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Qualitative Methods

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American Political Science Association
Organized Section on Qualitative Methods

Contents

APSA-QM Section Officers

Letter from the Section President

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This issue of the newsletter finds an active and productive Qualitative Methods section with membership at well over 700 and with some 80 students expected at the Institute on Qualitative Research Methods at Arizona State University this January, almost twice as many as the first institute in 2002. The section business meeting and reception at APSA were very well-attended. Highlights included the presentation of the Giovanni Sartori Award to James Mahoney and Dietrich Rueschemeyer for their book *Comparative Historical Analysis in the Social Sciences*, the Alexander George Award to Peter Hall for his book chapter on "Aligning Ontology and Methodology in Comparative Politics," and the Sage Award to John Gerring for his 2003 APSA paper, "Causation: A Unified Framework for the Social Sciences." The meeting also elected James Mahoney of Brown University as the president-elect, to take office for the 2005 meeting, and Deborah Larson of UCLA and Ted Hopf of Ohio State University as at-large members of the Executive Committee.

At the APSA meeting, I noted three priorities for the remainder of my term as section president. A first goal is to add to the section web site more curricular materials for helping teach qualitative methods courses—i.e., discussion questions, problem sets, and exercises (please send materials to Colin Elman at colin.elman@asu.edu). A second priority is to introduce more qualitative methods courses into the curricula of political science departments, both formally, in terms of course requirements, and informally, in terms of individual faculty (this means you, dear reader!) taking the initiative to field their own courses. A third goal is to raise the standards for the practice and teaching of qualitative research internationally, building ties to political scientists and political science organizations across the globe. I urge section members with ideas or initiatives along these lines to contact me by email.

Thank you for continuing to make our section a success.

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Symposium: Qualitative Comparative Analysis

The following debate is by no means the last word on this topic. Scholars interested in contributing should email their commentaries and critiques to consortium@asu.edu. The section will be publishing any additional material received on its website at http://www.asu.edu/clas/polisci/cqrm/QualitativeMethodsAPSA.html

Introduction: A Note on Terminology

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Charles C. Ragin has written two major books and a number of accompanying articles that discuss methods for the field of qualitative comparative research. These different publications address somewhat distinct techniques, creating the potential for confusion about the appropriate terminology to describe the various methods endorsed by Ragin and his collaborators. This brief note addresses this terminological issue.

Ragin's first book, *The Comparative Method* (1987), argues that Boolean algebraic logic is the implicit methodology of much qualitative comparative research. To formally specify this Boolean methodology, Ragin develops techniques that employ dichotomously measured variables (i.e., "crisp sets") and a veristic understanding of causation that does not systematically incorporate probabilistic considerations. The overall methodology of the book is often referred to as "qualitative comparative analysis" (QCA).

In a subsequent book, *Fuzzy-Set Social Science* (2000), Ragin both modifies the original Boolean approach and presents a quite distinct methodology grounded in fuzzy-set logic. Concerning the modifications to the original Boolean approach, Ragin introduces probabilistic criteria, such that the approach allows for disconfirming cases that arise from measurement error, random error, and/or human error. In addition, Ragin introduces statistical significance tests that identify the probability that observed relationships are the product of chance.

In conjunction with fuzzy-set logic, Ragin develops what could be seen as a completely separate methodological apparatus. Unlike the dichotomous Boolean approach, the fuzzy-set methodology is based on a form of continuous measurement in which cases are coded on a 0 to 1 scale according to their degree of membership in a particular set. Furthermore, the fuzzy-set methodology is grounded in assumptions about probabilistic causality and the importance of employing significance tests.

The evolution of Ragin's work creates the potential for terminological confusion. Much of the problem concerns the label "qualitative comparative analysis," which implies a broad methodology that might encompass all of Ragin's methods. However, because the QCA label was originally applied to the dichotomous Boolean approach, current usage often equates QCA with that approach, and not with the fuzzy-set methodology. Furthermore, many scholars associate QCA with the veristic framework of the 1987 book, not with the probabilistic framework outlined in the 2000 book. Thus, al-though "qualitative comparative analysis" has the potential to be a general description for all of Ragin's methods, in fact the QCA terminology is often associated with only Ragin's earlier work.

One solution to this confusion involves the use of the acronym fs/QCA to describe the full range of Ragin's methods. In my essay below, I use the fs/QCA label in this way. However, others use this label to refer exclusively to the fuzzy-set methods developed in the 2000 book. Thus, the contributors to this newsletter are not completely consistent with one another in their choice of terminology. For future purposes, I offer the following suggestions for describing Ragin's methods:

Dichotomous Boolean methods: The methodology that Ragin presented in the 1987 book. In his subsequent book, Ragin explored the use of probabilistic criteria in conjunction with these methods. Thus, scholars must distinguish between the veristic and probabilistic versions of dichotomous Boolean methods.

Fuzzy-set methods: The methodology that Ragin presented in the 2000 book. These methods are grounded in fuzzy-set measurement, not dichotomous measurement. In addition, although fuzzy-set methods can be used in a veristic fashion, Ragin and others strongly associate these methods with a probabilistic approach.

fs/QCA: An acronym to describe the full range of Ragin's methods— i.e., both fuzzy-set methods and dichotomous Boolean methods. In the contributions below, however, some authors occasionally use this label to refer exclusively to fuzzy-set methods.

QCA: An acronym that is used often to refer to dichotomous Boolean methods. All of the contributions below generally use the QCA label in this way. In time, however, this acronym may become associated with fuzzy-set methods, at which point it might replace fs/QCA as an all-purpose term for describing the various methods associated with Ragin.

References

Ragin, Charles C. (1987). *The Comparative Method: Moving Beyond Qualitative and Quantitative Strategies*. Berkeley: University of California Press.

Ragin, Charles C. (2000). *Fuzzy-Set Social Science*. Chicago: University of Chicago Press.

Qualitative Comparative Analysis (QCA): State of the Art and Prospects

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Qualitative Comparative Analysis (or QCA; Ragin 1987; Ragin and Drass 1994) and its successor, Fuzzy Set Qualitative Comparative Analysis (fsQCA; Ragin 2000; Ragin, Drass, and Davey 2004; Ragin 2004a), were both developed for the analysis of small-and intermediate-N data sets, typical of those used by researchers in comparative politics and related disciplines. These techniques are designed to unravel causal complexity by applying set-theoretic methods to cross-case evidence. Their central goal is to mimic some of the basic analytic procedures that comparative researchers use routinely when making sense of their cases. The key difference between QCA and traditional case-oriented methods is that with QCA it is possible to extend these basic analytic procedures to the examination of more than a handful of cases, for example, to more than 10.[2] In fact, there is no procedural limit on the number of cases that can be studied using QCA.

In this paper, we offer a conceptually oriented introduction to QCA and discuss the state of the art in research using QCA. We begin by examining two analytic procedures commonly used by comparative researchers and contrast these techniques with correlational analysis, the main analytical engine of mainstream quantitative social science. We then present an overview of the state of the art by mapping the diversity and scope of QCA applications. Next, we lay out some general guidelines for using QCA and provide a list of more specific guidelines in the form of "best practices." One of these guidelines concerns the use of "remainders" (combinations of causal conditions lacking cases) in QCA. This leads to an in-depth presentation of one of the most powerful features of QCA: counterfactual analysis.[3] We conclude by sketching several of QCA's future prospects.

The Distinctiveness of Comparative Research

Researchers in comparative politics and related fields often seek to identify commonalities across cases, focusing on a relatively small number of purposefully selected cases. There are two analytic strategies central to this type of research. The first strategy is to examine cases sharing a given outcome (e.g., consolidated third-wave democracies) and attempt to identify their shared causal conditions (e.g., the possibility that they share presidential systems).[4] The second strategy is to examine cases sharing a specific causal condition, or more commonly, a specific combination of causally relevant conditions and assess whether or not these cases exhibit the same outcome (e.g., do cases that combine party fractionalization, a weak executive, and a low level of economic development all suffer democratic breakdown?). Both strategies are set theoretic in nature. The first is an examination of whether instances of a specific outcome constitute a subset of instances of one or more causal conditions. The second is an examination of whether instances of a specific causal condition or combination of causal conditions constitute a subset of instances of an outcome.

Both strategies are methods for establishing *explicit* connections. If it is found, for example, that all (or nearly all) consolidated third-wave democracies have presidential systems, then an explicit connection has been established between presidentialism and consolidation—assuming this connection dovetails with existing theoretical and substantive knowledge.[5] Likewise, if it is found that all (or nearly all) third-wave democracies that share a low level of economic development, party fractionalization, and a weak executive failed as democracies, then an explicit connection has been established between this combination of conditions and democratic breakdown. Establishing explicit connections is not the same as establishing correlations. For example, assume that the survival rate for third-wave democracies with presidential systems is 60%, while the survival rate for third-wave democracies with parliamentary systems is 35%. Clearly, there is a correlation between these two aspects conceived as variables (presidential versus parliamentary system and survival versus failure). However, the evidence does not come close to approximating a set-theoretic relation. Thus, in this example there is evidence of correlation (i.e., a general or *tendential* connection), but not of an *explicit* connection between presidential systems and democratic survival.

