

*Review Articles*  
AFTER KKV  
The New Methodology of  
Qualitative Research

By JAMES MAHONEY\*

- Henry E. Brady and David Collier, eds. 2004. *Rethinking Social Inquiry: Diverse Tools, Shared Standards*. Lanham: Rowman and Littlefield, 362 pp.
- Alexander L. George and Andrew Bennett. 2005. *Case Studies and Theory Development in the Social Sciences*. Cambridge: MIT Press, 331 pp.
- John Gerring. 2007. *Case Study Research: Principles and Practices*. Cambridge: Cambridge University Press, 265 pp.
- Gary Goertz. 2006. *Social Science Concepts: A User's Guide*. Princeton: Princeton University Press, 296 pp.
- Charles Ragin. 2008. *Redesigning Social Inquiry: Fuzzy Sets and Beyond*. Chicago: University of Chicago Press, 225 pp.

*DESIGNING Social Inquiry: Scientific Inference in Qualitative Research*, by Gary King, Robert O. Keohane, and Sidney Verba, stands as one of the most widely read books in contemporary political science.<sup>1</sup> Nearly all leading political scientists are at least somewhat familiar with it, perhaps knowing the work as simply “KKV,” a label that acknowledges (though also possibly reifies) its prominent authors. No one can deny that *Designing Social Inquiry* has had a large impact in the discipline. It popularized many methodological terms and ideas—descriptive and causal inference, observable implications, unit homogeneity, selection bias, and mean causal effect. And its attempt to codify each step in research design—from formulating questions, to specifying testable theories, to choosing observations, to testing theories,

\*I would like to dedicate this article to David Collier, founder of the qualitative and multimethod research movement in contemporary political science. Three anonymous reviewers and Peter A. Hall provided helpful comments.

<sup>1</sup> King, Keohane, and Verba 1994, cited hereafter as KKV.

to reporting results—stimulated discussions about nearly all aspects of methodology. The book encouraged methodological self-consciousness in political science, and this legacy can only be seen as beneficial.

*Designing Social Inquiry*'s prominence was also an outgrowth of its authors' provocative central mission: KKV sought to improve qualitative research by using well-established norms drawn from quantitative research, in particular, ideas from regression analysis. Their tacit assumption was that "mainstream" quantitative research<sup>2</sup> employs superior methods and that qualitative research could benefit from adopting these methods to the extent possible. *Designing Social Inquiry* thus encouraged qualitative scholars to follow the principles of a particular quantitative template, one based on the norms of regression analysis. Many believe, however, that this aspect of the book has hindered progress in political science.

This dual legacy of KKV—beneficially increasing methodological awareness while controversially and perhaps unproductively promoting a singular quantitative approach—constitutes an important backdrop for the books under review. Henry E. Brady and David Collier's edited book, *Rethinking Social Inquiry: Diverse Tools, Shared Standards*, collects many of the essays that first underscored the contributions and pitfalls of the KKV argument, starting with review symposia from the mid-1990s.<sup>3</sup> Brady and Collier in collaboration with Jason Seawright develop their own major methodological statement, especially in two important chapters in the final part of the book. Charles C. Ragin's latest book, *Redesigning Social Inquiry: Fuzzy Sets and Beyond*, is similarly cast as an alternative to KKV's work; it is a collection of methodological essays on set-theoretic analysis that Ragin wrote after the publication of his *Fuzzy-Set Social Science*.<sup>4</sup> *Case Studies and Theory Development in the Social Sciences*, by Alexander L. George and Andrew Bennett, presents a comprehensive discussion of the methodology of case studies and raises its own serious concerns about KKV's argument, including concerns drawn from the perspective of the philosophy of science. Gary Goertz's *Social Science Concepts: A User's Guide* and John Gerring's *Case Study Research: Principles and Practices* engage KKV less directly,

<sup>2</sup> Following Brady, Collier, and Seawright, mainstream quantitative research can be defined as research based on regression analysis and related techniques. See their "Refocusing the Discussion of Methodology," in Brady and Collier, *Rethinking Social Inquiry*, 3.

<sup>3</sup> The Brady and Collier volume includes work (sometimes in revised form) that was previously published by Larry M. Bartels, Henry E. Brady, David Collier, James Mahoney, Timothy J. McKeown, Gerardo L. Munck, Charles C. Ragin, Ronald Rogowski, and Sidney Tarrow.

<sup>4</sup> Ragin 2000.

although they, too, propose basic alternatives to KKV's advice about descriptive inference and case-oriented methodology.

In no small part because of these contributions, it seems safe to say that the field of qualitative methodology has entered a post-KKV era. To be sure, *Designing Social Inquiry* is still an essential item on any graduate syllabus concerning qualitative methods and research design.<sup>5</sup> But leading qualitative, statistical, and experimental methodologists believe that its advice, as presented, is often simplistic, misleading, and inappropriate as a guide for designing social inquiry. This belief is grounded in a detailed critique of the specific claims made by KKV and supported by insights from both quantitative and qualitative methodology. Indeed, several telling critiques have come from statistical methodologists (for example, Henry E. Brady, Larry M. Bartels, and David A. Freedman) convinced that KKV overstate the strengths of quantitative research and undervalue the contributions of qualitative research.<sup>6</sup>

The field of methodology has thus benefited from KKV even as it has also moved beyond it. There is a new methodology of qualitative research. This new methodology, well represented by the books under review, encompasses KKV's helpful insights while avoiding their most obvious missteps. Qualitative researchers seeking advice about research design will do best to turn to the new methodology instead of (or in addition to) *Designing Social Inquiry*.

The new methodology covers a broad range of procedures for using concepts and comparisons to generate valid descriptive and causal inferences. The purpose of this article is to provide an overview of these various tools and to indicate how they are distinctive vis-à-vis regression analysis (though often consistent with other numerical approaches such as Boolean algebra and Bayesian analysis). It bears emphasizing at the beginning, however, that the field of qualitative methodology has not reached a point of full consensus. Disagreements exist, including among some of the authors considered here, over issues such as the relative importance and appropriate place of particular techniques.<sup>7</sup> In addition, while the field has many stable orientations

<sup>5</sup> I once tried to teach my core graduate seminar on qualitative methods at Northwestern without assigning KKV. The students let me know that it had to be on the syllabus next time!

<sup>6</sup> See Henry E. Brady, "Doing Good and Doing Better: How Far Does the Quantitative Template Get Us?" and Larry M. Bartels, "Some Unfulfilled Promises of Quantitative Imperialism," both in *Rethinking Social Inquiry*. George and Bennett (chap. 1) offer an extended discussion of the reception of *Designing Social Inquiry*. Freedman 2008 emphasizes the distinctive strengths of qualitative research. For an experimental critique of KKV, see Gerring's justification for case studies in his chapter 6 (this chapter is coauthored with Rose McDermott). See also Gerber and Green 2008.

