

QUALITATIVE RESEARCH: Recent Developments in Case Study Methods

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Key Words process tracing, selection bias, path dependence, typologies, fuzzy set

■ **Abstract** This article surveys the extensive new literature that has brought about a renaissance of qualitative methods in political science over the past decade. It reviews this literature's focus on causal mechanisms and its emphasis on process tracing, a key form of within-case analysis, and it discusses the ways in which case-selection criteria in qualitative research differ from those in statistical research. Next, the article assesses how process tracing and typological theorizing help address forms of complexity, such as path dependence and interaction effects. The article then addresses the method of fuzzy-set analysis. The article concludes with a call for greater attention to means of combining alternative methodological approaches in research projects.

INTRODUCTION

Over the past decade, the field of political science has witnessed a renaissance in qualitative methods. From roughly the mid-1970s through the mid-1990s, courses on qualitative research methods in political science typically incorporated the same standard readings. These included classic works by Eckstein (1975), George (1979), Lijphart (1971), and Przeworski & Teune (1970), with some notable newer contributions by George & McKeown (1985), Ragin (1987), and Collier (1993). Since the mid-1990s, however, a surge of new scholarship has built on this earlier literature, resulting in a new qualitative methods canon. For example, most of the readings on the 2006 Arizona State University Institute on Qualitative Research Methods syllabus were either published in the past few years or were works in progress (see <http://www.asu.edu/clas/polisci/cqrm> for the most recent syllabus). Significant recent writings on qualitative methods include books by Abbott (2001), Brady & Collier (2004), Elman & Elman (2001, 2003), Geddes (2003), George & Bennett (2005), Gerring (2001), Goertz (2006), Goertz & Starr

(2002), Mahoney & Rueschemeyer (2003), Pierson (2004), Ragin (2000), and Tetlock & Belkin (1996). Several articles by these and other authors in the past decade have broadened and deepened the discussion of qualitative methods as well.

The renaissance in qualitative methods has been influenced by many sources, including developments in the philosophy of social science, the training of a new generation of graduate students in multiple methods, the catalyzing effect of the controversy over King, Keohane, and Verba's *Designing Social Inquiry* (King et al. 1994), the creation of a new Qualitative Methods section of the American Political Science Association, and the annual institutes organized since 2001 by the Consortium on Qualitative Research Methods.

This article focuses not on the causes of this revival but on some of its substantive contributions to social science methodology. Although it is impossible to do justice to the prodigious and quickly growing body of new literature on qualitative research in a single article, we discuss some of its key developments and novel contributions in the context of case study research. We review the increasing focus on causal mechanisms as the basis of explanation and its relationship to qualitative methods. We then turn to the refinement of process tracing, a key form of within-case analysis that is now on an equal basis with the method of cross-case comparison favored by earlier literature on qualitative methods. We next discuss the new view of case-selection criteria in qualitative research methods and the ways in which that view takes issue with standard critiques rooted in quantitative approaches. We discuss as well the new literature's emphasis on path dependence, which relates to renewed interest in causal complexity and to the method of process tracing. This leads to a discussion of typological theorizing as a new approach to systematic comparison and case selection and as a means of iterating in a structured way between within-case analysis and cross-case comparison. We then discuss fuzzy-set analysis and its relationship to more traditional means of qualitative research. We conclude with a discussion of new understandings of the comparative advantages of qualitative methods and a call for greater attention to means of combining alternative methodological approaches into research programs and individual research projects.

EPISTEMOLOGICAL ISSUES: EFFECTS OF CAUSES VERSUS CAUSES OF EFFECTS

Contemporary mainstream qualitative methodologists continue to debate and refine the ontological and epistemological commitments that inform their methodological choices. The more recent contributions are notable for the confidence with which they advance different justifications for using qualitative methods, compared with those suggested by quantitatively oriented methodologists.

With respect to ontology, scholars have beliefs about what the social world is made of and how it operates, and these beliefs influence their choices about

how to construct and verify knowledge statements about that world.¹ Qualitative methodologists tend to believe that the social world is complex, characterized by path dependence, tipping points, interaction effects, strategic interaction, two-directional causality or feedback loops, and equifinality (many different paths to the same outcome) or multifinality (many different outcomes from the same value of an independent variable, depending on context). The possible presence of these complexities affects how knowledge statements can be most usefully constructed and verified (Hall 2003).

A key question for methodologists is how best to draw causal inferences, which in turn depends on the understanding of causality invoked. Brady (2003), in an outstanding summary of a large and diverse literature, suggests that there are at least four approaches to causality (see also Mahoney 1999). The neo-Humean regularity approach establishes causation through constant conjunction and correlation. The counterfactual approach looks to compare similar worlds and asks whether differences between them can be attributed to a change in a particular cause. The manipulation account investigates the effects of manipulating a cause in a controlled setting. Finally, causation can be thought of as a process involving the mechanisms and capacities that lead from a cause to an effect. Brady (2003) observes that these different approaches to causation have affinities with different methodologies. Notably, neo-Humean regularity theory lends itself to large-*n* regression analysis; experimental research designs are consistent with the counterfactual and manipulation theories; and case studies have a relative advantage in the search for mechanisms and capacities.

Small-*n* qualitative methodologists in political science thus tend to marry a complex view of the social world with a “mechanisms and capacities” approach to causation. This nexus of commitments results in a coherent and distinct set of methodological choices, nicely captured by the notion of causes-of-effects, as distinct from effects-of-causes (Brady 2003, Goertz & Mahoney 2006).² The conventional quantitative view takes an effects-of-causes approach. Users of quantitative methods commonly direct their investigations to inferring systematically how much a cause contributes on average to an outcome within a given population. Scholars holding this view would find little to argue with in Abell’s (2004, p. 293)

¹Our view is that social scientists should begin with ontology before proceeding to epistemology and methodology. We recognize, however, that the relationship is likely to be more complex, because epistemological and methodological choices make it much more likely that scholars will “see” the social world in a particular way [see Pierson (2004, pp. 9–10), citing Jepperson (unpublished manuscript)].

