogical aims, the authors never present the multitude of suggestions they offer in a succinct and organized fashion. This shortcoming has made it difficult to grasp KKV's broad agenda. It has also diminished the coherence of the debate around *Designing Social Inquiry,* given that commentaries on the book have simply accepted the book's loose organization. This summary, then, is a critical first step in an attempt to use KKV's book to assess the state of comparative methodology.

Second, this article evaluates the rules KKV propose for qualitative researchers in terms of three criteria: the novelty of these rules in light of ongoing debates about qualitative methodology, the degree of consensus or disagreement regarding these rules that is likely to emerge among qualitative researchers, and the degree to which these rules specify procedures that are sensitive to the practices of qualitative research. The point of this exercise is both to assess the alternative views that have emerged in various critical reviews and to examine KKV's recommendations in light of the considerable stock of knowledge on comparative methodology that qualitative researchers customarily draw on.[5]

This assessment shows that KKV offer some genuine contributions to our understanding of qualitative research. However, I also argue that KKV undervalue important insights of qualitative methodologists, and specifically the effort of these researchers to show that 1) defining the universe of cases is a complex task that may be influenced by concerns that arise from contextually grounded analysis, from the effort to construct analytically appropriate typologies, and from the kind of learning about cases that occurs through process tracing; 2) case selection can be influenced by a concern with conceptual validity that arises from potential problems of conceptual stretching, and also by a recognition that in some contexts of research, "no-variance" designs can make a contribution; 3) in data collection, concern with issues of validity can lead the investigator to employ system-specific indicators and/or contextualized comparisons; and 4) causal assessment may take different forms, depending on the understanding or model of causal processes employed.

By way of conclusion, I suggest that the terms of the dialogue between quantitative and qualitative researchers KKV propose should be refrained, so as to move beyond their attempt to derive rules for qualitative research primarily by extending practices commonly used by quantitative researchers. I propose a more multifaceted dialogue, which acknowledges the distinctive contributions of qualitative researchers and which seriously advances the idea of a multimethod approach to shared research questions.

King, Keohane, and Verba's Guidelines for Qualitative Research[6]

Summarizing KKV's book is not easy, given the range of topics the authors discuss and the large number of concrete recommendations they offer qualitative researchers. One way to do justice to KKV's efforts is to identify the broad array of methodological issues they discuss, to provide succinct statements that condense their practical advice on each point in their own words, and to organize the resulting list of methodological rules according to their place within what may be thought of as a research cycle (see Figure 1).

The notion of a research cycle is valuable for a number of reasons. Because it provides an overall view of the research process, it offers a framework in which KKV's recommendations can be presented in an organized fashion, thus allowing us to gain a clear sense of the agenda they advance. Moreover, because it makes explicit the sequential connection between various steps in the research process, it provides a framework that more clearly identifies the significance of each methodological rule, as well as a basis for thinking about the specific strengths and weaknesses of different research strategies. Finally, because it draws attention to the whole process of research, it provides an indispensable tool for identifying methodological gaps that deserve greater attention. The idea of a research cycle, in sum, provides a framework for the kind of concise summary of KKV's book that these authors never offer,[7] while also laying the groundwork for my subsequent attempt to provide an overall assessment of their agenda and to take stock of the state of methodological debates.

Rules for Qualitative Research[8]

A. Defining the Research Problem

1. *Relevance I: The "real" world.* "A research project should pose a question that is 'important' in the real world" (p. 15).
2. *Relevance II: The scholarly literature.* "A research project should make a specific contribution to an identifiable scholarly literature by increasing the collective ability to construct verified scientific explanations of some aspect of the world" (p. 15, see also pp. 16–17).
3. *Testability: Can it be studied?* "A proposed topic that cannot be refined into a specific research project permitting valid descriptive or causal inference should be modified along the way or abandoned" (p. 18).

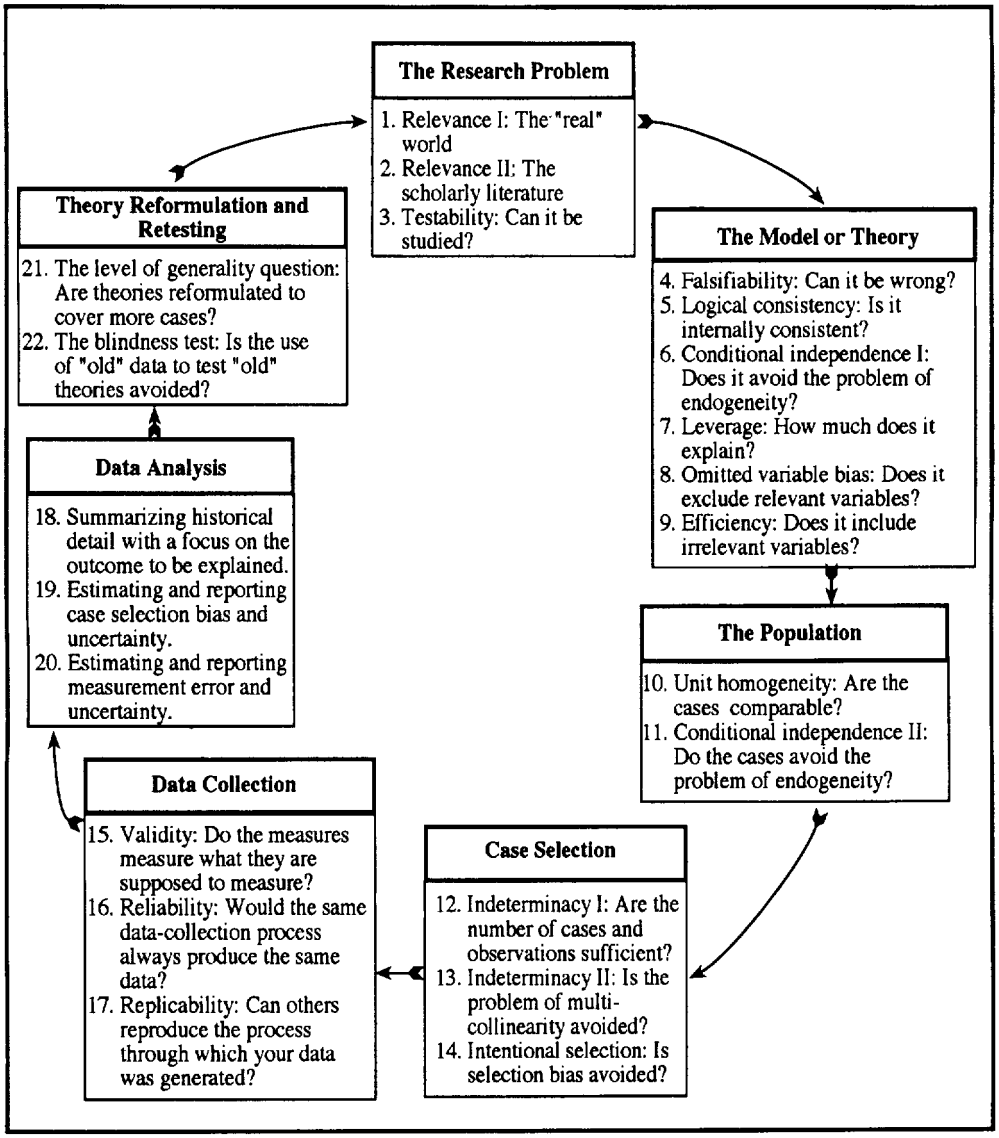
B. Specifying the Model/Constructing Theories

4. *Falsifiability: Can it be wrong?* "Construct falsifiable theories," that is, "choose theories that could be wrong" (p. 100, 19).
   - 4a. "To make sure a theory is falsifiable, choose one that is capable of generating as many observable implications as possible" (p. 19).
   - 4b. "In designing theories, be as concrete as possible. . . . Theories that are stated precisely and make specific predictions can be shown more

easily to be wrong and are therefore better" (p. 20, see also pp. 109–12).