<sup>7</sup> Moreover, the methods that I identify are not the only orientations that fall within the camp of "qualitative" methodology. Other branches of qualitative methodology, such as interpretive analysis, offer

and well-established tools, it is at the same time a rapidly developing area. Further refinements and extensions to the techniques described in these books will no doubt take place.

The discussion is organized as three sections that showcase different components of the new methodology: (1) process tracing and causal-process observations; (2) set-theoretic methods and logic; and (3) strategies for combining qualitative and quantitative research. Each section begins with a brief consideration of the KKV approach and its shortcomings and then discusses the new methodology.

### PROCESS TRACING AND CAUSAL-PROCESS OBSERVATIONS

Process tracing is perhaps the tool of causal inference that first comes to mind when one thinks of qualitative methodology in political science. Under various labels, the method has been discussed for decades.<sup>8</sup> The goal of recent writings is both to formally specify the distinctive kinds of leverage for causal inference provided by process tracing and to offer guidelines for using process tracing in substantive research.

### THE KKV APPROACH: SUMMARY AND CRITIQUE

KKV understand process tracing as the search for intervening variables that link an independent variable with a dependent variable. They view uncovering these intervening steps as part of the more fundamental goal of estimating the causal effect (if any) of an independent variable of interest. On the one hand, they advocate process tracing as potentially “very valuable” because it could “increase the number of theoretically relevant observations.” On the other hand, they suggest that process tracing is “unlikely to yield strong causal inference” and can only “promote descriptive generalizations and prepare the way for causal inference” (pp. 227–28). One problem with process tracing is that there are an infinite number of intervening causal steps between any independent and dependent variable and thus the “approach quickly leads to infinite regress” (p. 86). In addition, the authors argue that process tracing is generally unable to determine which of multiple potential intervening mechanisms truly link the independent variable with the dependent variable. Given these problems, their final conclusion is that process tracing is mainly a descriptive tool and an early step in causal analysis.

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their own critiques of KKV even as they may stand in some tension with the methods considered here. For recent essays on interpretive analysis, see Yanow and Schwartz-Shea 2006. See also Johnson 2006.

<sup>8</sup>Campbell 1975; George and McKeown 1985.

This view accords nicely with an overarching approach in which regression norms are the basis for understanding and evaluating qualitative methods. At the core of KKV's advice is the admonition to expand the N of qualitative studies in order to achieve a determinate research design. They believe that qualitative research often lacks a sufficient number of observations to estimate the effect of the independent variable(s) of interest. Process tracing would be helpful if it expanded the number of observations. Yet for KKV process tracing often simultaneously increases the number of variables and thus does not expand degrees of freedom. Indeed, insofar as process tracing introduces more new variables than new observations, it may be a liability for causal inference.

The new methodology, by contrast, links process tracing not to regression norms but to a distinctive qualitative approach to causal analysis. Collier, Brady, and Seawright's discussion of two kinds of observations is useful for highlighting this difference.<sup>9</sup> These authors use the label "data-set observation" (DSO) to refer to an "observation" in the normal statistical and KKV sense. A DSO is equivalent to a row in a rectangular data set—that is, the scores for a given case on all measured variables. In mainstream statistical research, adding DSOs is a standard method for increasing degrees of freedom. By contrast, a "causal-process observation" (CPO) is "an insight or piece of data that provides information about context, process, or mechanism, and that contributes distinctive leverage in causal inference" (p. 277). The information contained within a CPO reflects in-depth knowledge of one or more particular cases rather than data collected as part of a systematized array of variables. The leverage gained for causal inference from CPOs is correspondingly distinct from though not incompatible with DSOs: "A causal-process observation sometimes resembles a 'smoking gun' that confirms causal inference in qualitative research, and is frequently viewed as an indispensable supplement to correlation-based inference in quantitative research as well" (pp. 277–78).

Process tracing contributes to causal inference primarily through the discovery of CPOs. It is not a methodology whose strength derives mainly from DSOs. As Gerring observes, process tracing often generates noncomparable observations that cannot be assembled into a standardized rectangular data set but that are nonetheless extremely useful for causal inference. He concludes that "a large number of standardized

<sup>9</sup> David Collier, Henry E. Brady, and Jason Seawright, "Sources of Leverage in Causal Inference: Toward an Alternative View of Methodology," in *Rethinking Social Inquiry* (pp. 229–66); see also the definition of causal-process observation in the glossary written by Jason Seawright and David Collier (pp. 277–78).

observations are not always superior to a single noncomparable observation” (p. 183). Likewise, George and Bennett note that KKV err in their “treatment of process-tracing as simply another means of increasing the number of observable implications of a theory. In fact, process-tracing is fundamentally different from statistical analysis because it focuses on sequential processes within a particular historical case, not on correlations of data across cases” (p. 13).

#### TYPES OF CPOS FOR TESTING THEORY

In the quest to gain leverage for causal inference, different kinds of observations may be valuable. Whereas mainstream quantitative research is built around DSOs, qualitative researchers often rely primarily on CPOS. These CPOS can serve two fundamental research tasks (see Figure 1). First, they can be used to develop, elaborate, or specify more precisely a given theory or hypothesis. This use of CPOS for *theory development* is widely appreciated by both quantitative and qualitative scholars. For example, when quantitative researchers use detailed knowledge from cases to help with the specification of their statistical models, they are drawing on CPOS for the purpose of theory development. The same is true when qualitative researchers refine their theories on the basis of key observations gathered during field research or via other forms of intensive data collection.

My focus in this section, however, is on a second and less well understood use of CPOS, specifically, their role in *theory testing*. Building in particular on the books by Brady and Collier and by George and Bennett, I distinguish three types of theory-testing CPOS (see Table 1). These types are differentiated according to whether the key piece of information provided for theory testing concerns the independent variable, a mechanism, or an auxiliary outcome. The following discussion examines each type in turn.