As explained in Ragin (2000), the first analytic strategy, identifying causal conditions shared by cases with the same outcome, is appropriate for the assessment of necessary conditions. The second strategy, examining cases with the same causal conditions to see if they also share the same outcome, is suitable for the assessment of sufficient conditions, especially sufficient combinations of conditions. Establishing conditions that are necessary or sufficient is a longstanding interest of comparative researchers (e.g., Goertz and Starr, 2002). However, it is important to point out that the use of set-theoretic methods to establish explicit connections does not necessarily entail the use of the concepts or the language of necessity and sufficiency, *or any other language of causation*. A researcher might observe, for example, that instances of democratic breakdown are all ex-colonies without drawing any causal connection from this observation. A simpler example: colleagues might "act out" only in faculty meetings, but that does not mean that analysts must therefore interpret faculty meetings as a necessary condition for acting out. Demonstrating explicit connections is important to social scientists, whether or not they are interested in demonstrating causation. In fact, qualitative analysis in the social sciences is centrally concerned with establishing explicit connections.

As Ragin (2000) demonstrates, correlational methods are

not well suited for studying explicit connections. This mismatch is clearly visible in the simplest form of variable-oriented analysis, the 2x2 crosstabulation of the presence/absence of an outcome against the presence/absence of an hypothesized cause, as illustrated in Table 1.

Table 1: Crosstabulation of presence/absence of an outcome against presence/absence of a causal condition

	Causal Condition Absent	*Causal Condition Present*
Outcome Present	1.Cases here undermine researcher's argument	2.Cases here support researcher's argument
Outcome Absent	3.Cases here support researcher's argument	4.Cases here undermine researcher's argument

The correlation focuses simultaneously and equivalently on the degree to which instances of the cause produce instances of the outcome (the number of cases in cell 2 relative to the sum of cells 2 and 4) and on the degree to which instances of the absence of the cause are linked to the absence of the outcome (the number of cases in cell 3 relative to the sum of cells 1 and 3).[6] In short, it is an omnibus statistic that rewards researchers for producing an abundance of cases in cell 2 and/or cell 3 and penalizes them for depositing many cases in cell 1 and/or cell 4. Thus, it is a good tool for studying tendential connections.

A researcher interested in explicit connections, however, is interested only in specific components of the information that is pooled and conflated in a correlation. For example, comparative researchers interested in causally relevant conditions shared by instances of an outcome would focus on cells 1 and 2 of Table 1. Their goal would be to identify causal conditions that deposit as few cases as possible in cell 1. Likewise, researchers interested in whether cases that are similar with respect to causal conditions experience the same outcome would focus on cells 2 and 4. Their goal would be to identify combinations of causal conditions that deposit as few cases as possible in cell 4. It is clear from these examples that the correlation has two major shortcomings when viewed from the perspective of explicit connections: (1) it attends only to relative differences (e.g., relative survival rates of presidential versus parliamentary systems), and (2) it conflates different kinds of set-theoretic assessment.

It is important to remind readers that the bivariate correlation is the foundation of most forms of conventional quantitative social research, including some of the most sophisticated forms of variable-oriented analysis practiced today. A matrix of bivariate correlations, along with the means and standard deviations of the variables included in the correlation matrix, is all that is needed to compute complex regression analyses, factor analyses, and even structural equation models. In essence, these varied techniques offer diverse ways of representing the bivariate correlations in a matrix and the various partial relations (e.g., the net effect of an independent variable in a multiple regression) that can be constructed using formulas based on three or more bivariate correlations. Because they rely on the bivariate correlation as the cornerstone of empirical analysis, these sophisticated quantitative techniques eschew the study of explicit connections, as described here.

QCA, by contrast, is centrally concerned with explicit connections. It is grounded in Boolean algebra—the algebra of logic and sets—and thus is ideally suited for identifying key set-theoretic relations. An especially useful feature of QCA is its capacity for analyzing complex causation, defined as a situation where a given outcome may follow from several different combinations of causal conditions—different causal "paths." For example, a researcher may have good reason to suspect that there are several distinct "recipes" for the consolidation of third-wave democracies. By examining the fate of cases with different combinations of causally relevant conditions, it is possible, using QCA, to identify the decisive recipes and thereby unravel causal complexity.

The key analytic tool for analyzing causal complexity using QCA is the truth table. Truth tables list the logically possible combinations of causal conditions (e.g., presence of presidential versus parliamentary form of government, presence/absence of party fractionalization, and so on) and the outcome associated with each combination (e.g., whether democracies with each combination of conditions consolidate).[7] Table 2 illustrates a simple truth table with four causal conditions and sixteen causal combinations.

Table 2: Truth table with four causal conditions (A, B, C, and D) and one outcome (Y)

A	B	C	D	Y*
no	no	no	no	no
no	no	no	yes	?
no	no	yes	no	?
no	no	yes	yes	?
no	yes	no	no	no
no	yes	no	yes	no
no	yes	yes	no	?
no	yes	yes	yes	no
yes	no	no	no	?
yes	no	no	yes	?
yes	no	yes	no	?
yes	no	yes	yes	?
yes	yes	no	no	yes
yes	yes	no	yes	yes
yes	yes	yes	no	?
yes	yes	yes	yes	?

* Rows with "?" in this column lack cases—the outcome cannot be determined.

In more complex truth tables, the rows (combinations of causal conditions) may be quite numerous, for the number of causal combinations is a geometric function of the number of causal conditions (number of causal combinations = 2^k, where k is the number of causal conditions). The use of truth tables to unravel causal complexity is described in detail elsewhere (e.g., Ragin 1987; Ragin 2000; De Meur and Rihoux 2002). The essential point is that the truth table elaborates and formalizes one of the two key analytic strategies of comparative research—examining cases sharing specific combinations of causal conditions to see if they share the same outcome. The goal of truth table analysis is to identify explicit connections between combinations of causal conditions and outcomes.

The State of the Art[8]

At present, we have referenced about 250 QCA applications worldwide.[9] Most of these are English-language, although some applications have been published in French, Swedish, German, Norwegian, Finnish, and Japanese. Apart from work by Japanese scholars, the bulk of QCA applications has been developed by scholars in the U.S. and Northern Europe. It must be pointed out that in some instances it is not clear whether the authors have actually used the QCA software to perform their analyses. All the references we have identified, though (see footnote above), use the QCA Boolean logic (including the minimization procedure, one way or another). This being said, during the last few years, most (if not all) contributions now clearly indicate the use of the software, indicating that the formal tools implemented in the software are becoming better known and more widely taught.

In terms of disciplinary orientation, the largest proportion of applications—more than two-thirds—can be found in political science, political sociology, and sociology. Applications cover "classical" comparative politics topics (political parties, decision-making, social movements, revolutions, welfare states, and so on), as well as an increasing number of policy-oriented topics. In sociology, applications cover mostly topics in historical sociology, as well as organizational sociology. There is also a growing number of applications in other disciplines such as political economy, management studies, and criminology. Finally, a few applications can be found in history, geography, psychology, and education.

Although QCA is designed mainly for small- and intermediate-N research, there is substantial variation across studies in the number of cases. Quite a few applications have a very small N, as low as five cases (e.g., Kitchener et al. 2002), six cases (e.g., Vanderborght & Yamasaki 2004), and seven cases (e.g., Bruegemann & Boswell 1998; Hellström 2001). We have identified some fifteen distinct applications with ten or fewer cases. In the intermediate-N range, most applications are to be found in the broad range from 10 to 50 cases. However, several applications address between 50 and 80 cases (e.g., Williams & Farrell 1990; Rudel & Roper 1996; Nomiya 2001). Still further, some applications are to be found in the large-N domain: respectively 129 (Drass & Spencer 1987), 159 (Yonetani et al. 2003), 1936 (Ragin & Bradshaw 1991), 2964 (Amoroso & Ragin 1992) and 5755 cases (Miethe & Drass 1999). Hence, the method has been applied fruitfully in a very broad range of research designs.

The nature of the cases studied is also diverse. In most applications, cases (and outcomes) are macro- or meso-level phenomena, such as policy fields, collective actors, country or regional characteristics, and so on. However, some scholars have applied QCA to micro-level data. The number of cases ("units of observation") is sometimes increased through specific techniques, often utilizing the temporal dimension (e.g., Clément 2004: from 3 to 9 cases; Rihoux 2001: from 14 to 44 cases).

There is also substantial variation as to the number of conditions included in the analysis, though of course there is (or at least there should be) some connection between the number of cases and the number of variables (Aarebrot & Bakka 1997; De Meur & Rihoux 2002; De Meur, Rihoux & Ragin forthcoming). The vast majority of applications consider between 3 and 9 conditions (modal number: 4 to 6 conditions). Hence, models elaborated for QCA analysis tend to be parsimonious. Some less parsimonious models are to be found, however—for instance, with 12 conditions (Herala 2004) or even 20 conditions (King & Woodside 2001).