5. *Logical consistency: Is it internally consistent?* "Build theories that are internally consistent . . . [for] if two or more parts of a theory generate hypotheses that contradict one another, then no evidence . . . can uphold the theory" (p. 105).
6. *Conditional independence I: Does it avoid the problem of endogeneity?* When constructing theories, "choose explanatory variables that are clearly exogenous and dependent variables that are endogenous" (p. 108, see also p. 94). In other words, avoid the problem of endogeneity, when "the values our explanatory variables take on are . . . a consequence, rather than a cause, of our dependent variable" (p. 185).
7. *Leverage: How much does it explain?* "Maximize leverage by limiting the number of explanatory variables" and "explaining as much as possible with as little as possible" (p. 123, 29, see also pp. 104–05).
   7a. "State theories in as encompassing ways as feasible" and "list all possible observable implications of [your] hypothesis that might be observed in [your] data or in other data" (p. 113, 30).
8. *Omitted variable bias: Does it exclude relevant variables?* "Systematically look for omitted control variables and consider whether they should be included in the analysis," for "if . . . some omitted variable . . . is correlated with the included explanatory variable and has an effect on the dependent variable . . . then failure to control for it will bias [your] estimate . . . of the effect of the included variable" (p. 172, 170).
   8a. To avoid "omitted variable bias," "list all variables that, according to [your] theoretical model, could cause the dependent variable. . . . In general, [you] should not control for an explanatory variable that is in part a consequence of [your] explanatory variable. Having eliminated these possible explanatory variables, [you] should then control for other potential explanatory variables that would otherwise cause omitted variable bias—those that are correlated with both the dependent variable and with the included explanatory variables" (p. 174).
9. *Efficiency: Does it include irrelevant variables?* Do not "collect information on every possible causal influence," for "the inclusion of irrelevant variables can be very costly," that is, it would not be an efficient use of data for the purpose of drawing inferences. "Even if the control variable has no causal effect on the dependent variable, the more correlated the main explanatory variable is with the irrelevant control variable, the less efficient is the estimate of the main causal effect." Thus, while "the best solution is to always collect more observations . . . if this is not possible, researchers are well-advised to identify irrelevant variables and not control for them" (pp. 182–84).

FIGURE 1
The Research Cycle and Methodological Rules for Qualitative Analysis



C. Defining the Population or Universe of Cases

10. *Unit homogeneity: Are the cases comparable?* Theories should be tested on data from a universe of cases that meet the criterion of "unit homogeneity," where "unit homogeneity is the assumption that all units with the same value of the explanatory variable have the same expected value of the dependent variable" (p. 91, see also p. 116).
11. *Conditional independence II: Do the cases avoid the problem of endogeneity?* Theories should be tested on data from a universe of cases that meet the criterion of "conditional independence," where "conditional independence is the assumption that . . . the values of the explanatory variables are not caused by the dependent variables" (p. 94).

D. Selecting Cases

12. *Indeterminacy I: Are the number of cases and observations sufficient?*[9] Avoid indeterminate research designs, that is, ones in which "virtually nothing can be learned about the causal hypothesis" because you "have more inferences to make than implications observed" (p. 118, 119, see also pp. 213–17). There are two solutions to such a problem:
    - 12a. Increase the number of cases, refocusing the study when such cases do not exist (p. 120, see also pp. 24, 47).
    - 12b. Increase the number of "observations measured at a different level of analysis" (p. 120, see also pp. 217–28).
13. *Indeterminacy II: Is the problem of multicollinearity avoided?* Avoid indeterminate research designs in which there is a problem of multicollinearity, that is, when you "have two or more explanatory variables in [your] data that are perfectly correlated with each other" (p. 119). As with rule No. 12, there are two solutions to such a problem:
    - 13a. "Collect additional observations" (p. 123).
    - 13b. "Search for observable implications at some other level of analysis" (p. 123).
14. *Intentional selection: Is selection bias avoided?* "When [you] are able to focus on only a small number of observations, [you] should rarely resort to random selection of observations," for "random selection of observations in small-n research will often cause very serious biases. . . . Usually, selection must be done in an intentional fashion," which forces one to "consider how the criteria used relate to each variable" (p. 139, 126, 129, see also pp. 196–99):
    - 14a. *Selecting on the independent variable.* "The best 'intentional' design selects observations to ensure variation in the explanatory variable (and any control variables) without regard to the values of the dependent variables," because "selecting observations for inclusion in a study according to the categories of the key causal explanatory variable causes no inference problems" (p. 140, 137, see also pp. 146–47).
    - 14b. *Selecting on the dependent variable.* When selecting on the dependent

variable, "choose a dependent variable that represents the variation [you] wish to explain" and select cases that "allow for the possibility of at least some variation on the dependent variable" (p. 108, 129, see also pp. 147–49).

14c. *Selecting on both the independent and dependent variables.* "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" (p. 142).

14d. *Selecting on variables that are constant.* "The causal effect of an explanatory variable that does not vary cannot be assessed. . . . We can also learn nothing about a causal effect from a study which selects observations so that the dependent variable does not vary" (p. 146–49, 108–09, 129).

E. Collecting Data[10]

15. *Validity: Do the measures measure what they are supposed to measure?* "Maximize the validity of [your] measurements. Validity refers to measuring what [you] think [you] are measuring" (p. 25).
    15a. "Use the measure that is most appropriate to [your] theoretical purposes" (p. 153).
16. *Reliability: Would the same data-collection process always produce the same data?* "Ensure that data-collection methods are reliable. Reliability means that applying the same procedure in the same way will always produce the same measure" (p. 25).
17. *Replicability: Can others reproduce the process through which your data was generated?* Because "all data and analyses should, insofar as possible, be replicable . . . record and report the process by which the data are generated" (pp. 26, 23, see also p. 51).[11]

F. Analyzing Data

18. *Summarizing historical detail with a focus on the outcome to be explained.* "Summaries should focus on the outcomes that [you] wish to describe or explain" (p. 54).
    18a. "After data are collected, the first step in any analysis is to provide summaries of the data" (p. 53)
    18b. "A summary must simplify the information at [your] disposal" (p. 54).
19. *Estimating and reporting case selection bias and uncertainty.* "If selection bias is unavoidable, [you] should analyze the problem and ascertain the direction and, if possible, the magnitude of the bias, then use this information to adjust [your] original estimates in the right direction. . . . If all else fails—that is, if [you] know there is bias but cannot determine its direction and magnitude— . . . at least increase the level of uncertainty [you] use in describing [your] results" (p. 133, 199, see also pp. 128–37, 168–82).
20. *Estimating and reporting measurement error and uncertainty.* "Since all

observation and measurement . . . is imprecise . . . estimate the amount of [measurement] error" and "offer uncertainty estimates in the form of carefully worded judgments about [your] observations" (pp. 151–52).