#### INDEPENDENT VARIABLE CPOS

One type of causal-process observation—what can be called an *independent variable CPO*—provides information about the presence of an independent variable (or about the presence of a particular range of values on an independent variable). These observations are essential for testing theories in many domains of scientific research where the key issue is whether a cause occurred in the manner and/or at the time posited by the theory. Not uncommonly, the simple existence of the cause itself is more controversial than the presumed link between the cause and its outcome. A good illustration is found, interestingly enough, in

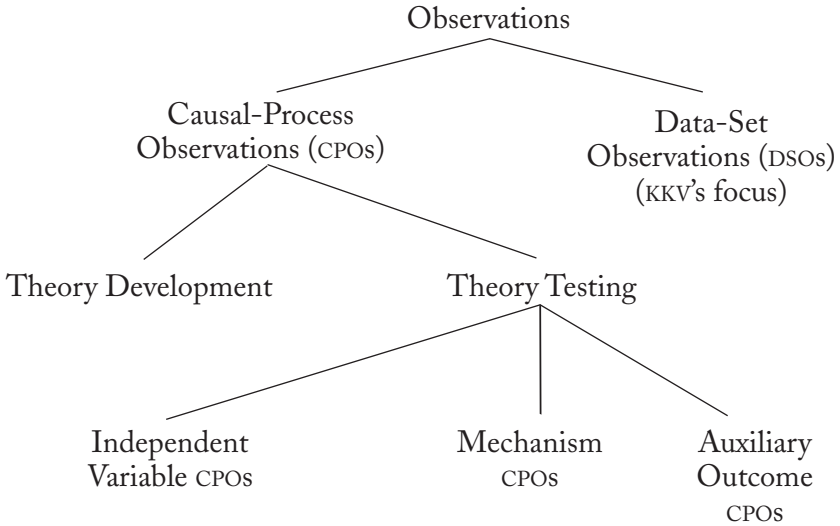


FIGURE 1  
TYPES OF OBSERVATIONS

KKV's discussion of the meteorite-collision theory of the extinction of dinosaurs (pp. 11–12).<sup>10</sup> As they point out, one “observable implication” of this theory concerns the presence of iridium in a particular layer of the earth's crust. The hypothesized iridium is not an intervening mechanism linking the meteorite collision to the dinosaur extinction. Rather, it is a piece of data that gives us confidence that, in fact, a meteorite collision did occur when it was hypothesized to have occurred.<sup>11</sup> A core challenge of testing the meteorite-collision theory involves making sure that the independent variable (a massive meteorite collision at the KT boundary) actually occurred at all. Confirmation of the existence of the independent variable increases substantially the theory's plausibility. Other famous examples where research has centered on finding pieces of data to confirm (or not) the very existence of a posited cause include Mendel's theory assuming alleles as a source of phenotype variation, the germ theory of disease, and the big bang theory of the creation of the universe.

<sup>10</sup> Waldner 2007 suggests that KKV's description of how this theory was tested is not correct. Brady, Collier, and Seawright 2006 also provide an alternative account, discussing the use of both CPOs and DSOs.

<sup>11</sup> The presence of iridium is an auxiliary outcome of the independent variable (that is, the meteorite collision). However, as discussed below, what I am calling “auxiliary outcome” CPOs are used with causal factors that are already known to exist.

TABLE 1  
TYPES OF CAUSAL-PROCESS OBSERVATIONS FOR THEORY TESTING

	<i>Type of Data Observed</i>	<i>Type of Theory Tested</i>
Independent Variable CPO	data concerning the existence of posited cause	theory proposes a cause whose existence is controversial
Mechanism CPO	data concerning posited intervening events and processes	theory offers expectations about intervening processes that should (or should not) occur if cause really affects outcome
Auxiliary Outcome CPO	data concerning posited auxiliary outcomes	theory offers expectations about outcomes besides the main one of interest that should be present if cause really affects outcome

The same issue also comes up in social science research. An example is Nina Tannenwald's work<sup>12</sup> on the nonuse of nuclear weapons, which was referenced by Collier, Brady, and Seawright and is now featured in their ongoing debate with Nathaniel Beck.<sup>13</sup> Tannenwald argues that the existence of a "nuclear taboo"—a normative prohibition against nuclear weapons that "has stigmatized nuclear weapons as unacceptable weapons of mass destruction"—is a cause of the nonuse of nuclear weapons by the United States since World War II.<sup>14</sup> Beck raises the concern that Tannenwald analyzes only four DSOS (that is, four historical episodes) and thus has only a tiny sample. He further suggests that her qualitative analysis of decision-making processes changes the research question to: "What did decision makers claim was important to them?" By contrast, Collier, Brady, and Seawright argue that the main leverage for causal inference in Tannenwald's study comes from CPOs (not her four DSOS) and that her analysis of foreign policy decision making is focused on the question of whether a nuclear taboo contributed to nuclear nonuse. Tannenwald's analysis calls attention to particular pieces of data (for example, specific conversations among high-level decision makers) which suggest that sustained discussion and even consideration of nuclear use was inhibited by prevailing norms. In evaluating her argument, the critical issue is precisely whether the nuclear taboo

<sup>12</sup> Tannenwald 1999.

<sup>13</sup> See Beck 2006a; Brady, Collier, and Seawright 2006; Beck 2006b; Collier, Brady, and Seawright 2007.

<sup>14</sup> Tannenwald 1999, 433.



actually exists; if it does, it seems quite plausible that it would affect decision making about nuclear weapons. In fact, given how the concept of nuclear taboo is defined by Tannenwald, its presence almost by definition shapes decision making concerning nuclear use. Tannenwald's study can thus be seen mainly as an effort to use independent variable CPOs in support of the idea that a nuclear taboo did in fact exist after World War II.<sup>15</sup>

#### MECHANISM CPOS

A second kind of causal-process observation—a *mechanism CPO*—provides information about whether an intervening event posited by a theory is present. It is not primarily by expanding the size of the N that these CPOs increase leverage. Instead, the leverage they provide derives from the ability of individual observations to confirm or challenge a researcher's prior expectations about what should occur.

George and Bennett (pp. 175, 219) argue that the analysis of mechanisms involves a logic that can be modeled as Bayesian, as opposed to the frequentist logic that underpins KKV's regression-oriented approach. As Bennett writes in a subsequent article, "What is important is not the number of pieces of evidence within a case that fit one explanation rather than another, but the likelihood of finding this evidence if the alternative explanations are true."<sup>16</sup> When engaging in process tracing to locate mechanisms, qualitative researchers in effect ask how much a given observation affects their prior theoretical beliefs. Most observations have little or no effect on prior beliefs. But occasionally, a new fact will dramatically support a theory (much like a smoking gun) or dramatically undermine a theory (much like an air-tight alibi).

At least reasonably clear prior expectations are needed for mechanism CPOs to carry such weight; without these "theoretical priors," the extent to which an intervening variable is "expected" cannot be assessed. Results are thus sensitive to initial beliefs. Nevertheless, with the right kind of evidence, "even researchers who start out with very different prior probabilities on the alternative explanations should converge toward shared expectations (a phenomenon known as the 'washing out

<sup>15</sup> It bears emphasizing that establishing the existence of a hypothesized causal factor (whether genes or a nuclear taboo) can be achieved through both CPOs and DSOS. Sometimes statistical tests using a large number of DSOS are the basis for answering, "Does the posited cause exist?" Other times, however, individual pieces of data not linked to a rectangular data set provide the key support for the existence of the posited cause. Combinations are also possible. The spirit of the new methodology is very much that both kinds of data can be essential to theory testing.