Last, but not least, one particularly interesting development in QCA applications is the explicit combination of QCA-type analysis with other types of analysis, both qualitative and quantitative. Most often, there is already a great deal of "upstream" qualitative work involved in the process of achieving an in-depth understanding of cases (Rihoux 2003; Ragin 2004b). Hence our focus here is more on the combination of QCA with other formal—mainly quantitative—methods. A first strand of attempts builds bridges between QCA and process-oriented formal techniques, such as Event Structure Analysis (Bruegemann & Boswell 1998), strategic narratives (Stevenson & Greenberg 2000), game modeling (Boswell & Brown 1999), and social network analysis (Stevenson & Greenberg 2000). On the other hand, several fruitful attempts have been made to confront QCA with some more or less mainstream quantitative techniques: discriminant analysis (Berg-Schlosser & De Meur 1997; Berg-Schlosser forthcoming), factor analysis (Berg-Schlosser forthcoming), various types of multiple regression (Amenta & Poulsen 1996; Ebbinghaus & Visser 1998; Kittel et al. 2000; Nelson 2004), logistic regression (Amoroso & Ragin 1992; Ragin & Bradshaw 1991), and logit regression (Heikkila 2001; Dumont & Bäck forthcoming). We should also mention explicit combinations of QCA with fuzzy sets and/or with multi-value QCA (e.g., Berg-Schlosser forthcoming; Nelson 2004).

In a nutshell: the field of QCA and QCA-related applications is evolving rapidly, with many ongoing projects, including some in-depth endeavors by Ph.D. students in several disciplines. There is still a lot of room for refinement and innovation. At the same time, it is encouraging to see that—probably as a result of the technique becoming more extensively documented and taught—the most recent QCA applications tend to follow some basic rules of "good practices."

Best QCA Practices I: General Guidelines

QCA is still a methodological newcomer; hence, it is important to formulate, out of experience and observation of existing applications, some guidelines regarding "best practices."[10] To start with, some *general* guidelines can be stated.

First, one should make a "reasonable" use of QCA. The technique can be used for at least five different purposes (De Meur & Rihoux 2002; De Meur, Rihoux & Ragin forthcoming). QCA may first be used in a straightforward manner simply to *summarize data in the form of a truth table*—in other words, as a tool for data exploration. Second, the researcher may take advantage of QCA to *check the coherence* of his/her data, mainly through the detection of contradictions. Third, QCA can be used to *test hypotheses or existing theories*. A fourth use, quite close to the third, is the *quick test of any assumption* formulated by the researcher—that is, without testing a preexisting theory or model as a whole. This is another way of using QCA for data exploration. Last, but not least, QCA may be used in the process of *developing new theoretical assumptions* in the form of hypotheses, following a more inductive approach. The researcher should determine which uses of QCA best suit his/her research goals. Indeed, some of these five uses of QCA are still very much under-exploited.

Second, it is advisable to draw on the different functions of the software. Many of these functions are still under-used, such as the "hypothesis testing" function (Watanabe 2003, Yamasaki 2003), which can be exploited in different ways.

Third, technical and reference concepts should be used with precision, in order not to induce confusion in the reader. Several misunderstandings—and misplaced critiques of QCA—stem from the misuse of technical terms. One of the most frequent examples is the reference to "independent variables" (instead of "conditions," as potential explanatory factors are referred to in the language of QCA). The problem is that conditions are *not* "independent variables" in the statistical sense (Rihoux et al. 2004).

Fourth, one should never forget the fundamentally configurational logic of QCA (Ragin 2003b; 2004b; Nomiya 2004). Hence, one should never consider the influence of a condition in an isolated manner, especially in the interpretation of the solution of a truth table.

Fifth, QCA should never be used in a mechanical manner, but instead as a tool that requires iterative steps. With QCA, there are frequent moves back and forth between the QCA analysis proper (i.e., use of the software) and the cases, viewed in the light of theory. Bottom line: the use of QCA should be both case-informed (relying on "case-oriented knowledge"; Ragin 2003b) and theory-informed. When researchers encounter difficulties, they should not try to conceal them; instead, they should explain, as transparently as possible, how they have been resolved. This often implies being transparent about trade-offs, "pragmatic" choices which may at times seem somewhat arbitrary ("rules of thumb") in real-life research. But at least the reader is informed about the choices that have been made and their rationale.

Sixth, one should be careful in the interpretation of the solution of a truth table (reached at the end of the Boolean minimization procedure with QCA). In particular, it is advisable to be cautious before interpreting a truth table solution in terms of "causality." Technically speaking, such solutions express, more modestly, co-occurrences reflecting potential explicit connections. It is then up to researchers to decide (relying on their substantive and theoretical knowledge) how far they can go in the interpretation of the truth table solution in terms of causality.

Finally, in the research process, it is almost always fruitful to use different methods. No researcher should become a "QCA monomaniac"—indeed we would argue that the same is true for any other method, whether or not QCA is used. At different stages of research, it is often the case that different methods suit different needs. Thus it is advisable to use QCA in some stages of research, while exploiting other methods (qualitative or quantitative) at other stages of the research. This is not to say that QCA should necessarily be used in a "modest" way. Indeed, we believe that, in some research situations, QCA should be used as the main data analytic tool.

Best QCA Practices II: Technical Aspects and Procedure

In all QCA applications, at least when one strives to make a "full" use of QCA, there are three main phases: (1) constructing the truth table, based on accumulated case-level knowledge and theoretical knowledge, (2) analyzing the truth table (the "analytic moment"), and (3) taking the results back to the cases and theory to evaluate findings. Questions illustrating the third phase might include: Do the results illuminate new things about the cases? How do they confront existing theories? Following this typical QCA application process, some more precise technical and procedural pieces of advice may be offered:[11]

1. Select cases in a rigorous manner. The way cases are selected—the comparative research design—should be stated explicitly.

2. To the extent possible, develop an "intimacy" with each case, to gain a deep, "thick" understanding.

3. Select the condition variables in a rigorous fashion—in a theoretically and empirically informed way. Do not select too many conditions. It is best to focus on conditions that seem decisive from the perspective of either substantive or theoretical knowledge.

4. When the raw data is quantitative (for example, interval-scale data) and the *N* is not too large, display the data in tables so colleagues can test other operationalizations of your conditions.

5. When using conventional (crisp) sets, explain clearly how each condition is dichotomized. Justify the placement of the 0/1 threshold on empirical and/or theoretical grounds. Use technical criteria (e.g., the mean or the median) only as a last resort. When constructing fuzzy sets, carefully calibrate membership scores using theoretical and substantive knowledge. Pay close attention to the meaning of the label attached to the set (see Ragin 2000; 2004a).

6. If possible, display the truth table and indicate which observed cases correspond to each combination of conditions.

7. If the truth table contains contradictory configurations, resolve them. There are several ways to do this (De Meur & Rihoux 2002; De Meur, Rihoux & Ragin forthcoming).

8. Proceed systematically to four analyses: those for the configurations with a positive outcome (coded 1 for present), first without and then with the inclusion of remainders (i.e., combinations of conditions lacking cases); and then analyze the configurations with a negative outcome (coded 0 for absent), first without and then with the inclusion of remainders. In order to do so, quite naturally, cases with a "0" outcome and cases with a "1" outcome should be included in the research. If the analysis embraces only positive cases, then only one analysis is possible—the analysis of cases coded 1 on the outcome, with remainders defined as negative cases (false).

9. The analysis should be done with software using the Quine-McCluskey algorithm and not by hand.

10. Resolve any "contradictory simplifying assumptions" that may have been generated in the process of minimization with the inclusion of remainders.[12]

11. Provide some information (even in a shorthand manner) about the main iterations of the research (back to cases, back to theories, fine-tuning of the model, etc.).

12. At the end of each truth table analysis, report all combinations of conditions linked to the outcome (i.e., the full solution of the truth table). If one or some subset of these combinations is eventually selected as being more relevant or important, justify this choice.

13. Proceed to a real "return to the cases" (and/or theory, depending on research goals) at the end of the analysis, using the truth table solution as a guide.

Needless to say, there is no *perfect* QCA analysis. In real-life research, some of these best practices are not easy to implement, and indeed they can be time-consuming (especially "case intimacy").

Best QCA Practices III: "Remainders" and Counterfactual Analysis

One of the most interesting—and powerful—features of QCA is its explicit consideration of unobserved combinations of causal conditions (remainders). This aspect of QCA is also subject to some (mostly misplaced) critiques (Markoff 1990; Romme 1995; see De Meur & Rihoux 2002; De Meur, Rihoux & Ragin forthcoming). Thus, a detailed discussion of this aspect of QCA is warranted.

A central characteristic of comparative research, and qualitative research in general, is the simple fact that researchers work with relatively small Ns. Investigators often confront more variables than cases, a situation that is greatly complicated by the fact that comparativists typically focus on *combinations* of case aspects—how aspects of cases fit together configurationally. For example, a researcher interested in a causal argument specifying an intersection of four conditions ideally should consider all sixteen logically possible combinations of these four conditions in order to provide a thorough assessment of this argument. Naturally occurring phenomena, however, are profoundly limited in their diversity. The empirical world almost never presents social scientists all the logically possible combinations of causal conditions relevant to their arguments (as shown with hypothetical data in Table 2, above). While limited diversity is central to the constitution of social and political phenomena, it also severely complicates their analysis.

As a substitute for absent combinations of causal conditions, comparative researchers often engage in "thought experiments" (Weber [1905] 1949). That is, they imagine counterfactual cases and hypothesize their outcomes, using their theoretical and substantive knowledge to guide their assessments. Because QCA uses truth tables to assess cross-case patterns, this process of considering counterfactual cases (i.e., combinations of causal conditions lacking cases) is explicit and systematic. In fact, this feature of QCA is one of its key strengths. However, the explicit consideration of counterfactual cases and the systematic incorporation of the results of such assessments into statements about cross-case patterns is relatively new to social science. The specification of best practices with respect to QCA and counterfactual analysis, therefore, is essential. We begin our discussion of best practices with the description of two techniques for addressing the "limited diversity" of combinations of causal conditions in QCA.