G. Reformulating and Retesting Theory

21. *The level of generality question: Are theories reformulated to cover more cases?* "Ad hoc adjustments in a theory that does not fit existing data must be used rarely." However, when a theory is altered after observing the data, you "can make the theory less restrictive (so that it covers a broader range of phenomena and is exposed to more opportunities for falsification), but [you] should not make it more restrictive without collecting new data to test the new version of the theory" (pp. 21–22).
22. *The blindness test: Is the use of "old" data to test "old" theories avoided?* While you "should always . . . avoid using the same data to evaluate the theory [you] used to develop it," the original data set can be used to test a new implication of a theory, "as long as the implication does not 'come out of' the data but is a hypothesis independently suggested by the theory or a different data set" (p. 46, 30).

As this summary shows, KKV's book offers a comprehensive set of rules for qualitative research. For this reason, *Designing Social Inquiry*, as opposed to other more limited and partial attempts at articulating an agenda for qualitative methodology, provides a particularly useful point of reference in an effort to take stock of methodological debates. Obviously, a central concern facing qualitative researchers is to ascertain whether the proposed set of rules serve as adequate guidelines for the kind of research they carry out. In the next section, therefore, we turn to a consideration of the value of the KKV's proposed guidelines, stressing three criteria: the novelty of these rules in light of ongoing debates about qualitative methodology, the degree of consensus or disagreement regarding these rules that is likely to emerge among qualitative researchers, and the degree to which these rules specify procedures that are sensitive to the practices of qualitative research.

New Concerns: Points of Consensus and Problems of Specification

It is important to stress at the outset, that there are several insights that KKV draw from quantitative methodology that are genuine contributions to the debate on qualitative research. For example, although the need to study testable problems (rule No. 3) and develop falsifiable and logically consistent theories that avoid the problem of endogeneity (rules No. 4, 5, and 6) have long been recognized by qualitative researchers (Warwick and Osherson 1973a, 14–17), KKV specify how qualitative researchers might address these concerns with greater clarity and force than do writings on qualitative research. The applicability to qualitative research of these rules, which originate in the work of philosophers of science like Karl Popper but which have become part of the convention of quantitative research, is thus hard to

dispute. Indeed, these rules are so fundamental to the pursuit of good social science that it would be unwise to downplay their value.[12]

The same argument can be made about other issues that are standard concerns within the quantitative tradition, yet rarely receive much consideration in qualitative research. Among them are KKV's insistence that researchers build theories that include neither too many variables (rule No. 9) nor too few variables (rule No. 8), and that, when testing hypotheses, researchers guard against the problem of multicollinearity (rule No. 13). This would appear to be an obvious piece of advice but explicitly insisting on it is valuable. The significance of this advice can be seen by way of an example: the attempt to theorize about the effect of institutions on democratic stability. In this literature, many authors have focused on the difference between parliamentary and presidential forms of government, eschewing consideration of a large number of potentially irrelevant variables. According to KKV, however, a single-minded focus on institutional structures would violate rule No. 8, given that other variables, such as the country's level of economic development, could also affect democratic stability. The concern with efficiency should be balanced against a concern that all relevant variables are considered. But, in turn, the attempt to test the causal effects of these different variables could create a new problem, that of multicollinearity, given that these additional explanatory variables may be correlated. This would be the case, for example, if a researcher sought to compare the cases of stable democratic regimes of Western Europe, which are examples of both parliamentarism and developed economies, with the less stable democracies of Latin America, which are examples of presidentialism and developing economies. These are frequently encountered problems that qualitative researchers have not always acknowledged and KKV do raise awareness of the kind of pitfalls which they can cause.

There is also much in KKV's book that is particularly relevant for qualitative researchers in light of their characteristic familiarity with the cases they study. In this regard, KKV's insistence on the need for reliable (rule No. 16) and replicable (rule No. 17) data merits consideration. The difficulties qualitative researchers face concerning these issues are substantial. Indeed, in some cases, as when data are gathered through confidential interviews, it is practically impossible to repeat the data-collection process and thus assess data reliability and replicability. At the same time, it is crucial that qualitative researchers do their best to avoid the tendency to present data that are justified on idiosyncratic grounds, such as the authority of the author as someone knowledgeable about the case. In other words, they must acknowledge KKV's (p. 8) basic point that one of the hallmarks of social science is that its "procedures are public."[13] Another potential drawback to the qualitative researcher's familiarity with the cases being studied, which KKV wisely warn against, is the danger of data "fitting" through the related practices of adjusting a theory to fit the data (rule No. 21) or using the same data both to generate and test ideas (rule No. 22).[14] The downside of this advice is that it may inhibit learning from cases (Collier 1999). Nonetheless, KKV's advice reminds us that an important trade-off is entailed in such learning.

In short, KKV address some issues that are central to the conduct of social research but that either have not been acknowledged enough by qualitative researchers or represent concerns that are novel from the perspective of qualitative researchers. Moreover, they offer sound and specific advice, which is likely to be accepted by most qualitative researchers. Indeed, these rules show that qualitative researchers have much to gain from considering the insights KKV derive from conventions of quantitative research.

A second set of rules, which also raise novel methodological concerns, make less of a contribution due to KKV's insufficient attention to what could be called the challenge of rule translation, that is, the need to refine rules used in quantitative research in ways that fit the context of the specific practices of qualitative research.[15] At many points, KKV show sensitivity to the specific dilemmas faced by qualitative researchers (Collier 1995a, 461). This sensitivity is amply demonstrated, for example, when they argue why qualitative researchers should not select their cases randomly, even though such a method is widely regarded as a standard way to avoid selection bias in large-N studies, and why they should opt instead for an intentional form of selection (pp. 124–28, 139, 200–06). But this awareness is not sustained throughout.

Indeed, when it comes to other methodological problems, KKV fail to acknowledge the conditions under which qualitative research is conducted and, as a result, propose rules that offer little specific and useful guidance that are more a source of confusion than insight. One example of such a shortcoming is KKV's argument that qualitative researchers must have a sufficient number of observations or cases so as to avoid an indeterminate research design, that is, a research design that precludes causal inferences because it has more inferences than observations or cases (rule No. 12). Qualitative researchers acknowledge this problem, often referring to it as the "many variables, small N" problem (Lijphart 1971, 685–86; Smelser 1973, 77, 43); thus, they would naturally welcome advice on this point. It remains unclear, however, how a qualitative researcher would translate into concrete and clear advice the formula KKV offer to determine the precise number of cases or observations a researcher needs (pp. 213–17). Another example is KKV's persuasive argument that qualitative researchers should admit that all knowledge is at least partially flawed or imperfect and thus should emulate their quantitative counterparts in seeking to estimate the different forms of error and bias that occur in descriptive and causal inference (rules No. 19 and 20). Again, when it comes to practical suggestions, they offer little concrete advice as to how qualitative researchers would implement these rules (Bartels 1995, 9–10).

In sum, with regard to this set of rules, KKV make a good case for considering certain methodological issues that are standard concerns among quantitative researchers but have been ignored or not stressed sufficiently by qualitative researchers. Nonetheless, their effort falls short because they do not suggest procedures that provide qualitative researchers with specific guidance about how to apply such rules in their research. In other words, the reservations I am raising here do not involve any disagreement over the advice KKV offer. Rather, they seek to stress how

KKV's failure to adequately translate insights between different research traditions gives rise to suggestions that are at best vaguely specified.