²See also Ragin’s (1987) discussion of variable-oriented and case-oriented research. Although the causes-of-effects and effects-of-causes distinction has only recently received increased attention, qualitative methodologists are aware that it is not new. For example, as long ago as 1850 Mill (reprinted 1974, p. 388) observed in his discussion “Of the Four Methods of Experimental Inquiry” that “inquiries into the laws of phenomena . . . may be either inquiries into the cause of a given effect, or into the effects or properties of a given cause.”

summary of Lieberson's (1992) criticism of small-*n* studies: "We cannot . . . speak of causal explanations until we have located a secure generalization by comparing cases . . . and protected our conclusions against any chance or spurious associations." The same template animates King, Keohane, and Verba's well-known quantitative gloss on qualitative methods, which gives priority to identifying causal effects rather than causal mechanisms (King et al. 1994, p. 86).

Mainstream qualitative methodologists, by contrast, fit more comfortably into a causes-of-effects template, in particular, explaining the net outcome of a particular case or a few cases. They do not look for the net effect of a cause over a large number of cases, but rather how causes interact in the context of a particular case or a few cases to produce an outcome. As noted above, the causes-of-effects approach is the product of mutually reinforcing ontological and epistemological commitments. For example, in a complex social world, cases are at some level unique and causal stories are likely to be knowable only after the fact.

Although various versions of the causes-of-effects approach, albeit often implicit, underpin a great deal of research in political science, we acknowledge that it is not a universally held view of qualitative methods. For example, as noted above, King, Keohane, and Verba hold much more closely to the effects-of-causes template (King et al. 1994). This disagreement helps explain some of the "from Mars and from Venus" moments in the methods literature. As Rogowski (2004) has argued, King, Keohane, and Verba are at a loss to explain how it is possible that so much qualitative research, some of which they acknowledge has been both good and influential, ignores proscriptions that are so obviously and easily applicable in the quantitative view. We argue that the reason for the lacuna is straightforward: Most mainstream qualitative methodologists have different ontological and epistemological commitments, and these produce a discrete set of methodological injunctions. Although sometimes these rules overlap with quantitative methods, quite often they do not. For example, as we argue below, to the extent that qualitative studies rely on within-case analysis, case-selection criteria that apply to quantitative studies will not be applicable (see also Collier et al. 2004).

Scholars who use qualitative methods with the causes-of-effects template do not do so because they do not know any better or because they are "journalists without a deadline." Nor is the causes-of-effects approach a second-best strategy to be followed when circumstances do not allow the use of quantitative methods. Even when there are enough observations to allow statistical analysis, conducting in-depth case studies can still offer separate inferential advantages. Some of these advantages are highly complementary to quantitative approaches. For example, case studies can help determine whether correlations in statistical analyses are spurious or subject to problems of endogeneity. In short, qualitative methodologists have developed distinct justifications for a discrete set of methods that are capable of producing verifiable, and in some instances, generalizable scientific explanations of the social world.

PROCESS TRACING ON THE OBSERVABLE IMPLICATIONS OF HYPOTHESIZED CAUSAL PROCESSES

As noted above, in mainstream qualitative methods, within-case methods are largely aimed at the discovery and validation of causal mechanisms. George & Bennett's (2005) process tracing and Collier et al.'s (2004, pp. 252–55) causal process observations allow inferences about causal mechanisms within the confines of a single case or a few cases. Causation is not established through small-*n* comparison alone [what Collier et al. (2004, pp. 94–95) call intuitive regression] but through uncovering traces of a hypothesized causal mechanism within the context of a historical case or cases.

Because they do not rely on establishing causation through comparison, these within-case research methods do not suffer from some of the pathologies associated with various quantitative research designs. For example, George & Bennett (2005, pp. 28–29) argue that case studies are often mistakenly criticized for having a “degrees of freedom” problem, when in fact within-case methods may provide evidence that bears on multiple testable implications of a theory within a single case (see also Campbell 1975). There may or may not be a sharp underdetermination problem in any particular case study, but whether a researcher can exclude all but one of the alternative explanations for a case depends on how the accessible evidence matches up with the proposed alternative explanations, not how many independent variables are considered or how many within-case observations are made. What is indeterminate for an effects-of-causes quantitative design does not necessarily map onto a causes-of-effects process tracing research design. A single “smoking gun” piece of evidence may strongly validate one explanation and rule out many others. Similarly, numerous within-case observations may fail to identify which of two incommensurable explanations is more accurate if there is no evidence on key steps in the hypothesized processes on which they differ.

Mainstream or third-generation qualitative methodologists are optimistic about the probative value of studying a single or few cases. The more of the following elements that are present, the more persuasive the results of such within-case analyses are likely to be, all other things being equal.

First, an account that runs from a suitably chosen beginning to the end of the story is likely to be more persuasive than one that starts or ends at an odd or unconvincing moment. For example, an account of the origins of the Second World War that began with Germany's invasion of Poland would, however well it scored on the other dimensions, very likely be considered incomplete. This need not imply an infinite regress into the recesses of history, however; indeed, to the extent that an explanation pushes too far back in time across an increasing number of potential critical junctures, it risks becoming unpersuasive about why one particular outcome emerged.

Second, process tracing accounts that have fewer (and preferably no) noteworthy breaks in the causal story are to be favored over those that have many. As George &

Bennett (2005, p. 30) suggest, it is the “insistence on providing [a] continuous and theoretically based historical explanation of a case, in which each significant step toward the outcome is explained by reference to a theory, that makes process tracing a powerful method of inference.”