Limited diversity can be seen in the rows of the truth table shown in Table 2 that lack cases. The solution to a truth table depends in part on how these remainder rows are treated. The most conservative strategy is to treat them as instances of the absence of the outcome when assessing the conditions for the presence of the outcome, and to treat them as instances of the outcome when assessing the conditions for its absence. Doing so yields the following solutions to the truth table (Table 2):

presence of the outcome:

$$A \cdot B \cdot c \rightarrow Y$$

absence of the outcome

$$a \cdot c \cdot d + a \cdot B \cdot D \rightarrow y$$

In these equations, uppercase letters indicate the presence of a condition; lowercase letters indicate its absence; A, B, C, and D are causal conditions; Y is the outcome; multiplication (·) indicates combined conditions (intersection); addition (+) indicates alternate combinations of conditions (union), and "→" indicates an explicit connection. The equation for the presence of the outcome states simply that there is a single combination of conditions explicitly linked to Y, the presence of A and B combined with the absence of C (A·B·c). The equation for the absence of the outcome states that there are two combinations of conditions linked to the absence of Y: (1) the combined absence of A, C, and D (a·c·d), and (2) the absence of A combined with the presence of B and D (a·B·D).

In QCA, an alternate strategy is to treat remainders as *don't care* combinations (the *don't care* label reflects the ori-

gin of truth table analysis in the design of switching circuits). When treated as a *don't care*, a remainder is available as a potential simplifying assumption. That is, it will be treated as an instance of the outcome if doing so results in a logically simpler solution. Likewise, it also can be treated as an instance of the absence of the outcome, again, if doing so results in a logically simpler solution for the absence of the outcome. Using the remainder terms in Table 2 as *don't cares* yields the following solutions:

presence of the outcome:

$$A \rightarrow Y$$

absence of the outcome:

$$a \rightarrow y$$

Obviously, the solutions incorporating *don't care* combinations are remarkably parsimonious, but are they plausible? Before addressing this question, it is important to point out that given the evidence in Table 2, a conventional quantitative analysis of these data would quickly lead to the identification of condition A as the proper explanation of outcome Y. After all, as the table shows, whenever A is present, Y is present; whenever A is absent, Y is absent. None of the other causal conditions displays this simple relationship. Thus, the QCA solution incorporating *don't care* combinations dovetails with the results of a conventional quantitative analysis of the same data.

The plausibility of this solution, however, depends upon the results of the researcher's counterfactual analysis. Consider the analysis of the presence of outcome Y. Without incorporating *don't care* combinations, the solution is A·B·c; with *don't care* combinations, it is A. It follows that six *don't care* combinations have been incorporated into the parsimonious solution: A·b·c·d, A·b·c·D, A·b·C·d, A·b·C·D, A·B·C·d, and A·B·C·D. In essence, the conclusion that A is the sole cause of Y, based on the analysis framed by the truth table, assumes that if any of these six combinations of conditions could be found, they would also display the outcome (Y).[13] In other words, the analysis of six counterfactual cases undergirds the conclusion that A by itself causes Y, which is a dramatic use of simplifying assumptions. For this reason, it is common in presentations of QCA to emphasize the fact that researchers must evaluate any "remainders" incorporated as "simplifying assumptions" into the solution of a truth table. This admonition is equivalent to advising QCA researchers to conduct all the requisite counterfactual analyses. (Notice that no comparable admonition would follow from a conventional quantitative analysis of these same data.)

Too often researchers bypass counterfactual analyses because these assessments are tedious and time consuming. Instead, they embrace parsimony and automatically use all the simplifying assumptions incorporated into the most parsimonious solution they can produce. This unfortunate practice duplicates many of the foibles of conventional quantitative analysis. At first glance, the task of evaluating counterfactual cases may seem daunting. However, once it is recognized that theoretical and substantive knowledge make some counterfactuals "easy," this task is greatly simplified. Further, as we show, the incorporation of "easy" counterfactuals into a solution is straightforward and does not require line-by-line assessment of all the remainders reported in the truth table.

Imagine a researcher who postulates, based on existing theory, that causal conditions A, B, C, and D are all linked in some way to outcome Y. That is, it is the presence of these conditions, not their absence, that should be linked to the presence of the outcome. Suppose the empirical evidence revealed that many instances of Y are coupled with the presence of causal conditions A, B, and C, along with the *absence* of condition D (i.e., $A \cdot B \cdot C \cdot d \rightarrow Y$).[14] The researcher suspects, however, that all that really matters is having the first three causes, A, B, and C. In other words, for A·B·C to generate Y, it is not necessary for D to be absent. However, there are no observed instances of A, B, and C combined with the presence of D (i.e., there are no empirical instances of A·B·C·D). Thus, the decisive case for determining whether the *absence* of D is an essential part of the causal mix (with A·B·C) simply does not exist.

Through counterfactual analysis (i.e., a thought experiment), the researcher could declare this hypothetical combination (A·B·C·D) to be a likely instance of the outcome (Y). That is, the researcher might assert that A·B·C·D, if it existed, would lead to Y. This counterfactual analysis would allow the following logical simplification:

$$A \cdot B \cdot C \cdot d + A \cdot B \cdot C \cdot D \rightarrow Y$$
$$A \cdot B \cdot C \cdot (d + D) \rightarrow Y$$
$$A \cdot B \cdot C \rightarrow Y$$

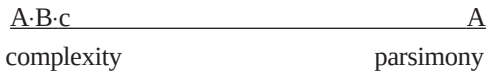
How plausible is this simplification? The answer to this question depends on the state of the relevant theoretical and substantive knowledge concerning the connection between D and Y in the presence of the other three causal conditions (A·B·C). If the researcher can establish, on the basis of existing knowledge, that there is every reason to expect that the presence of D should contribute to outcome Y under these conditions (or conversely, that the absence of D should not be a necessary contributing factor), then the counterfactual analysis just presented is plausible. In other words, existing knowledge makes the assertion $A \cdot B \cdot C \cdot D \rightarrow Y$ an "easy" counterfactual, because it involves the addition of a redundant contributing condition (D) to a configuration which is believed to be linked to the outcome (A·B·C).

It is important to point out that what has been accomplished using Boolean algebra in this simple example is routine, though often implicit, in much case-oriented research. If conventional case-oriented researchers were to examine the empirical instance just listed ($A \cdot B \cdot C \cdot d \rightarrow Y$), they would likely develop their causal argument or narrative based on factors thought to be linked to the outcome (that is, the presence of A, B, and C). Along the way, they *might* consider the possibility that the absence of D observed in these cases might be connected in some way to the production of Y by A·B·C. They would be quite likely to conclude otherwise, given the presumed state of existing knowledge about the four causal con-

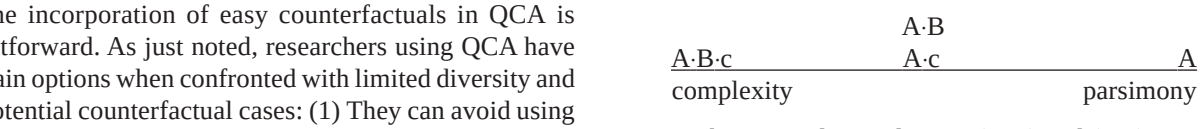
ditions relevant to outcome Y, namely that it is the presence of these causal factors, not their absence, that is linked to the outcome. Thus, they would quickly arrive at the more parsimonious conclusion, A·B·C→ Y. The point is that counterfactual analysis is not always explicit or elaborate in case-oriented research, especially when the counterfactuals are "easy." Such analyses are routinely conducted by case-oriented researchers "on the fly"—in the process of constructing explanations of a specific case or category of cases.

The incorporation of easy counterfactuals in QCA is straightforward. As just noted, researchers using QCA have two main options when confronted with limited diversity and thus potential counterfactual cases: (1) They can avoid using any remainders to simplify a truth table, or (2) they can permit the incorporation of the subset of remainders that yields the most parsimonious solution of the truth table. The first option bars counterfactual cases altogether; the second permits the inclusion of both easy and difficult counterfactuals, without any evaluation of their plausibility. At first glance, neither of these options seems attractive. The first is likely to lead to results that are needlessly complex; the second may lead to results that are unrealistically parsimonious due to the incorporation of "difficult" counterfactuals. It is useful to view these two "options" as endpoints of a single continuum of possible results. One end of the continuum privileges complexity; the other end privileges parsimony.[15] Both endpoints are rooted in evidence; they differ in their tolerance for the incorporation of counterfactual cases.