Old Concerns: Points of Disagreement and Further Problems of Specification

Other rules KKV discuss do not represent novel problems from the perspective of qualitative methodology, but rather point to overlapping concerns that quantitative and qualitative researchers have grappled with for some time. In some cases, KKV's discussion of these issues shows that quantitative and qualitative researchers have converged on similar solutions. For example, their sound and clear advice about the importance of focusing on important research problems (rules No. 1 and 2) echoes the writings of qualitative methodologists (George 1979, 54), as does their suggestion that researchers should strive to increase the leverage of their theories by limiting the number of explanatory variables (rule No. 7).[16] In most other cases, however, the rules KKV propose as solutions to these overlapping concerns are not clearly specified, despite suggestions available in the qualitative methodology literature, and do not take note of the different perspective qualitative researchers have sometimes brought to these issues.

Indeed, an important handicap of KKV's work is that it pays scant attention to the insights qualitative researchers have offered on a range of methodological issues, especially concerning concept formation and measurement, but also regarding causal inference.[17] These omissions lead them to miss the opportunity to formulate procedures that would more clearly specify some of the key rules they discuss. Even more seriously, these omissions lead them to overlook how some of their proposals might be highly debatable, in light of different perspectives on the conduct of qualitative research. As I will seek to show, these shortcomings are anything but minor. Rather, the preliminary and contested status of KKV's effort to provide guidelines for qualitative researchers is apparent in four key aspects of the research process: the definition of the population, case selection, data collection, and data analysis.

Defining the Population: The Uses of Context, Typologies, and Process Tracing

KKV ignore important insights from qualitative research about the definition of the population from which researchers pick their cases. To tackle this problem, KKV warn qualitative researchers that their cases should be selected from a universe of cases that 1) are conceptually equivalent and comparable, that is, that meet the criterion of unit homogeneity (rule No. 10), and 2) avoid the problem of endogeneity, that is, that meet the criterion of conditional independence (rule No. 11).[18] But they also make the unhelpful argument that unit homogeneity and conditional independence are ultimately "untestable assumptions" (p. 91). The problem that leaving such assumptions untested all too often leads quantitative researchers to include in their studies "a jumble of dissimilar cases," as Henry Brady (1995, 13) recognizes, is never pursued by KKV. Likewise, KKV overlook the fact that quali-

tative researchers do sometimes explore precisely these issues (Ragin 1987, 16).[19] It is instructive, then, to consider how qualitative researchers deal with this problem.

To ensure unit homogeneity or the comparability of their cases, qualitative researchers often turn this assumption into an "initial 'hypothesis'" that must be investigated at the outset of the research process (Ragin, Berg-Schlosser, and Meur 1996, 752–53; see also Ragin 1997, 30–32). As a consequence, this may lead the researcher to conclude that an initial definition of the population is not defensible and that certain cases are actually cases of something different than what was initially thought. In other words, by insisting on the need to ask of a given unit being studies, what is this a case of? Qualitative researchers have forced themselves to evaluate and possibly reconceptualize the relevant population, producing in the process carefully calibrated scope or domain statements (Collier 1995a, 461, 465; Ragin, 1997, 31, 41).[20]

Qualitative researchers have expressed their concern with comparability in various ways. One is via the frequently invoked notion that certain theories are *context-sensitive* and that they do not apply beyond the domain of cases in which the same context is found.[21] Thus, Theda Skocpol (1979, 40–42, 287–92; 1994, 4–7) argues that it would be a mistake to apply her theory of revolution directly to twentieth century revolutions because one of the key contextual variables that characterizes the cases she studied, the presence of agrarian-bureaucratic monarchies that had not experienced colonial domination, is simply not present in most twentieth century revolutions.[22] Another example is found in warnings not to extend various theories developed in the context of stable institutionalized democracies in the North directly to the new democracies of the South, where stability and institutionalization have not been achieved (O'Donnell 1994, 1996).[23] In other words, statements about context are frequently used to distinguish cases in which the same variable is likely to operate differently and have a different effect due to its interaction with other contextual variables. These statements express scope conditions and put boundaries on generalizations (Collier 1995a, 465).[24]

A second way in which qualitative researchers have expressed their concern with comparability has been through the use of *typologies*. KKV (pp. 48) discuss typologies only in passing and rapidly dismiss them as an useless tool. Nonetheless, as Alexander George and Timothy McKeown (1985, 28–29, 45) argue, typologies can play a significant methodological role in that they can be used to define the universe of comparable cases (see also Stinchcombe 1968, 43–47; Ragin 1987, 20, 149; Brady 1995, 13, 18). Fruitful examples of this usage abound in the literature, from Juan Linz's (1964) effort to distinguish cases such as Spain under Franco from communist-led dictatorships with a typology that differentiates between authoritarian and totalitarian regimes, to Philippe Schmitter's (1974) effort to distinguish patterns of interest intermediation more commonly found in Western Europe from those more typical of Latin America through the contrast between societal and state forms of corporatism; to Guillermo O'Donnell's (1973; 1994; 1996) effort first to distinguish the authoritarian regimes of Brazil and Argentina in the 1960s from earlier authoritarian regimes through the concept of bureaucratic authoritarianism, and his more recent

attempt to distinguish Western European democracies from the democracies that have recently emerged in Latin America through the concept of delegative democracy and the contrast between formally and informally institutionalized polyarchies. Indeed, it is crucial to acknowledge how typologies are used as a tool that helps researchers clarify why certain cases are seen as part of a certain universe of cases while others are seen as non-comparable and how typologies can be used to provide precise and well grounded statements concerning the scope of theories.[25]

In the work of many qualitative researchers the assumption of conditional independence, that is, the assumption that the values of the explanatory variables are not caused by the dependent variables likewise sometimes receives attention.[26] For example, the *process tracing* method (George and McKeown 1985, 34–41) allows researchers to meticulously identify and analyze the historical sequence whereby potential explanatory variables affect outcomes. This focus on causal mechanisms and causal chains can be used to ascertain the direction of causality and whether a posited factor is a causal variable, thus providing a check against the problem of endogeneity that is appropriate to non-experimental settings.[27] In other words, the in-depth knowledge qualitative researchers have of their cases allows them to ascertain changes in variables over time, with a clear sense of what came first. This, in turn, allows these researchers to verify the assumption of conditional independence and move beyond an approach to explanation only based on correlations.[28]

Case Selection: Conceptual Stretching and No-Variance Studies

The implications of literature on the comparative method are once more overlooked by KKV when they discuss research designs which are indeterminate due to a lack of a sufficient number of cases or observations (rule No. 12). The solutions KKV offer to this problem, while not particularly novel, show an important degree of convergence between their thinking and conventions in qualitative research. Thus, the suggestion to increase the number of cases (rule No. 12a) is a point already stressed in various standard sources about qualitative methodology (Lijphart 1971, 686; Smelser 1976, 198–202). The pointed and useful advice KKV offer to increase the number of observations (rule No. 12b) again matches a long-standing concern of qualitative researchers with employing various forms of within-case analysis.[29]