Third, each process tracing account suggests evidence that should be found if the account is true. Some evidence will be probative for many alternative explanations, and some evidence will be germane for only one explanation. A process tracing account will be stronger if key nonsubstitutable links in the hypothesized process are supported by the evidence. Although we do not expand on this argument here, this is analogous to Bayesian rationales for diversity of evidence as an important check on causal inference. More generally, process tracing can be construed as following Bayesian logic, rather than the frequentist reasoning that undergirds regression analysis.

Fourth, and related, our confidence in the suggested explanation will be increased if process tracing finds evidence of observable implications that are inconsistent with alternative explanations. If only one theory proves correct at a temporal or spatial juncture in a case that alternative theories agree is important and if the alternatives are all wrong, our confidence is greatly increased in the one theory that proved correct on the point in question. Indeed, it is possible that we can increase our confidence in the existence of the hypothesized mechanism even without plentiful evidence of its operation if key steps in all the alternative feasible explanations are proven untrue. As the fictional character Sherlock Holmes famously argued, “eliminate all other factors, and the one which remains must be the truth” (Doyle 1930, p. 93). Here again, process tracing can be characterized as following Bayesian logic.

Finally, process tracing is more persuasive to the extent that the researcher has guarded against confirmation bias. It is important in this respect to look within a case for the observable implications of a wide range of alternative explanations, to give these explanations a “fair shake” vis-à-vis the evidence, and to develop sufficiently diverse, detailed, and probative evidence to elevate one explanation (which may derive from a single theory or a combination of theories) over all others. Books by Evangelista (1999), Khong (1992), and Sagan (1993), among many others, do an excellent job of process tracing by these standards.

CASE-SELECTION CRITERIA AND THE ISSUE OF SELECTION BIAS

Generations of would-be assistant professors have risked having their small-*n* job talks brought to a sudden stop by some version of the question, “Is it a problem that you have selected cases on the basis of the value of your theory’s dependent variable(s)?” Authors of small-*n* studies appear vulnerable to the selection critique because they often investigate cases where an outcome is known to have occurred or almost occurred (George & Bennett 2005, p. 23). One reason

for this focus, as Collier et al. (2004, p. 87) have noted, is that such cases provide “a better opportunity to gain detailed knowledge of the phenomenon under investigation.”

Although acknowledged as a commonly used strategy, selecting on the dependent variable has been widely critiqued by quantitatively oriented methodologists as a research design strategy that leads to “wrong answers” (Geddes 2003, p. 87). One detractor goes so far as to suggest that, thanks to such helpful advice, it is a norm that has begun to change (Geddes 2003, p. 87). The most common form of the critique, popularized by King, Keohane, and Verba among others, is that the selection of cases on the basis of values of the dependent variable leads to an underestimation of the effects of the independent variable (King et al. 1994; see also Geddes 1990). Truncating case selection on the dependent variable leads to a flatter regression line than would be produced if cases were selected from among the whole population (George & Bennett 2005, p. 23; see also Collier & Mahoney 1996, pp. 59–63).

These case-selection issues have recently received a great deal of attention from qualitative methodologists (see Collier & Mahoney 1996; Collier et al. 2004, pp. 85–102; George & Bennett 2005). We agree with scholars who note that the truncation critique applies to comparative small-*n* studies that rely on intuitive regression (Collier et al. 2004, p. 94). We also concur that small-*n* studies are susceptible to additional case-selection problems that are potentially worse than those identified by the truncation critique and can lead to more damaging consequences than just a flattening of the regression line. Indeed, if a small number of cases are badly selected, and if a researcher overgeneralizes from his or her findings, the researcher may overstate the relationship among the variables and even get the sign of this relationship wrong (Collier & Mahoney 1996, George & Bennett 2005).

However, we join these same methodologists in noting that conventional arguments on selection bias are often misapplied to qualitative case studies. First, the truncation critique assumes a preconstituted population to which the purported causal relationship applies. If the author is not working with such a population, then “addressing the question of selection bias before establishing an appropriate population puts the cart before the horse” (Collier et al. 2004, p. 88).

Second, the selection bias critique does not apply in the same way to inferences drawn from within-case process tracing or causal process observations.³ As George & Bennett (2005, p. 207) note, process tracing is fundamentally different from methods that rely on covariation. The method’s contribution to causal inference arises from its evidence that a process connects the cause and the outcome.⁴ Because the method does not rely on intuitive regression, it is not susceptible to selection bias (Collier et al. 2004, p. 96). Moreover, for the same reason, two sets

³On process tracing or causal process observations as a distinct form of drawing causal inferences, see George & Bennett (2005, pp. 205–32) and Collier et al. (2004, pp. 252–55).

⁴Strictly speaking, the process itself is not directly observable. The process and its associated mechanisms are believed to exist because of the observable implications of their operations.

of causal process observations derived from cases that were initially identified by their values on the dependent variable may be compared without bias (Collier et al. 2004, p. 97).⁵ The process-tracing qualification is a much more significant exception than one might think, because although many small-*n* studies are posed as comparisons on independent and dependent variables, they actually draw many of their causal inferences from process tracing.

Third, the critique does not apply where cases are selected on the dependent variable in order to test claims of necessity and/or sufficiency. For example, it is appropriate to study a case in which the outcome is known if the purpose is to determine whether a purported necessary cause is operating [Dion (1998); on necessary conditions, see Braumoeller & Goertz (2000) and Goertz & Starr (2002)].