<sup>16</sup> Bennett 2006, 341. See also Bennett 2008; Goldstone 1997; and Van Evera 1997.

of priors’).<sup>17</sup> In general, as philosophers of science have long argued, bold predictions about unlikely intervening processes count more than safe predictions about processes that routinely occur and that could be explained by multiple theoretical frameworks (see George and Bennett, chap. 7). By the same token, the failure of a theory to make sense of important intervening processes that are easily explained by other frameworks counts substantially against that theory.

George and Bennett offer a lengthy appendix with many recent examples of social science studies that draw on mechanism CPOs to test theory. As a classic example—one with which readers should be familiar—consider Theda Skocpol’s *States and Social Revolutions*.<sup>18</sup> Skocpol formulates a structural theory that holds that ideologically motivated vanguard movements are not important causes of social revolutions. True, she says, ideologically motivated vanguard movements were present in all cases of social revolution analyzed in her book (and not in several cases of nonrevolution). Despite this association, she argues, the mechanisms through which these movements are postulated to produce social revolutions are not present. Theories that stress the importance of vanguard movements argue that they help trigger and support urban and/or rural lower-class revolts. Yet Skocpol shows through process tracing that vanguard movements in fact emerge on the political scene quite late, only *after* major revolts have occurred (due to prior structural conditions) and only once prerevolutionary states have already been destabilized. Thus it is that ideologically motivated movements take advantage of crises they did not create. Given this, Skocpol concludes quite reasonably that they are not critical causes of the social revolutions under consideration in her book.

#### AUXILIARY OUTCOME CPOS

Theories can also be tested with *auxiliary outcome CPOs*. These CPOs are information about particular occurrences that should occur alongside (or perhaps as a result of) the main outcome of interest if in fact that outcome were caused in the way stipulated by the theory under investigation. Auxiliary outcomes are *not* intervening variables connecting the cause to the outcome of interest. Nor do they provide information about the *existence* of the cause. Rather they are separate occurrences that should be generated if the theory works in the posited fashion. Normally, these occurrences will be closely related to the outcome of

<sup>17</sup> Bennett 2006, 710.

<sup>18</sup> Skocpol 1979.

interest and can be viewed as additional “traces” or “markers” left behind by the same causes that generated the outcome.<sup>19</sup>

To illustrate this idea, one can usefully draw on the metaphor of a criminal detective, a metaphor often invoked in discussions of process tracing.<sup>20</sup> Criminal detectives assume that causal processes may leave behind multiple traces, and while the discovery of a single trace may not in itself be conclusive, the simultaneous presence of several related ones may lend decisive support to a given theory. For example, the theory that the cause of death was the work of a murderer may be derived from multiple clues (signs of struggle, direction of entry wound, blood splatter, and suspicious suicide note). Prosecutors in turn argue that it would take a miracle to explain why this evidence is present if a given causal process were not at work. One can also think about the point in terms of probabilities. If the probability of each of five events being individually present is .25 at a given time, the chance of all five being simultaneously present is only about .001.

As an example familiar to political scientists, consider Gregory M. Luebbert’s famous book, *Liberalism, Fascism, or Social Democracy*.<sup>21</sup> Luebbert hypothesizes that an alliance between the socialist party and the middle peasantry (a “red-green” alliance) was a key cause of social democracy in interwar Europe. While he develops this claim in part through small-N comparative research and in part through mechanism CPOs, he builds the case by arguing as well that if a red-green alliance really did foster social democracy, it should have left behind other markers. These include a reluctance of socialists to challenge the distribution of wealth in the countryside; high levels of worker and union autonomy; and an inability of socialists to rely on the middle peasantry as viable electoral base. The presence of these auxiliary outcomes bolsters the idea that the red-green alliance had a dramatic impact on the shape of national politics. Given this overall large impact, we have more confidence that the alliance also shaped the structure of the national political-economic regime itself.

From the perspective of KKV, efforts such as those by Luebbert to explore auxiliary outcomes are to be applauded, since they expand the observable implications of the theory. Even with these observations, however, the total N of Luebbert’s study is still quite small. KKV’s work

<sup>19</sup> Unlike independent variable CPOs, auxiliary outcome CPOs are used in conjunction with a potential causal factor that is already known to exist; the auxiliary outcome CPOs help establish whether the factor exerts causal effects on closely related outcomes.

<sup>20</sup> For example, Van Evera 1997.

<sup>21</sup> Luebbert 1991.

thus leads to the conclusion that Luebbert has only weak leverage for theory testing. By contrast, the new methodology suggests that Luebbert's argument is built in part around the claim that the probability of several different outcomes all occurring simply because of chance is quite low. While no one event is exceedingly surprising in itself, Luebbert persuades the reader that finding all of them together is quite unlikely unless the red-green coalition was a leading causal force driving regime formation in these interwar social democratic cases.

To conclude this section, let me return to a few basic points. Process tracing is a method that uses particular observations from within specific cases. It is, fundamentally, a case-based methodology that can be applied successfully only with good knowledge of individual cases. When used for testing theory, process tracing can provide insight about the existence of causes, mechanisms, or auxiliary traces posited by the theory under consideration. It does so through an implicit Bayesian approach to inference in which singular pieces of data may carry great weight. Although process tracing differs sharply from mainstream quantitative analysis, its findings can nevertheless supplement those from quantitative research (and vice versa). Process tracing is thus best seen as a *complement* to other methods, including quantitative methods, not as a competitor to these methods.

### METHODS USING SET THEORY AND LOGIC

Set theory and logic—as opposed to regression-oriented analysis—underpin several cross-case methods in the new literature. These methods include approaches to causal inference associated with qualitative comparative analysis (QCA), such as fuzzy-set analysis. They also include methods of concept analysis, such as the classical and family resemblance approaches. With each of these techniques, causal or descriptive inferences are made using logical ideas about necessary, sufficient, and/or INUS conditions.<sup>22</sup> And Boolean operators such as the logical AND and the logical OR are employed for the purposes of aggregating conceptual attributes or assessing causal hypotheses. This section explores some of the major developments in this strand of methodology that have occurred over the last two decades.

<sup>22</sup> INUS conditions are parts of larger combinations of factors that are jointly sufficient for outcomes. Thus, while an INUS condition is itself neither necessary nor sufficient for an outcome, it is part of a larger combination of factors that is sufficient for an outcome. Mackie 1965, 246, originally defined an INUS condition as follows: “An *insufficient* but *necessary* part of a condition which is itself *unnecessary* but *sufficient* for the result.” For an argument that links INUS causes to both qualitative and statistical research, see Mahoney 2008.