One strength of QCA is that it not only provides tools for deriving the two endpoints of the complexity/parsimony continuum, it also provides tools for specifying intermediate solutions. Consider again the truth table presented in Table 2, which uses A, B, C, and D as causal conditions and Y as the outcome. Assume, as before, that existing theoretical and substantive knowledge maintains that it is the presence of these causal conditions, not their absence, that is linked to the outcome. The results of the analysis barring counterfactuals reveal that combination A·B·c explains Y. The analysis of this same evidence permitting any counterfactual that will yield a more parsimonious result is that A by itself accounts for the presence of Y. Conceive of these two results as the two endpoints of the complexity/parsimony continuum, as follows:



Observe that the solution privileging complexity (A·B·c) is a subset of the solution privileging parsimony (A). This follows logically from the fact that both solutions must cover the rows of the truth table with Y present; the parsimonious solution also incorporates some of the remainders as counterfactual cases and thus embraces additional rows. Along the complexity/parsimony continuum are other possible solutions to this same truth table, for example, the combination A·B. These intermediate solutions are produced when different subsets of the remainders used to produce the parsimonious solution are incorporated into the results. These intermediate solutions constitute subsets of the most parsimonious solution (A in this example) and supersets of the solution allowing maximum complexity (A·B·c). The subset relation between solutions is maintained along the complexity/parsimony continuum. The implication is that any causal combination that uses at least some of the causal conditions specified in the complex solution (A·B·c) is a valid solution of the truth table as long as it contains all the causal conditions specified in the parsimonious solution (A). It follows that there are two valid intermediate solutions to the truth table:



Both intermediate solutions (A·B) and (A·c) are subsets of the solution privileging parsimony and supersets of the solution privileging complexity. The first (A·B) permits counterfactuals A·B·C·D and A·B·C·d as combinations linked to outcome Y. The second permits counterfactuals A·b·c·D and A·b·c·d.

The relative viability of these two intermediate solutions depends on the plausibility of the counterfactuals that have been incorporated into them. The counterfactuals incorporated into the first intermediate solution are "easy" because they are used to eliminate c from the combination A·B·c, and in this example, existing knowledge supports the idea that it is the *presence* of C, not its absence, that is linked to outcome Y. The counterfactuals incorporated into the second intermediate solution, however, are "difficult" because they are used to eliminate B from A·B·c. According to existing knowledge, the presence of B should be linked to the presence of outcome Y. The principle that only easy counterfactuals should be incorporated supports the selection of A·B as the optimal intermediate solution. This solution is the same as the one that a conventional case-oriented researcher would derive from this evidence, based on a straightforward interest in combinations of causal conditions that are (1) shared by the positive cases (or at least a subset of the positive cases), (2) believed to be linked to the outcome, and (3) not displayed by negative cases.

As our example illustrates, incorporating different counterfactuals yields different solutions. However, these different solutions are all supersets of the solution privileging complexity and subsets of the solution privileging parsimony. Further, we have shown that it is possible to derive an optimal intermediate solution permitting only "easy" counterfactuals. This solution is relatively simple to specify. The researcher removes causal conditions from the complex solution that are inconsistent with existing knowledge, while upholding the subset principle that underlies the complexity/parsimony continuum: any intermediate solution constructed by the researcher must be a subset of the most parsimonious solution. The counterfactuals that are incorporated into this optimal solution would be relatively routine in a conventional case-oriented investigation of the same evidence.

One of the great strengths of QCA is that all counterfactuals, both easy and difficult, are made explicit, as is the process of incorporating them into results. QCA makes this process transparent and thus open to evaluation by the pro-

ducers and consumers of social research.

Future Prospects

In many ways, set-theoretic methods are still in their infancy in the social sciences today. QCA and fs/QCA are both works in progress, with new advances and refinements being made every year. However, there is now a critical mass of researchers in Europe, North America, and Japan using and refining set-theoretic methods. QCA has been advanced primarily through applications by comparative researchers as they confront foundational issues in social science methodology in the midst of conducting what they thought would be straightforward substantive research. While the approach is still developing and maturing, it is possible to sketch several promising prospects. These include: (1) further developing QCA-related software, (2) using QCA to support and advance collaborative research using middle-range Ns, (3) applying QCA to policy questions, and (4) transporting some of the methodological advances implemented in QCA to the analysis of large-N data sets.

Software development. At present, two promising developments are underway, aside from the regular updating of the "standard" (i.e., crisp, dichotomous) software modules and interface. The first one is the development of the fuzzy-set approach, already available in the fs/QCA software. The second one is the "extension" of QCA into MVQCA (through the TOSMANA software). MVQCA applies "crisp" logic to categorical data with more than 2 categories. Another challenge lying ahead will be to find ways to inject the time dimension (i.e., a dynamic perspective) *within* QCA-related software.

Collaborative research. QCA's greatest strength is the analysis of intermediateNs. By *intermediate* we mean more than a handful but too few for sophisticated statistical analysis (e.g., an N in the range of 5-100). In-depth analysis of many cases by a single researcher is quite difficult. However, QCA works best when used in conjunction with in-depth case knowledge. For these reasons, an especially promising use of QCA is in collaborative research involving scholars with different case expertise. This collaboration can extend to all phases of the research process, from developing analytic frames which might serve as the basis for truth tables, to the application of the results of QCA to specific cases. The key to the collaboration is the development of a common framework, which can be implemented through truth tables. A few of these collaborative research efforts are currently underway.

Policy research. The concerns of policy researchers sometimes diverge from those of academic researchers. For example, policy researchers, especially those concerned with social as opposed to economic policy, are often more interested in different kinds of cases and their different fates than they are in the estimation of the net causal effect of independent variables across a large, encompassing population of observations. After all, a common goal of social policy is to make decisive interventions, not to move average levels or rates up or down by some small fraction. Interventions are most feasible when connections are explicit, not tendential, as described in this paper. That is, a particular policy is most capable of decisive intervention when it is grounded in explicit case-oriented knowledge addressing specific categories of cases. QCA is especially well-suited to the generation and accumulation of this type of policy-oriented knowledge. A particularly promising sub-field for QCA applications in this respect is *policy evaluation*, both *ex post* and *ex ante* (De Meur, Varone & Rihoux 2004).

Large-N research. QCA departs fundamentally from the linear models that undergird most quantitative research in the social sciences today. It attends to configurations of case aspects; it seeks to specify complex conjunctural causation; and it addresses the issue of limited diversity by directly considering combinations of conditions that lack cases. In these respects, QCA challenges the mainstream of quantitative social science. Some of the techniques implemented in QCA can be transported directly to large-N investigations. After all, there is no procedural limit *per se* on the number of cases researchers can study using QCA.

For example, limited diversity is not limited to small- and intermediate-N investigations. That is, it is not a problem that exists simply because there are "not enough cases" to populate all the sectors of the vector space defined by the independent variables. Rather, limited diversity is inherent in the constitution of social and political phenomena. Ragin (2003a), for example, demonstrates that a large-N, individual-level data set (N = 758) populates only 24 rows of a 32-row truth table (five causal conditions), and that 13 of these 32 rows contain almost all the cases (96.7% of the total N). In an analysis of individual-level data on musical tastes (N = 1,606), Sonnett (2004) similarly finds that 22 of 64 rows in the truth table (34% of the rows) contain the bulk of the respondents in the sample (90%). Braumoeller (2003: 229) finds abundant evidence of "complex covariation" in a data set with 8,328 observations. Almost any analysis that investigates combinatorial complexity in naturally occurring phenomena will confront an abundance of combinations of causal conditions without cases and thus many potential counterfactual cases. QCA provides a blueprint for confronting these vacancies in the vector space with theoretical and substantive knowledge and thereby avoiding the homogenizing assumptions (e.g., linearity and additivity, to mention the two best-known assumptions) that quantitative researchers usually fall back on.

In conclusion, much remains to be done in this field. There is plenty of innovative work lying ahead, both in terms of methodological/epistemological reflection, further improvement of tools (software in particular), inventive exploitation of these tools, and use of QCA-related techniques in more "academic" as well as more "applied" (in particular policy-oriented) research.

Endnotes

[1] With the support of the *Fonds National de la Recherche Scientifique – FNRS* (Belgium).

[2] Hereafter, *QCA* is used generically to refer to both QCA and fsQCA, as well as to the multi-value QCA (MVQCA) variant; see the "Software" section of http://www.compasss.org and Cronqvist (2004).

[3] Practical descriptions of the technique are presented in De Meur and Rihoux (2002) and in De Meur, Rihoux, and Ragin (forthcoming).

[4] The term *causal condition* or *condition* is used generically in this paper to refer to an aspect of a case that is relevant in some way to the researcher's account or explanation of some *outcome* exhibited by the case.

[5] It is important to point out that neither strategy expects nor depends on perfect set-theoretic relations. For example, if *almost all* (as opposed to *all*) instances of democratic consolidation involved presidential systems, then the researcher would no doubt accept this as evidence of an explicit connection between presidentialism and democratic consolidation. Specific procedures for probabilistic assessment of set-theoretic patterns using benchmarks are presented in Ragin (2000).

[6] We use *correlation* generically here to refer to the examination of the strength of the association between two variables, and not as a specific reference to Pearson's *r* or to the specific calculations used to produce Pearson's *r*.

[7] It is important to point out that the procedures described here are *not* dependent on the use of dichotomies. Truth tables can be built from fuzzy sets (with set memberships in the interval from 0 to 1) *without* dichotomizing the fuzzy scores. These procedures take full advantage of the graded membership scores central to the fuzzy-set approach (see Ragin 2000, 2004a; De Meur, Rihoux and Ragin, forthcoming).

[8] This section is restricted to "crisp," dichotomous QCA (i.e., does not include fuzzy sets or MVQCA). The compilation of the data for this section has been made by Sakura Yamasaki and Sophie Ronsse (COMPASSS, UCL).