Finally, small-*n* researchers often use a kind of Bayesian logic to select cases on the basis of the inferential leverage they hope to gain from prior expectations about the likelihood or unlikelihood of the outcome occurring. For example, the “Least Likely” case study relies on what Levy (2002, p. 144) has labeled the Sinatra inference: If the theory can make it here, it can make it anywhere. Accordingly, a case selected for study because it has a positive outcome on the dependent variable may provide strong inferences about the validity of the theory. Similarly, Rogowski (2004, p. 82) notes the central importance of a deviant case in which the outcome is unexpectedly wrong: “A powerful, deductive, internally consistent theory can be seriously undermined . . . by even one wildly discordant observation.” George & Bennett (2005, pp. 114–15) suggest that such deviant cases may also yield information about previously unidentified causal mechanisms that may also operate in other cases (see also Brady & Collier 2004, p. 285).

We should be careful, however, not to claim too much for the study of deviant cases. For example, they will be less helpful if their deviance is the result of measurement error or the combined effects of many weak variables. Even when a clear cause can be identified, the reason for the deviance may not be generalizable to a broader population. It is impossible for a researcher to know a priori whether any new explanations or variables that he or she uncovers will be relevant only for the outlying case itself. One cannot ascertain the generalizability of a hypothesized causal mechanism, in other words, until one knows something about the mechanism itself and hence about its potential scope conditions. For example, Charles Darwin’s study of a few species on the Galapagos Islands resulted in a theory of evolution relevant to all species.

In sum, qualitative methodologists do not entirely dismiss the selection bias critique, but they do argue that it is often overstated and that there are other reasons for choosing cases on the basis of outcomes that may well outweigh the risks of that particular bias. In addition, qualitative methodologists are aware of other forms of selection bias that are at least as common as truncation on the dependent variable.

⁵Collier and Mahoney have changed their view on this question over time (contrast Collier & Mahoney 1996, p. 70).

For example, cases are very often selected because of their historical importance or because they have accessible evidence (George & Bennett 2005, p. 25). Such cases may or may not be good ones to select for the purposes of testing, uncovering, or refining the hypothesized causal mechanisms and scope conditions of theories.

Qualitative methodologists have recently reinvigorated discussion of a case-selection issue relevant to both statistical and qualitative research: namely, the question of how to define and select negative cases of a phenomenon, or contexts in which the outcome of interest was possible but did not occur (Mahoney & Goertz 2004, p. 656). The inclusion of irrelevant or impossible cases in a statistical study can make a false or weak theory appear stronger than it actually is. Mahoney & Goertz suggest a “possibility principle” for distinguishing between cases where an outcome is merely absent and irrelevant cases where an outcome was impossible. This principle involves a “rule of inclusion,” by which a case is considered relevant “if at least one independent variable of the theory under investigation predicts its occurrence,” and a “rule of exclusion,” by which cases are considered irrelevant and excluded from tests of a theory if one variable is at a level known from previous studies to make the outcome of interest impossible. Mahoney & Goertz (2004, p. 659) maintain that when the two rules conflict, the rule of exclusion should dominate, although they note that the application of these rules is in part theory-dependent and should not be undertaken too literally or mechanically. They illustrate their approach through a reanalysis of Skocpol’s *States and Social Revolutions*, arguing that Skocpol’s (1979) case selection was implicitly based on the possibility principle and hence is more defensible than some of her critics have suggested.

PATH DEPENDENCE

Incorporating time into causal accounts allows theorists to more accurately explain a complex social world (A. Bennett & C. Elman, unpublished manuscript). Political scientists, most notably Pierson (2000, 2003, 2004), Thelen (1999, 2003), Mahoney (2000, 2006), and Mahoney & Schensul (2006), have recently paid increasing attention to what Pierson (2004, p. 4) describes as “the temporal dimension of social processes.”⁶ This dimension can raise tricky methodological problems, some of which are well addressed (and perhaps only addressable) using small-*n* methods.

One prominent example from this temporal dimension is the study of path dependence. As Mahoney & Schensul (2006) note, path dependence is a much-debated process; there is strong disagreement about the required (and for that matter

⁶Like many of our discipline’s methodological developments, path dependence originated outside political science. It has, however, received its own interpretations in political science, most notably in generating a literature focusing on the path-dependent features of political institutions.

desirable) elements to be included in its definition and in its effects. With that caveat in mind, a conventional definition of path-dependent processes is that the social world often follows a particular trajectory: an open period during which there are a number of plausible alternatives, a critical juncture where contingent events result in one of these alternatives being selected, and then feedback that constrains actors to keep to that particular path. Path dependence has several significant consequences. As Pierson (2004, p. 18–19) notes:

[S]ocial scientists generally invoke the notion of path dependence to support a few key claims: specific patterns of timing and sequence matter; starting from similar conditions a range of social outcomes is often possible; large consequences may result from relatively “small” or contingent events; particular courses of action, once introduced, can be virtually impossible to reverse; and, consequently, political development is often punctuated by critical moments or junctures that shape the basic contours of social life.

A common characteristic of path dependence is that constraints increase as time goes by. After contingent events have selected a particular path, actors are constrained to remain on that path or forced to pay a higher price to leave it by a variety of mechanisms that make it more plausible or attractive than other alternatives. Hence, it often follows that perturbing causes will have bigger effects the sooner they happen. The further into a constrained period, *ceteris paribus*, a larger change is needed to move actors off that particular path (Pierson 2004, p. 19; see also Page 2006, p. 91). Second, and related, explanations in the open and contingent time periods will differ from explanations in the period when actors are strongly constrained. Often, the critical juncture period is explicable in terms of agent-centered theories, whereas the equilibrium period is more amenable to structural explanations. Thus, theorists often differentiate between accounts of how institutions are created and those of how they are sustained (Mahoney 2000, pp. 511–12).