Two important points, however, must be raised concerning the advice to increase the number of cases. First, this advice may be "neither feasible nor necessarily desirable" (Ragin, Berg-Schlosser, and Meur 1996, 752) and in many ways amounts to little more than saying that "qualitative researchers are inherently handicapped" and that they should "not be 'small-N' researchers" after all (Brady 1995, 12–13; see also McKeown 1999: 165–66).[30] Second, even if a qualitative researcher may want to study more cases, the advice KKV offer does nothing to recognize an important problem, repeatedly stressed by qualitative methodologists, that could undermine the effort to increase the number of cases studied: the problem of *conceptual stretching*. This term refers to the problem of taking concepts that 1) validly apply to a set of given cases and extending them to 2) a new set of cases that are not

conceptually equivalent or comparable vis-à-vis the original cases.[31] Moreover, rather than seeing this problem as an insurmountable obstacle that would simply make comparative analysis untenable, the pioneering work on conceptual stretching by Giovanni Sartori (1970; 1984; 1991), recently reworked and refined by David Collier and his collaborators,[32] has sought to spell out a set of procedures that would guide the process of reconceptualization that is necessary to avoid conceptual stretching. It is crucial, then, to acknowledge how some important insights developed by qualitative methodologists, which in this case clearly supplement KKV's work, go considerably further than KKV in offering practical suggestions for dealing with a fundamental methodological challenge.[33]

KKV also dedicate much attention to another aspect of case selection: the procedures that should guide the selection of specific cases so as to avoid biased estimates of causal effects, that is, the problem of selection bias (rule No. 14).[34] They argue persuasively that selecting on the dependent variable can yield cases that score either high or low on that variable and that biased estimates of causal effects can result. Qualitative researchers are therefore advised to select their cases on the independent variable, a procedure that eliminates such a bias. The relevance of this advice, within the framework of studies that focus on covariation, should be underlined. Qualitative researchers do tend to select their cases on the dependent variable (Ragin, Berg-Schlosser, and Meur 1996, 752). Moreover, because the problem of selection bias cannot be overcome through within-case analysis, as Collier and James Mahoney (1996, 63–64, 69–71) explain, it does affect the ability of small-N researchers to estimate causal effects, whether or not they are interested in generalizing beyond the cases they study. Thus, this is serious and sobering advice and qualitative researchers "should understand this form of bias and avoid it when they can" (Collier and Mahoney 1996, 65).[35]

However, the advice KKV offer concerning the problem of selection bias is premised upon a restricted approach, at the heart of conventional statistical analysis, that assesses distortions of causal inference on the basis of an analysis of covariation between independent and dependent variables (Lieberson 1985, 90–91; Ragin 1994, 107, 145–48). Inasmuch as qualitative work does assess causal effect through an analysis of covariation, a fairly frequent practice (Collier and Mahoney 1996, 65),[36] KKV's insistence that studies that include no variation either on the explanatory or dependent variable should be avoided (rule No. 14d) is relevant to qualitative researchers. But this approach entails an important limitation, in that it entirely overlooks the perspective suggested by Stanley Lieberson (1985, 224), who argues that "we need to know why the phenomenon exists before we worry about variation in it" and that "variation explained . . . does not tell us why something exists to begin with." This alternative perspective is at the core of much qualitative research, which focuses on problems and processes, rather than on covariation, and which employs what may be called a *no-variance design*. Such studies are seen as a fundamental stage in the research process, helping to generate the kind of insights about political phenomena without which the analysis of covariation would be a blind exercise. But KKV's insistence on avoiding studies with variables that are constant would lead

researchers to bypass entirely what may well be a critical stage in the research process. The appropriateness of this advice, then, is highly debatable.[37]

Data Collection: Validity, System-Specific Indicators, and Contextualized Comparison

With regard to issues of data collection, KKV's lack of attention to standard methodological texts on, and the established practices of, qualitative research, is again apparent in their impoverished discussion of validity. KKV are on solid ground in calling for qualitative researchers to maximize the validity of their measurements (rule No. 15). However, they do little to incorporate prior work by comparativists that have grappled with the problem of validity[38] or to acknowledge the difficulty of developing equivalent indicators across different cases. The nature of the problem is, simply, that much as words can take on different meanings when used in different contexts, so too can indicators measure different things in different contexts. To take a traditional example, while the magnitude of economic activity can be measured quite accurately in monetary terms in Western societies, money is an incomplete indicator in less developed societies that have not become monetized to the same extent (Smelser 1973, 69). More recently, concerns with this indicator arise due to the magnitude of the extra-legal or underground economy in many developed countries. Thus, a researcher cannot assume that the same indicator will be a valid measure of a concept across different cases and time periods.

Qualitative researchers have, for the most part, not been self-conscious about what steps they should take to ensure data validity. Nonetheless, as Collier (1998a, 5) suggests, the close familiarity that qualitative researchers tend to have of their cases has allowed them to implicitly follow the long-standing advice by Adam Przeworski and Henry Teune (1970, chap. 6) to construct *"system-specific" indicators* as opposed to "common" indicators. Further recommendations to tackle this problem have been offered more recently by Richard Locke and Kathleen Thelen (1995), who urge scholars to engage in a process of *contextualized comparison*.[39] Thus, KKV's discussion can be criticized on two grounds. First, they ignore a critical literature, merely making the general argument that researchers should ensure the validity of their data, but failing to focus on specific procedures for accomplishing this in the context of comparative research. Second, KKV fail to even note that the sensitivity to context that researchers bring to small-N studies actually puts them on a stronger footing concerning issues of validity than researchers conducting large-N studies. Surprisingly, an important reason for choosing to conduct small-N studies is simply ignored.

Data Analysis: Causal Assessment, Causal Models, and Mill's Methods

Finally, concerning data analysis, KKV ignore a series of fundamental issues that qualitative researchers face in assessing causal arguments. Essentially, *Designing Social Inquiry* is based on the most basic and common regression model, the OLS (Ordinary Least Squares) regression model. One critical implication of this, as

indicated above, is that KKV are only able to think in terms of covariation. This approach has significant limitations, as discussed at length by Lieberson (1985), including its inability to acknowledge the value of no-variance studies. Another critical limitation of this approach is that it implies a restricted view of causation that does little to reflect the manner in which qualitative researchers think about causation and the trade-offs they grapple with.

The first serious shortcoming associated with KKV's view of causality is their inability to acknowledge that much qualitative work operates with a deterministic as opposed to a probabilistic model of causation (Ragin and Zaret 1983, 743–44; Ragin 1987, 15–16, 39–40, 52), and to recognize the significance of the long-standing discussions among qualitative methodologists on how to assess deterministic causes. A deterministic understanding of causation, which allows the analyst to reject a potential explanatory factor on the basis of a single deviation from an overall pattern of matching, is implicit in arguments that even single case studies can be used to test theories, as in Arend Lijphart's (1971, 692) "crucial experiments" or Harry Eckstein's (1975, 113–32) "crucial case studies" (see also Rogowski 1995, 469–70). Moreover, this view of causation underlies John Stuart Mill's (1893, 278–91) *methods of difference and agreement*, which correspond to what Przeworski and Teune's (1970, 32–39) label as "most similar" and "most different" systems designs. Yet KKV do little to acknowledge the status of single case studies or the significance of debate over the relative merits of Mill's two methods (Przeworski and Teune 1970, 31–39; Lijphart 1971, 687–90, 1975).[40] The fact that small-N researchers have a set of standard tools that can constitute powerful means for causal assessment is all but glossed over.[41]

A second and just as damaging shortcoming is that KKV do not recognize that qualitative researchers also have used Mill's less discussed *method of concomitant variation*, a method which resembles KKV's statistical thinking more closely (Mahoney 1999). This method has the disadvantage of not providing researchers studying a small set of cases with a clear basis for eliminating potential explanatory factors but, as a method for assessing partial and probabilistic models of causation, it has the advantage of acknowledging the contingent nature of many political outcomes. Thus, as Mahoney (1999) argues, whether small-N researchers have formally presented their work in terms of these methods or not, they have drawn upon a variety of tools for causal assessment, each characterized by their own strengths and weaknesses, and have even explored the possibility of combining various methods.[42] Yet KKV virtually ignore these developments and thus provide no specific advice about what tools of causal assessment may be more appropriate for different causal models, nor any sense of how these various tools may be combined and what trade-offs may be involved.[43]