[9] A comprehensive list of applications is available through the COMPASSS International bibliographical database: http://www.compasss.org/Bibli%20database.htm.

[10] This section draws on (and extends) some material to be found on the "didactics" page of the COMPASSS resource site (Rihoux et.al. 2003).

[11] This discussion draws on and extends the didactics page of the COMPASSS website. For a discussion of fuzzy-set procedures, see Ragin (2000; 2004a).

[12] Several ways to tackle such "contradictory simplifying assumptions" are presented in a recent issue of the *Revue Internationale de Politique Comparée* (Clément 2004, Grassi 2004, Vanderborght & Yamasaki 2004). See also Rihoux (2001).

[13] Of course, if a "found" combination were to disconfirm the assumptions behind the solution (A causes Y), then it would be incumbent upon the researcher to explain the inconsistency, based on in-depth analysis of the found case(s) displaying the combination in question.

[14] There can be other, unspecified combinations of causal conditions linked to outcome Y in this example. There is no assumption that this is the only combination linked to the outcome (Y).

[15] For an argument in defense of this "most parsimonious" option, see De Meur & Rihoux (2002) and Rihoux et.al. (2004).

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Comments on the Use and Utility of QCA

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One has to respect and admire Charles C. Ragin, Benoît Rihoux and their associates for pursuing an ambitious and worthy task through the years, namely crossing the chasm between purely qualitative research procedures and big-N quantitative studies. Specifically, they propose a set of methods for dealing with an intermediate number of cases (an example might be a study of 20 countries). Despite their claims (both earlier and present), I am sorry to report that I find the results to be disappointing to the point where I would question its basic utility for solving the problem that it addresses. I will not detail those objections that appear in a careful review essay of Ragin's *Fuzzy-Set Social Science* (Lieberson, 2001). Among other matters, there are virtually no data-based comparisons between their proposed procedures and current practices; polemics yes, but no evidence that the proposed methods supplant existing practices. In this essay, I will emphasize several fundamental problems by proposing three hypotheses that should enable us to evaluate QCA—particularly its disturbing use of deterministic ways of thinking in situations for which there is no justification. My hypotheses do not require acceptance of my position, since they imply some distressful features of QCA that can be studied empirically. Hence, the reader unwilling to give up on QCA is in a position to evaluate it.

A crucial assumption in QCA is that the data cross-tabulations reflect a set of deterministic causes. Ragin and associates may not concede this, but the denial cannot pass the quack test—if it sounds like a duck, etc. The method does not allow for a condition in which a given cause increases or decreases the likelihood of some outcome—but not always. Rather, if the outcome in question is sometimes associated with the presence of a given causal variable, but not always, then Ragin and associates will look for additional variables such that a set of interaction effects is eventually found to account for all the observed connections. In other words, when QCA is finished, all conditions are accounted for. For example, the presence or absence of the dependent variable is fully explained in all, say, 20 countries examined. But is this reasonable? Yes and no. It is reasonable in the sense that this could be the case after they throw into the pot combinations of "necessary, insufficient, and sufficient." And, indeed, by the time they finish we have an explanation of every single case—in which the dependent variable appears or does not appear. You might well wonder: How could this be bad? For several reasons, what appears to be so wonderful is really not wonderful and, indeed, is apt to be misleading. It generates a plausible account of the observed data, but hardly the only plausible account that one could generate. "OK," you might want to say, "this is the story for social science in general. What's so special about this?" The answer is that we are no closer to knowing anything than before we started. First of all, a fan of QCA might become a little suspicious when he/she thinks about the fact that QCA is less prepared to allow for chance and probabilistic processes than is the case in many of the hard sciences. That ought to make us stop and wonder. If we are operating in a probabilistic universe, and if we recognize that there are errors in data and that almost surely the comparisons involve influences that are not comparable or necessarily or always measurable (even if imperfectly), then it is bothersome that the data table provided in QCA cannot ascertain whether the observed pattern includes a stiff dose of random results and leads to massive over-interpretation. Indeed the procedures do not rule out the possibility that the observations are all a random matter and/or that none of the causal variables were even measured. Everything gets explained, but never through any variable that simply increases or decreases the chances of an outcome. In QCA, added variables are brought in to generate interaction effects so as to explain why a given variable appears to have one effect in

some cases and another effect in other cases. Using QCA, we are bound to have a full explanation, but we have no reason to accept it. At this point, I will stick my neck out and propose several hypotheses that can be tested; indeed, I will describe how these tests can be performed.

Hypothesis I: *The QCA method is unable to distinguish randomly assigned values (in other words, a table that has no meaning) from a table based on real data*. Suppose I start with a table containing a number of independent variables and some dependent variable. And suppose, just to make our life simple, all of these variables are dichotomous Yes/No variables. Now let us randomly assign cases to a truth table. We are going to work with 20 cases. Let us start with case1: we will randomly assign it a "yes" or a "no" for the first independent variable; then go on to the second independent variable and do the same, and so forth. And finally, we will get to the dependent variable and randomly assign a "yes" or "no" for that too. We will then start with case 2 and repeat that over and over again until we have information for all 20 cases. Now what? If one applied QCA to the resulting truth table, my hypothesis predicts that a model would emerge, with interaction effects and all that, that can account for the pattern observed. If that is the case, how can we evaluate QCA as doing much for us? When it can't distinction random data from real data, and can't distinguish a pattern based on real data in which a central independent variable was excluded from a pattern in which the true independent variable was included?

Hypothesis II is based on a more realistic test of QCA. Suppose we take the actual marginal values from a table of real data that was analyzed through QCA. In this case, the values for each variable will not tend towards roughly half "yes" and half "no." Rather it will reflect the actual number of "yes" and "no" cases for each variable used in the original QCA. Again, we randomly assign the values to a truth table so that the marginals are identical to those observed in the original data but their distribution among cells is random. The pattern will be different of course, but this hypothesizes that QCA will generate a "full" account of the data. In other words, it will again have no problem explaining the dependent variable, but it will be with a largely different story.

As a bonus, I offer a third hypothesis that I think is the most crucial for many purposes since the actual data are taken as given and not manipulated. Suppose we obtain data for which there are 40 cases. We randomly assign the 40 into two sets of 20 cases (sets A and B). We apply QCA to set A. An account is obtained. We then apply QCA to set B. Again an account is obtained. Hypothesis III predicts that the two accounts are substantially different from each other. And, if that proves generally true, it tells us that QCA is being used far beyond what it is capable of doing. QCA over-interprets its data and therefore goes well beyond what is justified (again, unless an extraordinary form of determinism is assumed). For a variety of reasons, I believe this will rarely be justified.

So, if these hypotheses hold generally (albeit, as a probabilist would think about it, not always), then where does this leave us? It supports a more modest and less grandiose way of dealing with the kind of data that QCA is meant to handle. We can learn a great deal if we employ a Boolean approach, develop a simple truth table, observe the patterns, but refrain from attempting to account for everything. This will generate some straightforward and valuable observations. Fired up by Ragin's use of the Boolean approach, I was involved some years ago in a study where we examined various characteristics of popular first names such as their endings, their biblical status, their etymology, their historical usage, and the education level of the mothers favoring the name (Lieberson and Bell: 1992, 531-538). We did not worry about explaining everything but were content to observe taste patterns linked to education and could make reasonable sense of them. But I am sure that we would have been in great trouble if we took a deterministic view such that we searched for various interactions in order to explain everything. And that is a moral that I would pass on if it turns out that my three hypotheses hold.

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Qualitative Comparative Analysis vis-a-vis Regression

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Qualitative Comparative Analysis (hereafter QCA) is a relatively recent set of techniques. Its emergence can be roughly dated to Charles C. Ragin's (1987) book, *The Comparative Method*. Therefore, it is perhaps not surprising that QCA has not yet received the sustained evaluation of its inferential strengths and weaknesses that other techniques, such as regression analysis and comparative case study research, have benefited from.

This article offers an initial step in the direction of such a dialogue by comparing, at an intuitive level, the way three of the most important assumptions used in social scientific causal inference are handled in regression analysis and in QCA.[1]

The argument in favor of comparing different methods in terms of their approach to assumptions can be made as follows: Most of the factors that distinguish methods in practical terms—including ease of use, subtlety of interpretation, and room for analytic creativity—involve issues that can be overcome largely through investigator training. Thus, for example, poorly trained researchers using regression analysis may assume mechanically that all variables are correctly measured—just as poorly trained comparative case-study researchers may sometimes be overly inclined to trust actors' explanations of their own behavior. But well-trained research

ers in either tradition will be immune to these temptations. Hence, a discussion of these issues tends to become a plea for better training, rather than a reasonable argument for fundamental change in a method.

The assumptions that are required for causal inference using a specific set of analytic tools, by contrast, are a formal component of those tools. Indeed, a change in such assumptions is typically enough to constitute a fundamental change in the tools. Hence, a comparison of underlying assumptions provides a solid base for deciding whether one method is currently well enough developed to replace a second method. In making such a comparison between regression analysis and QCA, I will focus on three central assumptions: assumptions about functional form, assumptions about missing variables, and assumptions about association and causation.