Studies of the stable pathway frequently focus on the mechanisms [which are often systemic in the sense used by Jervis (1997, p. 6)] that keep actors and institutions moving along the single track. There is some disagreement, however, about what form these constraints take. Pierson (2004, pp. 22–44) suggests that the stable period is characterized by positive feedback, for example, through increasing returns to scale, learning, or network effects. By contrast, Mahoney (2000, pp. 526–27) also allows for “reactive sequences,” or “backlash processes that transform and perhaps reverse early events.” Mahoney’s more extensive list of constraints includes the amplification of what comes before, reactions against it, and successive chains of tightly bound causes and effects.⁷

In some respects, the most important choice in studying path-dependent processes is whether to problematize the critical juncture that precedes the stable

⁷Mahoney (2006, pp. 11–13) provides a very useful discussion of the distinction. See also Mahoney & Schensul (2006) for a typology of elements on which scholars disagree about the definition of path dependence.

causal pathway. As noted above, path-dependent accounts invoke elements of contingency in the selection from among different plausible alternatives. These critical junctures, “contingent occurrences that cannot be explained on the basis of prior historical conditions” (Mahoney 2000, pp. 507–8), interrupt the causal sequence and pose difficult methodological problems. The resulting accounts may be either post hoc, providing only limited (or no) predictions (Goldstone 1998, p. 834), or ad hoc, according major influence to variables unrelated to the theories under investigation.⁸

Whether one considers ad hoc₃ theory as a problem depends on one’s view of the generative tradition in theory evaluation. To the extent that path-dependent accounts invoke disparate and unrelated causes, they disobey the precept that good theory develops from a body of previous results and guiding principles.

There are some clear affinities between the causes-of-effects template followed by mainstream qualitative methodologists and path-dependent accounts. They are likely to be sympathetic to an ontology that sometimes only allows for a causal pathway to be delineated only after the fact, where there are only a few examples (and perhaps only a single case) of a particular phenomenon, and where detailed knowledge of a case is necessary to uncover the impact of disparate variables.

EXPLANATORY TYPOLOGIES OR TYPOLOGICAL THEORIES

Qualitative methodologists have recently turned their attention to the development of explanatory typologies or typological theories (George & Bennett 2005, Elman 2005), building on an earlier literature on typologies and “property spaces” (Lazarsfeld 1937, Barton 1955). As noted in Table 1, what differentiates a typological theory from a taxonomy designed to define types or classify cases is its theoretical content. The dimensions of the property space associated with a typological theory are provided by the theory’s explanatory variables, and the content of the cells comes from the logic of the theory: Given its posited causal relationships, what particular outcomes are associated with different combinations of values of the theory’s variables?

Explanatory typologies provide researchers with a number of associated benefits. First, because they specify particular conjunctions of variables, they are particularly useful for capturing configurative causation (Ragin 2000, pp. 67–82). As Ragin (2000, pp. 67–82) has noted, using property spaces for qualitative analysis empowers scholars to depict and summarize configurations of multiple causes without taking the variable-oriented approach of seeking a quantitative estimate

⁸The ad hoc issue bears a strong similarity to Lakatos’ (1970, p. 175) notion of ad hoc₃: the idea that theories should be criticized if they lack a unifying idea, and hence invoke disparate and unrelated causes (see also Elman & Elman 2003, pp. 31–32).

TABLE 1 Goals of typologies (Elman 2005)

	Descriptive	Classificatory	Explanatory
Analytic move(s)	Defines compound concepts (types) to use as descriptive characterizations	Assigns cases to types	Makes predictions based on combinations of different values of a theory's variables Places data in relevant cells for congruence testing and comparisons to determine whether data is consistent with the theory
Question(s) answered	What constitutes this type?	What is this a case of?	If my theory is correct, what do I expect to see? Do I see it?
Example	What is a parliamentary democracy, as opposed to a presidential democracy?	Are Britain and Germany parliamentary or presidential democracies?	According to the normative variant of the democratic peace theory, what foreign policy behavior is predicted from a dyad of two mature parliamentary democracies? Do the bilateral foreign policies of Britain and Germany agree with that prediction?

of the importance of those causes across a known population of cases. Relatedly, explanatory typologies are able to capture interaction effects in a different way (and arguably more easily) than large-*n* regression techniques.

Second, explanatory typologies permit theorists to specify the conjunctural conditions under which the same outcome may occur, i.e., they allow equifinality. Modeling theories typologically allows for the possibility of many paths, which may or may not have independent variables in common, to the same outcome. Likewise, a typologically modeled theory allows for the possibility of multifinality, or multiple outcomes in different cases with the same value of an independent variable, depending on the values of other variables (Bennett 1999, p. 9; see also Bennett & George 2001, p. 138).

Third, explanatory typologies are useful for capturing temporal effects (on which see Pierson 2000, 2003, 2004; Mahoney 2000; Buthe 2002; Thelen 2003; Aminzade 1992; see Nowotny 1971 for typologies and time). This is most obviously true for explicitly processual typologies, which capture links between successive types, but may also be a feature of explanatory typologies in which the temporal connections are not so clearly drawn. Nowotny (1971, p. 29) distinguishes between accounts that chart change over time and those that also include an explanation for that difference. One way of approaching the distinction is to

ask whether movement in the property space just reflects what Capecchi (1968, p. 11) describes as time-space links or also includes concepts that endogenize change. This distinction is closely related to Nowotny's (1971, p. 24) description of "processual typologies as consist[ing] of a number of types which are related to each other by an underlying historical-processual dimension."⁹ As an example, Nowotny (1971, pp. 25–28) cites Moore's 1966 study of modernization, in which previous structural arrangements shape the possible alternatives that become available at a later stage (Moore 1993). Nowotny's approach accords with Pye's (1968, p. 726) observation that typologies can track different stages of political development. Temporal effects are likely to be most important in processual typologies, to be present in classifications that include but do not endogenize change, and to be least relevant where transformation is neither explained nor included.