The importance of acknowledging how different models of causation raise distinct challenges for causal assessment extends well beyond debates about Mill's methods. An old concern of comparativists has been the need to analyze "historical" as opposed to "constant" causes (Stinchcombe 1968, 101–29) in their studies of a number of crucial problems such as party systems, regime change, and economic

transformations (Lipset and Rokkan 1967; Collier and Collier 1991; Stark 1992; Ekiert 1996), and some have offered exemplary discussions of the problem of testing historical explanations (see Collier and Collier 1991, esp. chap. 1). This concern has dovetailed with more recent work on critical junctures and path dependence, which seeks to ascertain what range of phenomena call for critical juncture/path dependent models and what implications such an understanding of causation have for efforts at causal assessment (Jackson 1996, 722–26, 730–45; Pierson 1997). Yet KKV fail to notice how many of the central interests of political scientists force them to deal with path dependent processes that fail to fit the assumptions of the OLS regression model (Abbott 1988; Abbott 1992, 432–34) and why, as John Jackson (1996, 730–45) shows, conventional statistical tools are inadequate when applied to path dependent outcomes.[44]

It bears stressing that this is not an issue that divides qualitative and quantitative researchers. Indeed, much of the impetus for this line of inquiry comes from quantitative researchers who have increasingly recognized that many problems they seek to explain do not reflect the assumption of conventional regression models and thus cannot be appropriately addressed with conventional statistical tools.[45] In short, KKV not only fail to reflect the state of debate on causal assessment within qualitative circles. They even do a poor job of acknowledging how quantitative methodologists are struggling with parallel issues.

Conclusion: Reframing the Dialogue between Quantitative and Qualitative Researchers

In drawing conclusions from this assessment of *Designing Social Inquiry*, I wish first to emphasize its positive contributions. KKV's work is driven by a core premise: that "the same logic on inference" underlies quantitative and qualitative research (p. 3). This idea is powerful. Without some sense of a common scientific aspiration, there would be no grounds whatsoever for a dialogue between quantitative and qualitative researchers. Moreover, the insistence on a common underlying logic does much to dispel the misconception that qualitative research cannot be systematic and scientific. Just as quantitative researchers can draw upon a series of methodological rules, so too can qualitative scholars turn to a series of guidelines that provide a basis for the systematic conduct and evaluation of research.

However, KKV give a specific and more contentious meaning to their general point concerning the common logic that underlies quantitative and qualitative research (McKeown 1999). Practically speaking, what the broad premise about the unity of social sciences means for KKV is that guidelines for qualitative research should be established by extending the lessons of the most basic and common regression model—the OLS model—because the rules of inference have been more clearly established in this context (p. 6). This strategy is not without merit. As I have sought to show, KKV's effort to extend methodological conventions used in quantitative research to qualitative research does make significant contributions to the discussion of qualitative research, raising novel concerns and offering clear guidelines concerning important methodological issues. Yet their peculiar interpre-

tation of the unity of social sciences seriously limits their ability to offer useful guidelines for qualitative researchers. This limitation is most evident in the case of those rules that could have been specified more clearly had they drawn upon the existing literature on qualitative methodology or that are of debatable appropriateness in light of the distinct perspective qualitative scholars have of the research process. Indeed, two major problems associated with KKV's attempt to construct guidelines for qualitative researchers flow directly from their contestable understanding of the unity of social sciences.

One problem with this approach is its depiction of the state of knowledge both within quantitative and qualitative methodology. Perhaps the most surprising and damaging critique is that this approach misrepresents the state of knowledge in quantitative methodology. KKV's approach assumes that quantitative methodologists are in a position to offer qualitative researchers clear solutions to the various methodological problems they face. But, as prominent quantitative methodologists insist, this is not the case (Bartels 1995, 11; Brady 1995, 18; Jackson 1996, 742–45). As Larry Bartels (1995, 11) suggests, the problem is simply that KKV "promise a good deal more than . . . [they] could possibly deliver given the current state of political methodology." In addition, as this article stresses, KKV implicitly assume that qualitative methodologists have little to offer by way of guidelines for research and thus persistently undervalue the contributions of qualitative methodologists (McKeown 1999: 166–67).

As I have argued, qualitative methodologists have offered important recommendations in at least four key areas. They have shown that: 1) defining the universe of cases is a complex task that may be influenced by concerns that arise from contextually grounded analysis, from the effort to construct analytically appropriate typologies, and from the kind of learning about cases that occurs through process tracing; 2) case selection can be influenced by a concern with conceptual validity that arises from potential problems of conceptual stretching, and also by a recognition that in some contexts of research, "no-variance" designs can make a contribution; 3) in data collection, concern with issues of validity can lead the investigator to employ system-specific indicators and/or contextualized comparisons; and 4) causal assessment may take different forms, depending on the understanding or model of causal processes employed. Some of these contributions are specific to small-N studies, as is the case with discussions of Mill's methods. However, others apply equally to both qualitative and quantitative research and in some areas, such as the discussion of validity, the suggestions offered by qualitative researchers are arguably more useful than those proposed by quantitative methodologists. It is nothing short of puzzling that KKV should have provided guidelines for qualitative research that so manifestly ignore insights offered by qualitative methodologists.

Reflecting this more nuanced understanding of the state of quantitative and qualitative methodology, this article suggests a more open dialogue between quantitative and qualitative methodologists, that breaks with KKV's attempt to derive rules for qualitative research primarily by extending practices commonly used by quantitative researchers. There is no reason why basic regression models cannot be seen as

part of a larger arsenal of tools, which includes methods suited for political phenomena that exemplify different models of causality. Likewise, there is no overriding reason preventing the pooling of contributions made by quantitative and qualitative methodologists or the borrowing of insights generated by qualitative methodologists by quantitative researchers. Indeed, the point is to accept KKV's broad argument about the unity of the social sciences but reject the specific and unduly restrictive implications they derive from this argument. Otherwise, discussion of a number of ongoing, and as yet unresolved, methodological issues would be prematurely foreclosed, and many fruitful avenues for exchange between quantitative and qualitative researchers would be left unexplored.

A second and more complex problem with KKV's approach concerns how their emphasis on the unity of social science leads them to downplay real differences within the social sciences. KKV state that differences between the quantitative and qualitative traditions are "only stylistic" and "methodologically and substantively unimportant" (p. 4). But I have pointed to many instances in which these differences are substantial, giving rise to specific methodological strengths and weaknesses. For example, the fact that qualitative researchers tend to have a greater prior knowledge of the cases to be studied than do quantitative researchers was linked both with certain strengths, especially with regard to managing the problems of conceptual validity and measurement validity, and the problem of endogeneity; but also certain weaknesses, particularly concerning the reliability and replicability of their data and the danger of data "fitting." This perspective suggests a second and more substantively oriented basis for a dialogue between quantitative and qualitative researchers, one that seeks to bring the strengths of different methods to bear upon the same substantive question.