## KKV'S OMISSIONS

KKV's work does not consider any qualitative tools associated with set theory and logic, even though leading methods of descriptive and causal inference often depend on these. Regarding descriptive inference, KKV's neglect of set-theoretic approaches is linked to their lack of any sustained discussion of concept formation. As Brady (p. 62) notes in a chapter in *Rethinking Social Inquiry*, KKV's focus is mainly on causal inference, with the result that "problems of theory construction, concepts, and measurement recede into the background." An adequate treatment of these problems would have required explicit engagement with set-theoretic methods. More generally, the advice KKV do offer about descriptive inference reflects the concerns of statistical analysis, not qualitative research. For example, KKV encourage researchers to avoid typologies and classifications except as temporary devices for collecting data. In contrast, as George and Bennett stress, qualitative methodologists carefully distinguish conceptual typologies from explanatory typologies, and they assign important roles to both theory development and testing.<sup>23</sup>

Regarding causal inference, KKV do not discuss the set-theoretic logic underpinning leading cross-case qualitative methods. For example, they include only a short subsection on Ragin's work on QCA. Their position is that QCA is fully compatible with their understanding of causality. They argue that Ragin's approach "contains no new features or theoretical requirements" (KKV, 87). Consequently, KKV find no reason to discuss QCA as a distinctive tool of causal inference.

Yet, as a wide spectrum of methodologists now recognize, techniques based in logic and set theory are quite distinct from KKV's regression-oriented approach. Leading statisticians distinguish between approaches that seek to estimate the average effect of particular independent variables (that is, effects of causes) from those that attempt to explain why specific cases have particular outcomes (that is, causes of effects).<sup>24</sup> Experiments and regression-oriented techniques—the models on which KKV draw—employ the effects-of-causes approach; they are designed to estimate the average effects of independent variables. But qualitative case studies often adopt a causes-of-effects approach; they seek to explain why cases have certain outcomes. Statistical methodologists in political science who recognize the difference assert that

<sup>23</sup> See also Collier, LaPorte, and Seawright 2008; Elman 2005.

<sup>24</sup> For example, Holland 1986; and Dawid 2000.

approaches grounded in an experimental template are neither intended nor well suited for the latter kind of causal analysis.<sup>25</sup> But because KKV focus entirely on the former, they end up leaving out a whole tradition of causal analysis—that is, the causes-of-effects tradition associated with qualitative analysis.

Qualitative methodologists have developed a host of new procedures for identifying and generalizing about the causes of outcomes in individual cases and sets of comparable cases. These procedures do not draw on KKV's discussion of causal inference because they do not seek to estimate the average effects of independent variables. Instead, they analyze logical types of causes, including necessary, sufficient, and INUS causes. And they rely on set-theoretic methods to identify these types of causes and to assess their relative importance.

Since this tradition of analysis may be quite foreign to scholars trained in mainstream statistical methods, two related points are worth making before proceeding. First, unless scholars are familiar with the latest works using set-theoretic analysis, they are not in any position to evaluate these methods. For instance, if the extent of one's knowledge is having read Ragin's *The Comparative Method*,<sup>26</sup> then one simply does not have a strong background in this strand of methodology as it is currently formulated. Second, some scholars who are trained in regression-oriented analysis may be inclined to react with skepticism to these methods. While this skepticism is in some respects useful, it can also lead scholars to dismiss the methods prematurely, before they thoroughly understand them. Here an analogy with mainstream statistical methods is useful. Statistical methodologists in political science understandably sometimes find it frustrating when their methods are criticized inappropriately by skeptics who lack real expertise. The same is true for scholars who specialize in set-theoretic methods; they usually have very good answers to skeptics who raise objections on the basis of limited knowledge.

#### METHODS OF CROSS-CASE ANALYSIS

More than twenty years ago Ragin's *Comparative Method* proposed Boolean algebra as a set of tools for extending the logic of case studies to comparative analysis.<sup>27</sup> Even though the book became a classic in the field of qualitative methodology, its reception was never unequivocally positive. Standard concerns were that Boolean algebra required

<sup>25</sup> Achen 2005; Brady 2008.

<sup>26</sup> Ragin 1987.

<sup>27</sup> Ragin 1987.

dichotomous measurement and deterministic causal relations.<sup>28</sup> Some qualitative scholars furthermore felt that Boolean algebra mimicked too much the DSO-driven approach of statistical analysis.<sup>29</sup> Critics suggested that Boolean truth tables were, in effect, rectangular data sets and thus that this methodology was really a variation of a method that depends primarily on DSOs. Insofar as they viewed CPOs—not DSOs—as the basis for causal inference in case study research, they were prone to react guardedly.

Subsequent work on qualitative comparative analysis (QCA) has pushed the discussion and debate forward in various ways. Ragin's *Fuzzy-Set Social Science* and the new book under review address head-on the concerns about dichotomous measurement and deterministic causation. With fuzzy-set analysis, all causal factors and outcomes are continuously coded from 0 to 1. Ragin makes it clear that fuzzy-set coding is preferable to dichotomous coding unless one is dealing with phenomena that are inherently binary in nature (p. 141). Two chapters in the new book are devoted exclusively to issues of measurement and calibration with continuous coding. When calibrating measures, researchers adjust them to conform to dependably known standards, often through an iterative process of refinement entailing movement back and forth between ideas and evidence. Calibration allows researchers to make precise comparisons such as “a lot versus a little” as opposed to simply “more versus less.” While not all will agree with Ragin's conclusion that fuzzy-set measures offer “the best of both worlds, namely, the precision that is prized by quantitative researchers and the use of substantive knowledge to calibrate measures that is central to qualitative research” (p. 82), no one can deny that the new QCA methods offer a sophisticated approach to continuous measurement.

Likewise, characterizations of QCA as inherently deterministic no longer make sense. The approach does remain centered on the analysis of necessary causes and combinations of INUS causes that are jointly sufficient for outcomes. However, probabilistic criteria are now used to analyze these causes. For example, researchers can assess whether a given cause is necessary at alternative benchmarks (for example, .95, .80, .67). They can similarly explore whether combinations of factors are jointly sufficient across different probabilistic benchmarks. Some will feel that the idea of a cause that is “90 percent necessary” waters down the concept of a necessary cause.<sup>30</sup> These kinds of causes are,

<sup>28</sup> Liebersohn 1991; Smith 1990.

<sup>29</sup> Markoff 1990; see also Seawright 2005.