Fourth, as George & Bennett (2005, pp. 233–55) note, typological theories are highly complementary with case study and process tracing methodologies. By focusing on specific types, and setting aside cells of the property space that are empty, theoretically unlikely, unsurprising, or overdetermined, researchers can move beyond two- or three-variable interactions and address potential higher-order interactions (although as a practical matter, beyond five or six dichotomous variables, or 32 to 64 possible configurations or types, the complexity of interactions among the variables becomes increasingly difficult to manage even if many types can be set aside).¹⁰ It is the combination of well-specified types and detailed study of a small number of cases that allows qualitative researchers to gain both inductive and deductive insights into higher-order interactions. The trade-off here is that the interactions theorized or uncovered may not be generalizable beyond the specified types in which they are theorized and/or discovered.

Fifth, with respect to theory testing, typologies help scholars to identify the degree of causal homogeneity between cells and to engage in counterfactual reasoning. As Munck (2004, p. 111) notes, typologies can be used to address the issue of causal homogeneity. Scholars can "identify multiple domains, within each of which the analyst finds causal homogeneity and between which there is causal heterogeneity. . . . [T]ypologies can play a valuable role in defining the universe of cases that can productively be compared" (see also Nowotny 1971, pp. 6–11). Munck's observation is of particular relevance for singling out cells that are important for testing theories (Bennett 1999, p. 21; see also Rogowski 2004, p. 76;

⁹Nowotny (1971, p. 34) observes that "one purpose [of typological procedures is] bringing order into the historical dimension which links the various types. Notions of historical processes, of development and change, underlie most typologies."

¹⁰Lazarsfeld & Barton (1965) developed a set of tools to manipulate the property space, thus decreasing the number of cells that need to be filled. These reductions can include (a) rescaling compression (reducing the level of measurement), (b) indexing (treating equal totals of additive causal variables as equivalent), (c) logical compression (deleting cells that are the product of impossible or highly improbable combinations of variables), (d) empirical compression (deleting empty cells), and (e) pragmatic compression (collapsing contiguous cells if their division serves no useful theoretical purpose) (see Elman 2005).

McKeown 2004, p. 151). Explanatory typologies, for example, can make it easier to identify which cases are suitable for carrying out Eckstein's (1975, pp. 117–20) suggestion that scholars study most likely, least likely, and crucial cases. An additional theory-testing advantage is that using a property space can help identify “distance” between potential cases and facilitate research designs on the basis of their similarity or difference (see Przeworski & Teune 1970, pp. 32–39). In addition, by requiring scholars to write down their expectations of outcomes on every combination, property spaces are also valuable in formulating more precise counterfactual propositions, “subjunctive conditionals in which the antecedent is known or supposed for purposes of argument to be false” (Tetlock & Belkin 1996, p. 4; see also Fearon 1991; Hempel 1965, pp. 164–65; Lebow 2000; G. Goertz & J.S. Levy, unpublished book manuscript).

FUZZY-SET ANALYSIS

Ragin, whose 1987 book *The Comparative Method* emphasizes the configurational nature of cases and the prevalence of complex interactions effects, has more recently outlined the applicability of “fuzzy-set” mathematics to the analysis of cases (Ragin 2000). A full explication is beyond our present purposes, but the key distinguishing characteristic of fuzzy sets is the measurement of cases according to their degrees of membership, between 0 and 1.0, in qualitative categories. For example, rather than giving a nominal or ordinal measure of how democratic a state is, one can describe it as “fully in” the set of democracies (a score of 1.0), “mostly in” (a score of 0.75), “more in than out” (above 0.5), “mostly out” (0.25), “fully out” (0), or somewhere in between these scores [Ragin (2000); see also Goertz (2006) on “family resemblance” concepts and measures]. As Ragin argues, such fuzzy-set scores can offer advantages over crisp, nominal scores. For many purposes, for example, once a state is fully democratic it matters little if it is extremely democratic, and fuzzy-set scores accordingly set aside irrelevant variation among extreme cases. Analysis of fuzzy-set scores can also help substantiate when variables are “nearly necessary” or “almost sufficient” for an outcome, rather than presuming that variables must be fully necessary or sufficient to be of interest.

In comparison with traditional within-case methods of analysis (such as process tracing and cross-case comparisons), fuzzy-set analysis makes different trade-offs between the goals of generalization versus “thick” causal explanation of individual cases. These approaches have different uses, limitations, and comparative advantages. Each approach is vulnerable in its own way to mistaken inferences when relevant variables are omitted from the analysis. In general, fuzzy-set methods are especially strong at assessing relationships of necessity and sufficiency for moderately large populations when theoretical concepts and measures are well established and the diversity of extant cases is not sharply limited. Traditional within-case analyses and cross-case comparisons, however, have advantages when concepts, measures, and theories are not yet strongly validated and when the

research objective places as much emphasis on understanding how causal mechanisms operate within individual cases as on generalizing these mechanisms across broader populations.

In addition to differing on their level of measurement, fuzzy sets and traditional case methods differ on the other three dimensions that Collier et al. (2004, pp. 244–49) have identified as distinguishing qualitative from quantitative work: size of n , use of statistical tests, and use of thick versus thin analysis. Fuzzy-set analysis is explicitly geared to studies of approximately 10 to 60 cases. Ragin (2000) notes the dearth of studies where n is in this range and argues that these methods can help fill this gap. For most research projects, an n smaller than 10 would make it difficult (though not necessarily impossible) to make much use of fuzzy-set methods; the limited diversity of <10 cases in any moderately complex relationship would be insufficient to support strong inferences by the probabilistic analysis of fuzzy sets. Above 60 cases, the comparative advantages of fuzzy sets over statistical analysis of cases begin to diminish. Traditional case comparisons, however, are suited to very-small- n studies of from 1 to 10 cases, as they rely on within-case analysis and discrete case comparisons.

Traditional case study methods, moreover, do not make use of statistical tests. Fuzzy-set research can use such tests to determine whether the outcomes of a particular cell or type of case are sufficiently clear and consistent to establish a claim of (near) necessity or sufficiency when the number of cases in the cell is sufficiently large.