In evaluating the contribution of large-N, as opposed to small-N, studies, KKV basically echo earlier perspectives (Lijphart 1971, 685; Smelser 1973, 53–54) that take a hierarchical view in which large-N studies are ultimately seen as superior. However, this view is hard to reconcile with the pathbreaking contributions made by qualitative scholars (Stinchcombe 1978, 21–22; Walton 1992, 125–26, 129; Ragin 1992, 225; Collier 1998a, 1998b). Indeed, any attempt to sanction a division of labor is probably premature.[46] We simply have not had a sustained exchange between quantitative and qualitative scholars concerning the strengths and weaknesses of each research tradition, focusing carefully on what each method can contribute at each stage in the research cycle and on the potential complementarity of quantitative and qualitative research.[47] The likely payoffs of such a multimethod approach gives added urgency to the need for an open and sustained dialogue about quantitative and qualitative methods.

Notes

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1. Smelser (1968; 1973; 1976), Przeworski and Teune (1970), Sartori (1970), Lijphart (1971; 1975), Eckstein (1975).
2. Obviously, publications on comparative methodology did not vanish in the late 1970s to late 1980s. See, for example, Skocpol and Somers (1980), Skocpol (1984b), Sartori (1984), and Tilly (1984). This period, nonetheless, saw nothing similar to the current explosion of publications. Some of the most significant contributions to this methodological revival include: Ragin (1987; 1994; forth.), Ragin and Becker (1992), Sartori (1991), Geddes (1991; forth.), Collier and Mahon (1993), Collier and Mahoney (1996), Collier and Levitsky (1997), King, Keohane and Verba (1994), Janoski and Hicks (1994), Teflock and Belkin (1996), McDonald (1996), Mjøsct, Engelstad, Brochmann, Kalleberg, and Leira (1997), Bates, Greif, Levi, Rosenthal, and Weingast (1998), Mahoney (1999), Collier and Adcock (forth.), George and Bennett (forth.).
3. The effort of synthesis by Collier (1993) is one clear exception to this trend. See, also, Ragin, Berg-Schlosser, and de Meur (1996).
4. The interest generated by this book is well attested by the large audiences in several plenary sessions organized to debate the book at annual professional meetings, by the fact that the book has already been through numerous printings and that it ranked as the fastest selling book ever published by Princeton University Press, and by the widespread use of the book in graduate seminars across the country.
5. The better known responses to KKV's effort to extend insights from quantitative methodology to qualitative research have been provided by qualitative researchers (Caporaso 1995; Collier 1995a; Laitin 1995a; Rogowski 1995; Tarrow 1995; see also McKeown 1999). However, some of the most suggestive points concerning the manner in which KKV conceive of the quantitative-qualitative connection have been made by quantitative researchers. In this regard, the important but not widely circulated reviews by two prominent quantitative methodologists, Larry Bartels (1995) and Henry Brady (1995), are worthy of attention.
6. All references in this article which only provide page numbers refer to King, Keohane, and Verba (1994).
7. KKV do discuss, early on, the major components of the research process. However, the four components they highlight—the research question, the theory, the data, and the use of the data (p. 12–13)—are not used to structure systematically the entire book. Furthermore, KKV's book lacks anything like a concluding chapter that would pull together the various pieces of advice it offers in a coherent manner.
8. The italics in many citations have been omitted.
9. While KKV "reserve the commonly used *n* to refer only to the number of observations and not to the number of cases," as is the practice in most discussions of the comparative method, they do retain the distinction between cases and observation. Thus they use the term "cases" to refer to "units" or countries, and "observations" to refer to "the values of the variables for each unit" (p. 51–53, 117–18, 217–18).
10. As KKV stress, because the selection of cases in small-N studies is carried out in an intentional fashion, "we know in advance the values of at least some of the relevant variables" (p. 139). Thus, data collection is inextricably linked with the process of case selection. Placing data collection at a stage in the research process following the selection of cases, however, clearly highlights the need for qualitative researchers to collect data on the unknown variables in a manner that is independent from the data that influenced the selection of cases (p. 46, 144, 204–06).
11. For the practical measures that it would entail, see King (1995a; 1995b).
12. Even if one were to have a broad understanding of the goals of social research (Skocpol and Somers 1980; Ragin 1994, 3, chap. 2), it is hard to deny that explanation is one of the central aspirations of social researchers and that, consequently, these fundamental rules would be applicable.
13. For an extensive discussion of these issues, see the September 1995 issue of *PS: Political Science & Politics* and the Winter 1996 issue of *APSA-CP. Newsletter of the APSA Organized Section in Comparative Politics.*
14. On the dangers of "fitting" in qualitative research, see Laitin (1995b) and Collier (1995a, 462). Because theories have multiple implications, rule No. 22 does not mean that an analyst could not generate theory from a case and also test this theory with the same case. Indeed, as KKV argue (p. 218, 223–24), because "a single case . . . contains multiple observations," it is possible to generate new data from old cases. The significance of within-case analysis, it bears stressing, has previously been recognized in debates on qualitative methodology (Smelser 1976, 217–20; Campbell 1975).

15. Of course, the same point would apply to any effort to demonstrate the applicability of insights derived from qualitative methodology to the conduct of quantitative research.
16. Lijphart (1971, 687, 690), Warwick and Osherson (1973a, 9–10), Smelser (1976, 152–53), Ragin, Berg-Schlosser and Meur (1996, 760–62).
17. In response to Laitin's (1995a, 455–56) critique that they do not stress the role of concept formation enough in their book, KKV (1995, 476) state that their "book is about doing empirical research designed to evaluate theories . . . not about generating theories to evaluate." However, as Collier and Mahoney (1996, 75–87) demonstrate, the link between conceptual issues and theory testing is so close that it is dangerous to divorce these two concerns.
18. Another way of stating these challenges, using Donald Campbell's terminology, is to say that researchers must take explicit steps to ensure the external and internal validity of their theories. The criterion of unit homogeneity affects external validity and the possibility of generalization, while the criterion of conditional independence affects internal validity and the possibility of ascertaining that there is a causal link between explanatory and dependent variables in the observed cases (Cook and Campbell 1979, chap. 2).
19. Closer to home, KKV could be faulted for not drawing upon insights available in the standard quantitative literature. For example, Brady (1995, 15) notices KKV's failure to incorporate Campbell's suggestions for ensuring internal validity, the issue at stake in the assumption of conditional independence (Campbell and Stanley 1963; Cook and Campbell 1979). See also Mohr (1996, 113–26).
20. On the process whereby a researchers classifies what cases are a case of, see Smelser (1976, 167–74), Ragin and Becker (1992), and Ragin (1994, 63–66, 88–90, 93–98).
21. On the significance of context-sensitive analysis, see Ragin (1987, 23–24, 32–33, 48–49). See also Laitin (1995a, 456) and Collier (1995a, 465).
22. At the same time that Skocpol (1979, 40–42; 1994, 6) argues against the inclusion of certain cases, she shows how the appropriateness of scope statements can be questioned, when she provides a reasoned arguments for expanding the universe of cases of revolution beyond those Marxist theorists considered. On this issue and Skocpol's work in particular, see Collier and Mahoney (1996, 80–82).
23. As the two examples show, in some instances a concern with differentiating contexts goes hand in hand with efforts at periodization, while in others spatial contrasts figure more prominently.
24. It remains the case that the term "context" is frequently invoked but rarely defined by qualitative researchers and, as Smelser (1973, 56) argues, that researchers should strive to be explicit about their use of contextual variables. The need to be explicit when discussing context, moreover, is critical because though in this instance one could translate "context" as "other explanatory variables," the notion of context is also used in another important manner, which is methodologically relevant to the issue of data validity. This alternative usage is discussed below.
25. On efforts to ensure unit homogeneity, see also the discussion of "frames of comparison" and "contrast space" in Collier and Mahoney (1996, 66–69). These suggestions are still in need of much refinement. Nonetheless, they are certainly worth pursuing, especially given Bartels' (1995, 10–11) argument to the effect that this is a fundamental problem that quantitative methodologists have still not dealt with adequately, even though it might be possible to deal with causal heterogeneity with a complex regression model (Bartels 1996).
26. As Brady (1995, 14–16) argues, there is some confusion in KKV's discussion of conditional independence given that it fails to adequately distinguish between conditional independence and the more burdensome criterion of independence, that does not allow for feedback loops.
27. While KKV (p. 226–28) discuss process tracing in a favorable light, as a way to increase the number of observable implications of a theory (rule No. 12), they are critical of this use of the process tracing method, arguing that the identification of causal mechanisms is a task that comes logically after the definition of causality, and that without a prior sense of causal effect the search for causal mechanisms "quickly leads to infinite regress" (p. 86). If experimental research has proceeded in this manner, seeking to establish whether a causal relationship exists before inquiring about the underlying processes or causal mechanisms that explain how a causal factor generates outcomes (McGraw 1996, 772–74), it is unclear why the reverse path cannot be followed and why the establishment of *how* a cause operates is not proof of *whether* it operates. On the importance of studying causal mechanisms, see Hedström and Swedberg (1998), Zuckerman (1997, 283–84), Tilly (1997, 47–50), and Rueschemeyer and Stephens (1997, 62–63).
28. Indeed, it is such an approach that has led qualitative researchers to critique the findings of quantitative research in the modernization literature (Rustow 1968, 48) and the survey based political culture literature (Schmitter and Karl 1991, 83). A basic problem with this research is that it has focused on