Functional Form

Assumptions about functional form play a prominent role in regression analysis. The simplest functional form to consider in regression is an additive linear functional form, in which the marginal effect of a one-unit change in a given independent variable on the dependent variable is constant—both across the entire range of that variable and for any value of the other independent variables. This assumption is so common in statistical work in the social sciences that Abbott (1988) has coined the phrase "General Linear Reality" to describe (and lament) it. More recently, Franzese (2003) has argued that causal interactions among independent variables are ubiquitous, and that regression analysts must move away from the still all-too-common assumption of additive linear functional forms to accommodate this reality.

Yet it is important to recognize that regression analysis is not formally tied to the assumption of additive linear functional form. Researchers are free to consider an endless variety of different functional forms. This is accomplished by transforming the observed independent variables, using the desired functional form, before the regression is run. The result is a model that estimates coefficients associated with a specific set of hypothesized nonlinear relationships between the independent variables and the dependent variable. In effect, this transforms assumptions about functional form into a set of choices about how to measure and score the independent variables.

Assumptions about functional form in QCA can be handled in a way that is surprisingly similar to the regression approach. QCA, in both its Boolean-algebraic and fuzzy-set forms, relies centrally on what could be described as a step function or a threshold function. This functional form is illustrated in Figure 1 for the Boolean-algebraic case and in Figure 2 for the fuzzy-set case. In each figure, vertical axis represents the conceptual and/or empirical dimension that lies behind the observed independent and dependent variables.[2] The gray region in each figure shows the empirical configurations that the functional form predicts; the white region indicates configurations that contradict the functional form.

A surprisingly wide range of specific functional forms can be accommodated within this threshold-function framework. By altering the cut-points used to translate underlying conceptual dimensions into variables, and potentially by reversing the polarity of variables, researchers can test an endless variety of different threshold models. For example, while Figure 2 shows an essentially linear fuzzy-set threshold model, Figure 3 presents an alternate coding of the variables that is more akin to a quadratic threshold model than a linear one. Hence, just as in regression analysis, assumptions about functional form in QCA can be translated into decisions about the measurement of included variables.

Also as in regression analysis, these decisions about functional form must be made before a QCA analysis begins. The technique itself is unable to make these decisions, or correct poor decisions, for the analyst. Incorrect assumptions about functional form may well lead QCA researchers to conclude that there is no relationship between an independent variable and the dependent variable, or only a very complex relationship mediated through several other independent variables—even when there is a straightforward relationship between the two that simply requires a change in functional form.

With respect to assumptions about functional form, then, QCA is in a position that is, at least in theory, similar to that of regression analysis. In practice, on the other hand, regression analysis may have a more established methodological literature on when and how to incorporate tests of alternative functional forms into a research program, see, for example, the research traditions involving regression diagnostics (Fox 1991) and generalized linear models (McCullagh & NeIder 1998; Gill 2001). It probably would be a positive step for QCA to imitate the trajectory of regression in this regard by developing a literature that provides detailed theoretical and practical advice on testing alternative functional forms.

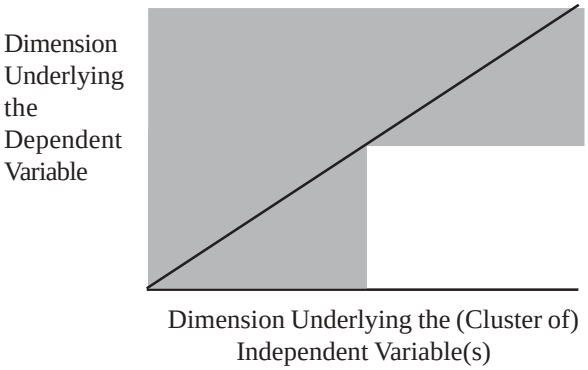
Omitted Variables

Assumptions about omitted variables rightly play a major role in standard statistical and econometric texts on regression analysis (Kennedy 1998: 78-80,88-90; Greene 2000: 334-37). The typical regression relies on the rather strong assumption that all causally relevant independent variables that are omitted from the analysis have no statistical relationship with any of the included variables. Roughly speaking, omitted variables are permitted only if they are completely random with respect to the independent variables. This assumption would obviously be reasonable in the context of a well-designed experiment, in which all variables except the treatment are randomized by design. The assumption also may be plausible in the context of some natural experiments (Brady & McNulty 2004). However, for the vast majority of observational studies, there is no good reason to believe that omitted variables will have no relationship with the included variables. Hence, regression analysis often requires a kind of leap of faith with respect to omitted variables.

Discussions of QCA, in a somewhat surprising contrast, rarely focus on issues of missing variables—yet assumptions about missing variables are just as important in QCA as in

Figure 1: A simplified image of the basic threshold function underlying Boolean-algebraic QCA.

Figure 2: A simplified image of the basic threshold function underlying fuzzy-set QCA, with a linear functional form.

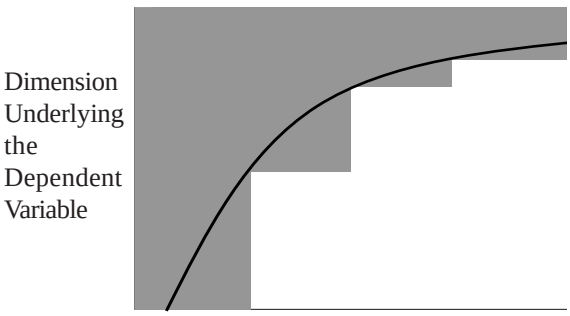


regression analysis. In fact, a careful consideration shows that QCA requires more stringent assumptions about missing variables than does regression.

This is easiest to see in Boolean-algebraic QCA. During the process of a Boolean-algebraic analysis, independent variables are sometimes dropped from the equation that summarizes the relationships between the independent variables and the dependent variable. This exclusion of a variable only happens when that variable has been inferred to have no relationship with the dependent variable.

However, if one or more independent variables have been omitted from a QCA analysis, that means the researcher has, in effect, dropped those variables from the equation by assumption. Since dropping a variable means that the variable has no relationship with the dependent variable, it follows logically that Boolean-algebraic QCA relies on the assumption that there are no causally relevant omitted variables.[3] This assumption is more restrictive than the assumption employed in regression analysis, which does allow certain special kinds of omitted variables. Hence, on this dimension of comparison, regression analysis currently comes out ahead: regression will succeed in any situations where QCA succeeds, and also in some contexts where QCA fails. Further research on how to make QCA more robust to omitted variables should therefore rank high on the methodological agenda.

Figure 3: A revised threshhold function for fuzzy-set QCA, now using a quadratic functional form.



Association and Causation

The third, and in many ways most important, set of assumptions to be considered in this article involve the relationship between association and causation. In discussions of regression, researchers often are warned that association is not causation (Kennedy 1998: 66-67; Greene 2000: 656-57, 742-44). Yet using the results of a regression analysis as the basis for a causal inference in fact requires an assumption that, at least in a given context, association is in fact causation.[4] This assumption is typically difficult to believe unless it can be supported with some kind of compelling side evidence, from theory from in-depth interviews, or some other source. Hence, if QCA requires a less stringent set of assumptions about association and causation than does regression, that fact would be sufficient to imply that QCA is a major methodological advance.

Unfortunately, neither Boolean-algebraic nor fuzzy-set QCA allow weaker assumptions about association and causation than regression. Boolean algebra, the mathematical core of the original version of QCA, is a set of "truth-preserving" logical operations. This means that the epistemological status of the initial data is preserved through to the conclusion of the analysis. If the initial data are false, the conclusion also will be false. If the data represent true causal relations, the conclusion also will represent causation. However, if the data only contain the results of observational studies, then the conclusions can be only associational. Making a causal inference from such conclusions thus requires the assumption that association is the same as causation.

Fuzzy-set QCA revolves around a somewhat different set of mathematical tools, depending centrally on significance tests based on the statistical distribution called the binomial. For present purposes, it is enough to note that, since inferences in fuzzy-set QCA are based on a statistical test that measures degree of association (which is all that statistical tests, in themselves, can usually do), the QCA results must also be associational in nature. Hence, once again, making a causal inference

requires some kind of assumption that association is equivalent to causation.

In summary, then, causal inference from either regression analysis or QCA relies heavily on rather dubious assumptions that association is equivalent to causation. Research within the QCA tradition on how to formally incorporate diverse kinds of outside information that might make such assumptions more plausible would therefore be a major contribution.

Conclusions

With respect to functional form and the relationship between association and causation, regression analysis and QCA depend on fundamentally similar assumptions. When it comes to omitted variables, QCA in fact requires more stringent assumptions than does regression analysis. Hence, there is some basis for concluding that QCA, in its current form, is a less appropriate technique for empirical research than standard regression analysis. Refinement and modification of QCA, along the lines briefly mentioned above, may well reverse this conclusion in the future. Alternatively, such refinement may well have serious costs, in terms of multicollinearity, problems of degrees of freedom, and other statistical issues that QCA has thus far managed to avoid. Dealing with these trade-offs is a complex matter. In light of the trade-offs, more established approaches, including statistical analysis and comparative case study techniques, may come to be seen as preferable to QCA in most situations.

Endnotes

[1] Many of the technical details behind this argument are discussed in Seawright (2004).

[2] In a simplification made necessary by the constraint of presenting figures in at most two dimensions, the independent variables are presented on a single axis. In reality, there may be many independent variables, and these variables need not move in lock-step.