Finally, the methods differ in their degree of reliance on thick within-case analyses. Ragin (2000) notes that fuzzy-set analysis requires sufficient knowledge of the cases to measure them with confidence, but most of his emphasis is on using these measures, once established, to aggregate relationships across divergent kinds of cases. Ragin also focuses on reducing these relationships to the minimum number of statements that cover or include them, although he notes that the conclusions of fuzzy-set analysis must be checked against the researcher's knowledge of cases. In repeatedly warning of the need for considerable theoretical and empirical knowledge of the cases in order to constitute the property space and check the conclusions, Ragin recognizes, but does not fully resolve, the tension between including a sufficient number of cases to make fuzzy-set methods powerful and gaining detailed knowledge of each case.

As discussed herein, typological theorizing is more intimately tied to thick within-case analysis of a small number of cases. In this approach, the emphasis is on the researcher's use of preliminary knowledge of the cases and the theories of interest, which are used to construct a property space, rather than on the thin analysis of many types of cases once the property space is constructed. The purpose of constructing the property space, in this view, is largely to facilitate the selection of a few cases for intensive study and pairwise comparison. The extensive study of these cases is then devoted to testing the typological theory that underlies the property space, to explicating the causal mechanisms at work in the case (rather than focusing mostly on relations of necessity or sufficiency to the outcome),

TABLE 2 Comparison of fuzzy sets and typological theorizing*

	Fuzzy sets	Process tracing and case comparisons
Level of measurement	Fuzzy-set degrees of membership	Nominal, ordinal, interval
Size of n	~10–60	Usually 1–10
Statistical tests?	Yes, if n is large enough	No
Thick versus thin analysis	Fairly thick, depending on n	Very thick

*Categories based on Collier et al.'s (2004, p. 246) chart, "Four approaches to the qualitative-quantitative distinction."

and to providing a theoretically based explanation of the individual case. In this view, process tracing within individual cases is a powerful check on the kinds of inferential errors that can arise from using correlative or comparative methods alone.

As a result of these differences (summarized in Table 2), the methods vary in how likely they are to suffer from omitting a relevant variable from their analysis and how damaging such an omission would be. All methods are vulnerable to omitting relevant variables and overlooking the attendant alternative explanations these variables offer: It is impossible to ever be sure that one has thought of all possible explanations for a phenomenon. The likelihood and consequences of omitting a variable, however, can differ across methods. Seawright (2005) has convincingly argued that the efficacy of fuzzy-set analysis depends on the assumption that omitted combinations of variables are uncorrelated with the combinations or configurations included in the study, not just the narrower assumption (required for statistical analysis) that the omitted variables are uncorrelated with those included in the study. More generally, Seawright (2005) argues that fuzzy-set analysis requires assumptions on the correct functional form of the relationship being studied, as well as on the ability to infer causation from association, which are at least as restrictive as the corresponding assumptions necessary for causal inference from statistical analysis in observational studies. In his reply to Seawright's critique, Ragin (2005, pp. 34, 36) stresses that the successful use of fuzzy-set analysis requires substantial knowledge of the cases being studied, but as noted above, he does not fully resolve the tension between studying a sizeable number of cases and attaining a detailed understanding of each case.

Traditional case study methods are also vulnerable to mistaken inferences because of omitted variables. The likelihood of leaving out a relevant variable may be somewhat lower, however, as this approach involves studying a few cases in detail. Also, as the claims made in typological theorizing are usually limited to contingent generalizations about the limited number of cells or types studied, the consequences of omitting a variable do not necessarily extend to all possible configurations of the variables. The trade-off, of course, is that the more contingent claims of the analysis are not as general as those that fuzzy-set analysis makes

regarding the necessity or sufficiency of various configurations. Put another way, fuzzy sets make somewhat more demanding assumptions about unit homogeneity, which allows them to generalize about the population. By contrast typological theorizing makes less ambitious assumptions about homogeneity but is thus less able to generalize across types.

An additional difference is that fuzzy-set analysis, because it uses probabilistic statistical analysis, does not necessarily call attention to deviant cases or require revision of the property space to accommodate such cases. Ragin (2000, p. 126) notes that the use of probabilistic criteria “partially ameliorates the problem of contradictory outcomes and thus allows for some discordance in outcomes within configurations.” In practice, however, in his analysis of a fuzzy-set example of a study of ethnic mobilization, Ragin (2000, p. 127) does make an effort to explain the one anomalous case of the nonmobilization of the ethnic Ladins of Italy.

George & Bennett (2005) emphasize that a single anomalous case may require revision to the theoretically defined property space. Of course, seemingly anomalous or deviant cases can also be the result of measurement error or of variables that have causal effects in one or a few cases but do not generally have a causal effect in a population. Owing to these various possible sources of anomalies, researchers can use either fuzzy sets or traditional case study methods, with differing philosophical assumptions about whether social life is at some level inherently probabilistic. Researchers using fuzzy sets may decide to try to track down and explain anomalous results (as Ragin does in the Ladin example), and those using typological theorizing may decide, once they have failed to find a theoretical explanation or measurement error, that an anomaly truly represents some irreducible probabilism. In effect, researchers make operative decisions on probabilism or determinism by deciding when to stop pursuing explanations for the error term, an issue that is not determined by the researcher’s choice of method.

Ragin (2000) points out that fuzzy-set methods draw attention to the problem of accounting for empty cells, or types of cases for which there is no extant case. This, he argues, is an advantage over how statistical methods treat this issue, as these methods tend to make assumptions at the front end of the analysis that divert attention from the problem of empty cells. Yet the empty-cell problem, which Ragin terms the problem of “limited diversity,” presents inferential challenges for all methods. Ragin presents two possible approaches to the empty-cell problem within the context of fuzzy-set analysis. First, one can make no assumptions about what the outcome would be in an empty cell, in which case fuzzy-set analysis results in more numerous or less parsimonious, logically reduced statements than would be possible by assuming the outcomes of the empty cells. Alternatively, one can make counterfactual assumptions about the outcomes cases would have had if there had been cases in the empty cells, which allows greater logical reductions but risks wrong inferences.