<sup>30</sup> Waldner 2005.

after all, really INUS causes.<sup>31</sup> Yet information about probabilistically necessary and probabilistically sufficient causes can be extremely valuable. For instance, the finding that nondemocracy is probabilistically necessary for social revolutions and that a high level of economic development is probabilistically sufficient for democracy represents important and useful information. And as many methodologists—both qualitative and quantitative—have recognized, standard regression-oriented techniques are not designed to discover or test these causes.<sup>32</sup>

Recent innovations also include methods for assessing the relative importance of causes and causal combinations. Braumoeller and Goertz developed tools for measuring the extent to which (probabilistic or deterministic) necessary causes are “trivial.”<sup>33</sup> A trivial necessary cause is a necessary cause that is always present, irrespective of the presence/absence of the outcome. Goertz then extended these ideas to assess the relative importance of both necessary and sufficient causes (including when continuously coded).<sup>34</sup> Ragin’s new book offers similar techniques built around the ideas of “coverage” and “consistency” within a set-theory framework (chap. 3). Another new method uses sequences as the basis for logically assessing the relative importance of causes in historical explanations, including when  $N=1$ .<sup>35</sup> In addition, there is new work beginning to explore the ways in which regression-oriented findings can be retested using QCA techniques (and vice versa).<sup>36</sup> There are now textbooks covering “best practices” and more technical guidelines for using QCA methods and associated software.<sup>37</sup> Overall, QCA methods are increasingly sophisticated and increasingly immune from the earlier criticisms that methodologists raised about Boolean algebra.

Certainly, QCA methods may nevertheless still raise the concern that they derive leverage largely from DSOs (that is, rows in a rectangular data set). Many of the new techniques require at least a moderate number of cases (almost always more than just three or four); they are often “medium-N methods.” To avoid the danger of overly mechanical applications of QCA, Ragin has repeatedly urged that the methods not be used without extensive case knowledge. In his view, “Fuzzy-set qualitative comparative analysis is best understood as an exploratory/interpretive technique” (p. 190). His vision of QCA work is more than simply gath-

<sup>31</sup> Mahoney 2008.

<sup>32</sup> For example, Goertz and Starr 2003; and Clark, Gilligan, and Golder 2006.

<sup>33</sup> Braumoeller and Goertz 2000.

<sup>34</sup> Goertz 2006.

<sup>35</sup> Mahoney, Koivu, and Kimball 2009.

<sup>36</sup> Eliason and Stryker forthcoming.

<sup>37</sup> Rihoux and Ragin 2008; Schneider and Wagemann 2007.



ering DSOs as suggested by a preexisting theory, entering the information into a software package, and hitting a button that generates results. Instead, QCA is supposed to mediate the relationship between theory and evidence in a research design that features an ongoing exchange between explanatory propositions and facts about particular cases.

QCA methods may be used at different points in the analysis.<sup>38</sup> They can help the researcher initially develop hypotheses that are then explored further through case-oriented research. Here the results of QCA tests are viewed as preliminary findings that need to be supplemented with causal-process observations from case-oriented research. Alternatively, QCA techniques can serve as tools for summarizing and logically checking findings that have already been derived from case studies. In my own recent research on colonialism and development, for example, I test a general theory using evidence from fifteen in-depth case studies.<sup>39</sup> These case studies rely heavily on observations gathered via process tracing. Fuzzy-set analysis is then employed as a method of “checking” the findings and demonstrating that the argument holds when variables are numerically coded and propositions are examined for logical coherence.

In sum, QCA tools work well only within the context of a broader multimethod research design that includes case-study analysis. While this news might seem disheartening to those who seek a single, fool-proof cross-case methodology, it will have been anticipated by those who assume multiple sources of leverage are ordinarily needed for tackling questions in the social sciences.

#### CONCEPTUALIZATION AND CONCEPT ANALYSIS

Within the field of measurement, the new qualitative methodology focuses centrally on issues of conceptualization and concept analysis. Concepts underlie all variables and theories, including in quantitative research, and they need to be defined before any indicators or scaling methods can be used. But until recently, concepts have received scant attention from methodologists, with only a few notable exceptions, especially Giovanni Sartori and David Collier.<sup>40</sup>

Goertz's *Social Science Concepts* brings together many of the best insights from this emerging literature and adds new ideas in the form of guidelines for concept users. He addresses questions such as: What strategies are available for defining my key concepts? How should my definitional attributes be structured and aggregated? How do these at-

<sup>38</sup> Rihoux and Ragin 2008.

<sup>39</sup> Mahoney 2010.

<sup>40</sup> On the contribution of Sartori, see Collier and Gerring 2009.

tributes relate to my indicators? How do my decisions about conceptualization influence my findings about causal patterns?

Goertz distinguishes two approaches to conceptualization. The classical one associated with Giovanni Sartori structures concepts by treating definitional attributes as individually necessary and jointly sufficient for membership in a concept. Here attributes are connected with the logical AND. For example, if the concept of political democracy is defined by (1) free elections, (2) broad suffrage, *and* (3) extensive civil rights and liberties, then with the classical approach each of these attributes is necessary for political democracy and together they are sufficient. It bears emphasizing that the logical AND can be used with either dichotomous or fuzzy-set measurement. With dichotomous measurement, democracy is present only if all three defining attributes are present (that is, the absence of even one necessary condition precludes membership in the concept). With fuzzy-set measurement, by contrast, the degree of conceptual membership for a case corresponds to the *minimum* value of that case across the three defining attributes. For example, if a case's fuzzy-set scores (ranging from 0 to 1) across the three defining attributes are .75, .25, and .50, then that case receives a membership value of .25 for the overarching concept of political democracy, since this is its lowest (minimum) score. Obviously, this fuzzy-set approach requires (and assumes) the proper calibration of the continuum used for measurement.<sup>41</sup>

The "family resemblance" approach associated with Ludwig Wittgenstein structures concepts quite differently.<sup>42</sup> It assumes that there are no necessary conditions for membership in a concept (that is, no single trait is shared by all members). There are instead different combinations of attributes that are each sufficient for membership. To take an example used by Goertz, Alexander Hicks<sup>43</sup> defines the "welfare state" circa 1930 as the presence of any three of four classic welfare programs: (1) workman's compensation, (2) health insurance, (3) old-age pensions, and (4) unemployment compensation. No attribute is required, and there are in fact five different combinations of attributes that are each sufficient for membership in the concept. This approach to concepts is built around the logical OR, a point that can be illustrated with a simple Boolean equation.<sup>44</sup> When used with fuzzy-set measurement,

<sup>41</sup> Ragin 2000.

<sup>42</sup> Collier and Mahon 1993.

<sup>43</sup> Hicks 1999.