correlations and has not adequately dealt with the problem of causal direction that underlies the issue of conditional independence.

29. Smelser (1973, 77–80; 1976, 217–18), Campbell (1975), George and McKeown (1985), Collier and Mahoney (1996, 70).
30. Much as Lijphart (1971, 685) does, KKV operate with the assumption that we would always be better off using a statistical method and that small-N research and the comparative method should only be used as a backup option, when a statistical method cannot be used.
31. KKV (p. 30–31) are careful to address the problem of cross-level inference that could emerge when testing theories with new information about the same problem but at different levels of analysis. However, this is a different problem which pertains to the validity of data rather than the broader problem of a study's basic concepts.
32. Collier and Mahon (1993) and, since the publication of KKV's book, Collier (1995b), Collier and Levitsky (1997), Collier and Adcock (forth.).
33. While KKV do briefly touch upon the need for theory reformulation, the rule that applies in that instance (rule No. 21) merely states what has to be done without offering practical suggestions as to how this should be done.
34. This rule has been among the most debated aspects of KKV's work. See Collier (1995a), Collier and Mahoney (1996), the exchange between Rogowski (1995) and King, Keohane, and Verba (1995, 477–79), and Dion (1998).
35. The logic behind KKV's advice to avoid selecting on both the explanatory and dependent variable (rule No. 14c) is much simpler. It is nonetheless an important rule because one of the characteristics of qualitative research and a source of strength with regards to other methodological issues—that researchers know their cases fairly well—makes the danger of knowing the value of both the explanatory and dependent variable quite real.
36. It bears emphasizing, as Collier and Mahoney (1996, 75–80) argue, that many studies that are seen to lack variance on the dependent variable actually do exhibit variance. Part of the reason for this misperception is the fact that analysts fail to see how the historical approach used by many qualitative researchers introduces variation, in that the study of cases over time naturally introduces variance on the dependent variable. KKV (p. 129) do not appear to appreciate the significance of the longitudinal dimension of much comparative research, as their discussion of Skocpol's work on revolution demonstrates.
37. An implication of KKV's refusal to consider studies with no-variance, and particularly studies in which cases are selected on a nonvarying dependent variable, as legitimate, is that they offer no advice on how to evaluate arguments about causal inference developed on the basis of such studies. As Collier and Mahoney (1996, 73–74) suggest, Mill's method of agreement and the logic of counterfactual reasoning are particularly relevant in the context of no-variance studies (see also Collier 1995a, 464; Ragin 1997, 32–34).
38. A number of early works on measurement equivalence, that draw on both the quantitative and qualitative traditions, include: Przeworski and Teune (1970), Zelditch (1971), and Warwick and Osherson (1973a, 14–28). See also Smelser (1976, 174–93). As O'Kane (1993, 175–80) shows, the problem of conceptual stretching also has implications for the validity of data.
39. Considering the study of labor politics and economic restructuring, Locke and Thelen (1995) argue that a researcher should not simply focus, say, on disputes over wages. Instead, a researcher should search for those points where conflicts emerge, which might very well vary from case to case. Thus, to ensure the equivalence of measurements one might have to focus on wages in one case, employment in another, working hours in yet another, and so on.
40. See also Skocpol and Somers (1980, 183–87), Skocpol (1984a, 378–79), Ragin (1987, 36–42), Collier (1993, 111–12).
41. Lieberson's (1991) critique of Mill's methods of difference and agreement is aimed more at the weaknesses of deterministic models of causation rather than at the use of these methods to assess deterministic causes.
42. Mahoney (1999) also stresses the importance to causal assessment in small-N research of various forms of within-case analysis (Campbell 1975; George and McKeown 1985; Abbott 1992; Sewell 1996; Bennett and George 1997).
43. While KKV (p. 199–206) discuss Mill in the context of case selection and the search for control variables, they give little sense of how Mill's methods can be used in causal assessment and what sorts of trade-offs are involved in using different versions of Mill's methods for small-N analysis.

44. While highlighting the challenge to conventional regression analysis presented by path dependency, it is important to acknowledge that many other models of causality also diverge from the one KKV use. Among these one should mention the notions of asymmetrical causation (Lieberson 1985, chap. 4), cumulative causation (Stinchcombe 1978, 61–70), multiple causation (Ragin 1987, 20, 24–25), and conjunctural causation (Ragin 1987, 20, 24–25). See also Zuckerman (1997).
45. In particular, the pioneering work of economists Brian Arthur (1988a; 1988b; 1989) and Paul David (1985; 1986; 1993) has helped to establish that the answer to important questions, such as standardization and industrial location, can only be addressed by giving attention to path dependency, emergent properties, evolution and adaptation, and endogenous change, a series of features highlighted by complexity theory but that are not reflected in the assumptions of conventional regression models.
46. One division of labor various authors have suggested would use small-N studies to generate theories and uncover causal mechanisms and large-N studies to test and establish the generalizability of theories. Lijphart (1971, 685–86, 692–93), Smelser (1973, 56–57), Rueschemeyer (1991), Ragin (1987, 44–45, 55; 1994, chap. 2), Collier and Mahoney (1996, 71, 73–74, 88), McKeown (1999: 172–74, 183–84). This view, however, is challenged by arguments that emphasize how small-N studies and within-case analysis could be used for both theory generation and theory testing. Campbell (1975), George and McKeown (1985), Rueschemeyer and Stephens (1997, 66–69).
47. For some suggestions, however, see Runkel and McGrath (1972, chap. 4), Ragin (1987, chap. 5), Tarrow (1995), Rueschemeyer and Stephens (1997), Collier (1998a), and Bennett and George (1998).

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