An important issue is whether any of these approaches has advantages over the other when diversity is sharply limited rather than just somewhat limited. In almost all of Ragin’s (2000) examples, diversity is only somewhat limited; that is, there

are only a few empty cells in property spaces of 16 or 32 or more cells. In George & Bennett's (2005) example of burden sharing in the 1991 Gulf War, however, diversity is limited to only about half of the possible 16 types. When diversity is sharply limited in this way, it is likely that the typological theory approach, in which within-case analysis of a few cases leads to narrow or contingent generalizations, is less at risk of inferential error than fuzzy sets, which rely more greatly on cross-case comparisons across broad parts of the property space.

Methodological choices involve trade-offs. No method is optimized for every research objective and every domain, and none is able to surmount fully the well-known challenges to valid causal inference in nonexperimental settings. In view of the different strengths and weaknesses of fuzzy sets and typological theorizing, when should researchers turn to one approach rather than another? First, as fuzzy-set analysis relies heavily on the proper prior construction of the property space, it is more applicable in research projects where prior theories, definitions, and measurement of variables are fairly well substantiated. Typological theorizing may be more powerful when theories and concepts are not as well established. Second, when the diversity of cases is sharply limited, typological theorizing has advantages over fuzzy sets. Third, when a relatively high number of cases (20 or more) is available and good secondary studies exist for many of these cases, fuzzy-set analysis may be a powerful tool of inference. When only a few cases are extant, typological theorizing is more appropriate. In general, fuzzy-set analysis offers techniques for dealing with many cases and reducing conclusions to parsimonious and general statements, but it is vulnerable to omitted variables that can affect inferences on the entire property space. Typological theory involves fewer but thicker comparisons and relies explicitly on process tracing of individual cases, so its conclusions are typically narrower and more contingent but less vulnerable to inferential errors that would extend beyond the cases thickly studied.

As Ragin (2000, p. 144) has argued, all comparative approaches involve the phases of (a) selecting cases and constructing the property space, (b) testing the necessity and sufficiency of causal conditions, (c) evaluating the results, and (d) applying the results to specific cases. Of Ragin's four phases, then, typological theorizing focuses on the first (specifying the property space) and process tracing focuses on the fourth (testing the space constructed through detailed process tracing in selected cases), whereas fuzzy-set methods are optimized for the second and third phases of testing necessity and sufficiency and evaluating the results of these comparative tests.

CONCLUSIONS

The new generation of work on qualitative research has clarified the practices and raised the standards for using these methods, while also bringing the relative advantages and limitations of different case study methods into sharper focus. This literature more clearly differentiates between within-case and cross-case methods, and it identifies how the iterative use of both can reduce the limitations of either

method used alone. The development of more systematic modes of typological theorizing has provided stronger case-selection techniques for alternative theory-building purposes. Recent writings have also illuminated the epistemological basis of within-case analysis and linked it to discussions in the philosophy of science of causal mechanisms and causal explanation. Qualitative methodologists have identified case study methods as having comparative advantages in developing internally valid and context-sensitive measures of concepts, heuristically identifying new variables through within-case analysis of deviant or other cases, providing a potential check on spuriousness and endogeneity through within-case analysis, and testing and elaborating theories of path dependency and other types of complexity.

The new literature also makes clear that some common critiques of qualitative methods are misguided, but that other challenges remain. Because within-case analysis or process tracing relies on a different logic (one that can perhaps be usefully modeled using Bayesian approaches), it is an inappropriate target for the frequentist degrees-of-freedom critique that has often been leveled against qualitative studies. Deeper problems of underdetermination can still afflict case study research, however. The degree to which these problems obtain is not a simple function of the number of variables included and the number of cases studied; it depends on how the evidence within a case lines up with alternative explanations, and how distinct or incompatible these explanations are in their predictions about the processes within the case through which the outcome arose. Similarly, critiques of case-selection practices developed in the context of frequentist statistical methods have often proven misguided when applied to case study methods. Yet the case-selection challenges and problems that confront qualitative methods are indeed difficult, and the consequences of poor case selection and overgeneralization can be more devastating in case studies than in statistical analyses. Finally, case study methods can provide bases for generalizing beyond the cases studied in some instances, but the potential for generalizing from any particular case study depends on several factors. Generalization requires inferences about the causal mechanisms underlying a case and an understanding of the kinds and frequency of contexts in which those mechanisms operate. The first type of inference can arise from a case study, but whether it will arise from the analysis of a particular case cannot be known a priori; the second type of inference requires separate or prior knowledge of populations.

Case study methods have emerged, like all widely used observational methods in the social sciences, as a useful but limited and potentially fallible mode of inference. Because the relative advantages of case studies are different from and complementary to those of statistical and formal methods, one of the newest and most exciting trends in methodology is the increasing focus on combining methods from the different traditions in the same study or research program (Lieberman 2005). Statistical studies can help identify outliers, which case studies can then examine for new or left-out variables. Statistical studies can in turn test the generalizability of new variables uncovered by case studies. Case studies can examine “typical” cases from statistical correlations to check for possible spuriousness and endogeneity and establish whether the hypothesized mechanisms are indeed

operative. Formal models can be tested through case studies, and the new variables identified in case studies can be formalized in models. The establishment of a firmer basis and clearer procedures for qualitative methods, in short, has created an opportunity for a new phase in social science methodology that emphasizes the complementarity of alternative methods while it more clearly recognizes their differences.

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