<sup>44</sup>  $Y = ABCD \vee ABC \vee ABD \vee ACD \vee BCD$ , where  $\vee$  stands for the logical OR,  $=$  stands for sufficiency,  $Y$  is welfare state,  $A$  is workman's compensation,  $B$  is health insurance,  $C$  is old-age pensions, and  $D$  is unemployment compensation.

the logical OR directs the researcher to a case's *maximum* value across attributes (or across combinations of attributes that are sufficient) as representing its degree of membership in the overarching concept. The aggregation procedure used with the family resemblance approach is, in this sense, the opposite of the classical approach, an outgrowth of the contrasting main logical operator (that is, AND versus OR) used to structure concepts in each approach.

Goertz also develops formally many of Sartori's insights about conceptual stretching and moving up and down the ladder of generality.<sup>45</sup> He shows, for instance, that Sartori's proposed trade-off between the intension of a concept (that is, number of defining attributes) and its extension (that is, number of empirical instances) assumes that the concept is structured using the classical approach (that is, each attribute is necessary and together they are sufficient).<sup>46</sup> By contrast, with the family resemblance approach, where there are no necessary conditions, the trade-off can work in the opposite way: extension may increase with intension.

This discussion may raise ideas unfamiliar to scholars who think about measurement issues as involving indicators and scaling and who use additive procedures and central tendencies (not logical operators) when coding cases. Indeed, within the field of measurement, qualitative work on concepts and quantitative work on indicators have been largely divorced from one another.<sup>47</sup> This in part reflects their different lineages: quantitative work on measurement grows out of psychology and education statistics, whereas the qualitative literature is rooted in philosophical logic. Given this difference, some communication and translation difficulties will inevitably arise, but there is no reason why methodologists cannot work to bridge these differences and achieve a better dialogue. Goertz's *Social Science Concepts*, which is written for both qualitative and quantitative scholars, is a major step forward in making this dialogue a reality.

### COMBINING QUALITATIVE AND QUANTITATIVE RESEARCH

Multimethod research that combines qualitative and quantitative analysis is increasingly common in contemporary political science. And

<sup>45</sup> Sartori 1970 actually refers to a ladder of abstraction. But I follow David Collier in referring to a ladder of generality.

<sup>46</sup> Collier and Levitsky's (1997) important work on "diminished subtypes" is an innovative extension of this classical framework.

<sup>47</sup> An exception is Adcock and Collier 2001.

procedures for linking findings from qualitative and quantitative research are now seen as an important part of methodology.<sup>48</sup> This section examines these linkages by briefly considering KKV's view and then turning attention to the latest techniques of multimethod research.

#### A SINGULAR QUANTITATIVE TEMPLATE VERSUS MULTIMETHOD RESEARCH

KKV characterize their book as, above all, an effort to link qualitative and quantitative research. As they write at the outset: "Our main goal is to connect the traditions of what are conventionally denoted 'quantitative' and 'qualitative' research by applying a unified logic of inference for both" (p. 3). Yet the "unified logic" to which they refer is derived almost entirely from regression-oriented analysis. As a result, their approach to connecting the two traditions entails encouraging qualitative researchers to follow norms that are practiced in regression research. At no point do KKV include a sustained discussion of how the distinctive research methods used in qualitative research might be incorporated into and improve quantitative research.

By contrast, the new methodology explores quite explicitly how different qualitative and quantitative tools can be used together in ways that preserve their respective strengths while overcoming their respective limitations. This literature suggests that qualitative work can help quantitative researchers with inevitable challenges concerning concepts and measurement, case homogeneity, and justifying key assumptions such as conditional independence. It likewise assumes that qualitative analysts can benefit from quantitative research when selecting cases, formulating hypotheses, and generalizing results.

#### USING QUALITATIVE RESEARCH TO SUPPLEMENT QUANTITATIVE FINDINGS

One strand of the new literature examines how qualitative analysis can supplement findings derived from quantitative analysis. Lieberman's recent work on "nested analysis" is an important effort in this regard.<sup>49</sup> Nested analysis, as proposed by Lieberman, involves starting with a preliminary regression analysis and then conducting a subsequent small-N analysis to assess the regression findings. The small-N analysis can be used for *model building* by drawing on case studies to improve the specification and measures of the regression model. Alternatively,

<sup>48</sup> See, for example, Symposium 2007.

<sup>49</sup> Lieberman 2005.

the small-N analysis can be used for *model testing* by employing process tracing and CPOs to retest regression findings. With model-building small-N analysis, Lieberman recommends selecting at least one case that is not well predicted by the preliminary regression model (that is, an “offline” case). Outlier cases are recommended because of their utility for exposing missing variables that can then be brought into a subsequent round of regression analysis. With model-testing small-N analysis, by contrast, he recommends cases that are well predicted by the statistical model (that is, “online” cases). The reason for preferring online cases is that they should follow the causal pattern in the regression analysis if the model is correctly specified.

Other ideas for using case studies as a supplement to large-N research are developed in Gerring’s *Case Study Research*. In one important chapter, coauthored with Jason Seawright, nine different techniques of case selection are identified: typical, diverse, extreme, deviant, influential, crucial, pathway, most-similar, and most-different cases. This impressive typology goes beyond previous discussions of types of case studies.<sup>50</sup> According to Gerring and Seawright, the techniques “show how case-selection procedures rest, at least implicitly, upon an analysis of a larger population of potential cases. The case(s) identified for intensive study is chosen from a population, and the reasons for this choice hinge upon the way in which it is situated within that population” (pp. 88–90). The authors discuss how each type of case can be used for theory testing and/or theory building, as well as the statistical technique needed for identifying it within a large population.

These methods will be especially appealing to scholars whose basic orientation to research is large N but who wish to use case studies for supplementary purposes. In one sense, in fact, both Lieberman and Gerring share with KKV a statistically oriented approach to social science. Lieberman ultimately sees small-N research as a means of improving regression findings; most of Gerring’s techniques of case selection depend on cross-case statistical methods. Unlike KKV, however, Lieberman and Gerring are convinced that case studies bring distinctive sources of leverage that are simply not found in quantitative research. This is why Lieberman emphasizes that “the strategy described here is quite distinct from the message outlined in King, Keohane, and Verba. Rather than advocating that there are ‘lessons’ for qualitative researchers that can be gleaned from the logic of statistical analysis . . . I show that there are specific benefits to be gained by deploying both analytical tools simul-

<sup>50</sup> For example, Eckstein 1975.

taneously, and I emphasize the benefits of distinct complementarities rather than advocating a single style of research."<sup>51</sup> Likewise, Gerring goes to lengths to highlight the distinctive contribution of case-study research, and he presents an extended discussion of the relative strengths and weaknesses of such research when compared with large-N research.

To be sure, some methodologists will not be fully satisfied with these multimethod procedures. With respect to Lieberman's nested approach, for example, Rohlfing argues that it is inappropriate to use the residuals of an unsatisfactory regression model as the basis for case selection in small-N research.<sup>52</sup> He reasons that if the model is unsatisfactory, one cannot be confident that a deviant case is not really a typical case (or vice versa). Likewise, when testing a regression model with small-N analysis, Rohlfing contends that one should not simply examine the variables already included in the regression; rather, one must also look for excluded variables and misspecified relationships. More generally, insofar as small-N researchers believe that assumptions such as conditional independence cannot be justified in a regression analysis, they will be skeptical of framing their research around findings from that analysis.

My own view, which is perhaps a minority position in contemporary political science, starts from the idea that case study research and statistical research are designed to do very different things. Case studies seek to tell us why particular outcomes happened in specific cases; statistical studies try to estimate the average effects of variables of interest. Both are important issues, and they are related to one another, but for some topics one cannot pursue them at the same time. When scholars engage in multimethod research, therefore, they often pursue primarily either the comprehensive explanation of specific outcomes or the estimation of the average effects of variables of interest. In the case of Lieberman's methods, the main goal is to estimate average effects and to use case studies in service of this larger end. In other research, however, the main goal is to explain specific cases and use statistical methods in service of that larger end. The next part considers this approach.

#### USING QUANTITATIVE RESEARCH TO SUPPLEMENT QUALITATIVE FINDINGS

The idea that quantitative analysis might supplement findings derived from an initial qualitative analysis is not new. Going back to Lijphart,<sup>53</sup>

<sup>51</sup> Lieberman 2005, 436.

<sup>52</sup> Lieberman 2005; Rohlfing 2008.

<sup>53</sup> Lijphart 1971.

scholars have proposed that case study and small-N results be evaluated with subsequent large-N statistical tests. The assumption is that the qualitative analysis *develops* a hypothesis or theory, whereas the quantitative analysis actually *tests* the hypothesis or theory.

The new methodological literature, however, does not assume this strict division of labor. Rather, it suggests that both theory development and theory testing may take place during each phase of a qualitative-quantitative iteration. An initial qualitative study might both propose a theory and use process tracing to test that theory. A subsequent statistical study might then further test that theory and stimulate new modifications to the theory. An example of this iteration is research on the democratic peace, which is discussed at length by George and Bennett. To test the hypothesis that democracies do not fight wars with one another, researchers have used both case studies and large-N statistical designs. The former studies have especially evaluated alternative hypotheses about the causal mechanisms underpinning the democratic peace, whereas the latter studies have explored whether the finding about the democratic peace still holds up when controlling for the effects of numerous other variables. Both kinds of designs also have contributed to theory generation: the original theory emerged in qualitative research dating back to Kant, but its current version, which emphasizes the absence of war among democratic dyads, has been strongly influenced by statistical work.

Nor does the new methodological literature assume that the primary way in which statistical analysis supplements qualitative research is through the generalization of small-N findings to a large number of cases. Rather, it emphasizes that statistical analysis may be used to test observable implications of an argument that inherently applies to only one case or to only a small number of cases. For example, in his well-known research on revolutions, Goldstone considers the hypothesis that changes in population caused increases in grain prices in eighteenth-century France, which in turn contributed to the massive peasant revolts at the end of the century.<sup>54</sup> As formulated, the hypothesis applies to a single case—that is, France. Yet the hypothesis proposes a connection between change in population and change in rural wages/prices that operates across many years. To test the hypothesis, then, Goldstone employs a time-series regression analysis of population change and wages/prices in France from 1650 to 1789. This large-N analysis supplements the overarching argument about a single case.

<sup>54</sup> Goldstone 1991, 187–89.

In short, process tracing in an individual case can lead a researcher to use regression analysis. The purpose of the regression analysis, however, is not necessarily to generalize a finding about the one case to a broad range of cases. Rather, its purpose may be to increase leverage for validly answering a question about that one specific case.

### CONCLUSION

The new methodological literature, as exemplified by the books under review, offers better advice for qualitative researchers than KKV's *Designing Social Inquiry*. That said, the main purpose of this article has not been to denigrate *Designing Social Inquiry*. Rather, it has sought to use KKV's missteps as a point of departure for discussing the positive contributions of recent work in qualitative methodology. By way of conclusion, I return to three areas of methodology examined above—causal-process observations, concepts, and multimethod research—and consider some research frontiers suggested by the new literature.

First, exciting research awaits those methodologists who explore the connections between different logical types of causes, on the one hand, and alternative uses of causal-process observations (CPOs), on the other. Cross-case qualitative methods ordinarily treat causes as logical types, for example, necessary causes, sufficient causes, and INUS causes. Yet the relationship between these alternative types of causes and different kinds of CPOs is not well understood. For example, if a given cause is treated as necessary for an outcome, as opposed to being an INUS cause of that outcome, what are the implications for process tracing? Is process tracing more straightforward for necessary causes than INUS causes? Along the same lines, the extent to which a CPO can indicate the presence of a particular logical type of cause itself merits further exploration. For example, is the identification of a “smoking gun” CPO related to the discovery of a sufficient cause? Is the discovery of a CPO that rules out a possible explanation related to the discovery of the absence of a necessary cause? Answers to questions like these will advance the methodology of process tracing and causal-process observations.

Second, new research beckons to be done on the implications of alternative approaches to concepts for causal inference. For example, if a scholar adopts the “family resemblance” approach to conceptualizing an outcome variable, such that there are different ways of constituting this outcome, will causal patterns be heterogeneous when explaining the outcome? The intuition here is that different causal factors may be needed to explain each of the specific ways of constituting the out-



come. Likewise, if a causal factor is conceptualized using the family resemblance approach, is it appropriate to assume homogeneity in the effects of this causal factor? Fuzzy-set measurement raises similar questions even when using the classical approach to conceptualization. For instance, even if two cases have the same fuzzy-set score for an outcome, they still may have different scores on the underlying constitutive dimensions that define this outcome. Qualitative researchers need to be open to the possibility that these differences on constitutive dimensions have consequences for causal analysis.

Finally, the basic question of whether and exactly how qualitative and quantitative approaches can be combined still needs to be resolved. This challenge involves not only practical problems but also philosophical ones.<sup>55</sup> What is the relationship between studies that seek to develop comprehensive explanations of outcomes in particular kinds of cases versus studies that seek to estimate the average effects of particular independent variables across a large number of cases? Can approaches grounded in set theory and logic be combined with approaches grounded in probability theory and statistics? What is the relationship between an INUS cause and a mean causal effect? Do variables that exert substantively significant mean causal effects in statistical models have their effects because they are actually probabilistically necessary and/or sufficient causes?

Many of these questions cannot be answered (or even fully comprehended and appreciated) unless scholars have a solid background in the new qualitative methodology. The books discussed in this article provide a basis for establishing that background.

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<sup>55</sup> For example, Mahoney 2008.

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