[3] Assumptions about missing variables in fuzzy-set QCA are somewhat more technically complex and are therefore not discussed in this short article. However, it turns out that the assumptions involved in fuzzy-set QCA are as problematic as those in Boolean-algebraic QCA. See Seawright (2004).

[4] A perhaps more palatable alternative is to use theory to derive causal implications from associations. However, this approach replaces the assumption that association is causation with the assumption that a given theory is correct. It is less than entirely clear that this latter assumption is generally preferred to the former.

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Reflections on Fuzzy-Set/QCA

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Ragin and Rihoux offer an informative overview of qualitative comparative analysis and its fuzzy-set extension (fs/QCA). They frame their discussion around three issues: (1) the distinctiveness of fs/QCA; (2) the state of the art; and (3) best practices. I will follow their lead by organizing this short note around these issues. However, whereas Ragin and Rihoux focus mostly on best practices, I will focus mostly on the distinctiveness of fs/QCA.

The Distinctiveness of fs/QCA

Ragin and Rihoux emphasize the distinctiveness of fs/QCA by noting that the approach is not grounded in a correlational understanding of causation, as is true of most forms of statistical analysis (e.g., multiple regression analysis, factor analysis, and structural equation models). Instead, fs/QCA relies on an understanding of causation that can be translated into the language of necessary and sufficient causes. As others have shown, both the correlational and the necessary/sufficient understandings of causation are commonly used in the scholarly literature, and both merit the attention of social scientists (e.g., Goertz and Starr 2003). At present, however, the correlational approach dominates methodology in the social sciences.

In part because of this dominance, scholars may not be aware of the difference between correlational causation and necessary/sufficient causation. A full explication of this difference requires mathematical language and a technical discussion that is not appropriate for this short and informal

note (see Goertz and Starr 2003). Hence, I will oversimplify here to draw the contrast.

Let us first assume that we are dealing with continuously measured variables. In this context, *correlational causation* assumes that higher (or lower) values on one variable will probabilistically produce higher (or lower) values on another variable. For example, a well-known correlational hypothesis is the idea that level of economic development is positively associated with level of democracy net of other variables. This approach to causation underlies additive linear regression. By contrast, *necessary causation* assumes that the occurrence of one specific range of values on an outcome variable usually or always requires the presence of one specific range of values on a causal variable, regardless of the values of all other variables. For example, a necessary causation hypothesis is the idea that a minimal level of economic development is a prerequisite for democracy. Finally, s*ufficient causation* assumes that one specific range of values on a causal variable will usually or always be associated with one specific range of values on an outcome variable, regardless of the values of all other variables. For example, a sufficient causation hypothesis is the idea that the absence of a minimal level of economic development will almost always produce the absence of democracy.

Let us now assume that we are working with dichotomously measured variables. In this context, *correlational causation* assumes that each value on an independent variable will be associated with a specific value on the dependent variable. For example, if the independent variable is economic development (present/absent) and the dependent variable is democracy (present/absent), then the correlational analyst might hypothesize that the presence of economic development is probabilistically associated with the presence of democracy, whereas the absence of economic development is probabilistically associated with the absence of democracy. This approach underlies logistic regression. By contrast, *necessary causation* assumes that the absence of a causal factor will usually or always be associated with the absence of an outcome, whereas the presence of a causal factor may or may not be associated with the outcome. For example, if the presence of economic development is necessary for democracy, then the absence of economic development will be associated with the absence of democracy. However, the presence of economic development may or may not be associated with the presence of democracy (e.g., there could be many more cases of non-democracy than democracy when economic development is present). Finally, *sufficient causation* assumes that the presence of a causal factor will usually or always be associated with the presence of an outcome, whereas the absence of a causal factor may or may not be associated with the outcome. For example, if economic poverty is usually sufficient for authoritarianism, then the presence of economic poverty will be associated with the presence of authoritarianism, but the absence of economic poverty may not be associated with the absence of authoritarianism.

The differences between the hypotheses entailed in these alternative understandings of causation are subtle but fundamental. It is possible to discover a relationship in which twovariables are strongly correlated, but they do not represent necessary or sufficient causation. Likewise, it is possible to discover a relationship in which one variable is necessary or sufficient for another variable, but the two variables are not statistically correlated with one another. Indeed, with continuous variables, the typical bivariate scatterplots that correspond with these two kinds of causation are quite distinct. The data points in scatterplots diagramming strong correlational findings normally are arranged in a pattern of linear covariation; the data points in necessary or sufficient scatterplots are arranged in a distinctively triangular pattern.

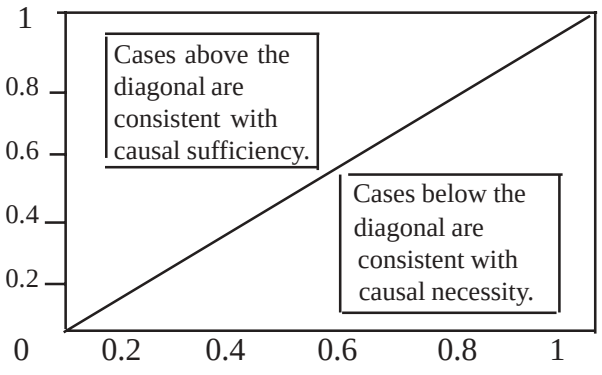
Table 1: Seven Erroneous Beliefs about Necessary and Sufficient Causes (from Mahoney 2004)

Belief 1: Necessary and sufficient causes do not exist
Belief 2: All necessary and sufficient causes are trivial, tautological, or irrelevant.
Belief 3: Social scientists do not formulate interesting hypotheses about necessary and sufficient causes.
Belief 4: Necessary and sufficient causes are deterministic and inherently inconsistent with probabilistic analysis.
Belief 5: Necessary and sufficient causes cannot be measured continuously.
Belief 6: Methods do not exist for testing hypotheses about necessary and sufficient causes.
Belief 7: It is impossible to test for statistical significance with necessary and sufficient causes.

Ideas of necessary causation and sufficient causation initially may seem simple, and, as a result, scholars may develop certain erroneous intuitions about these causes. Elsewhere I have discussed seven such erroneous beliefs about necessary and sufficient causes, and I summarize them here in Table 1 (see Mahoney 2004). In my view, these beliefs do not reflect a sophisticated understanding of necessary and sufficient causes, and they provide a weak basis for critiquing work that analyzes necessary and sufficient causes. Ragin and Rihoux present a dichotomous Boolean truth table to showcase the distinctiveness of the fs/QCA approach. By contrast, I prefer illustrations that use continuous measurement in the form of fuzzy-sets and that assume probabilistic relationships. Figure 1 reproduces a diagram from Stryker and Eliason (2003) that illustrates the location of cases on a fuzzy-set graph for necessary and sufficient causes.

To recall, fuzzy-set analysis codes variables continuously from 0 to 1 according to their degree of membership within a given category. With a necessary cause, fuzzy-set scores on the cause are greater than or equal to scores on the outcome. The resulting scattergram thus has a triangular shape, with all points on or beneath the main diagonal. By contrast, with a sufficient cause, fuzzy-set scores on the cause are less than or

Figure 1: Location of Cases Supporting Causal Necessity and Sufficiency in Fuzzy-Set Analysis
(adapted from Stryker and Eliason 2003)

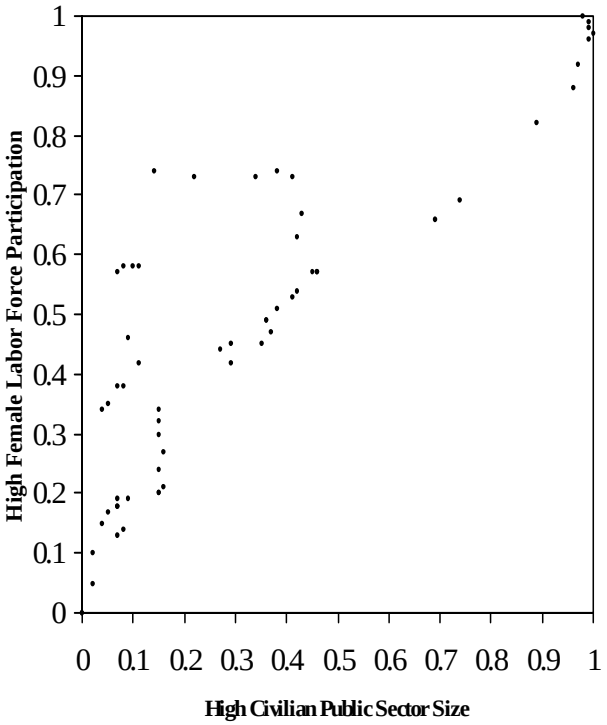


equal to scores on the outcome. Hence, all points will be located on or above the main diagonal. Ragin (2000) and others have formally elaborated the mathematics and logic behind these procedures, including the important issues that arise when considering combinations of variables that are sufficient for an outcome.

Figure 2 provides one example of a probabilistic sufficient cause from Stryker and Eliason's (2003) study of labor markets and the welfare state in France, Belgium, Germany, Italy, Denmark, and Britain from 1977-1994. The data in Figure 2 are quite consistent with the interpretation that "high civilian public sector size" is almost always sufficient for "high female labor force participation." We can see this because nearly all of the data points fall above the bisecting diagonal line.

When presenting fs/QCA to new audiences, I think diagrams like that in Figure 2 have several advantages over hypothetical dichotomous presentations. To begin, Figure 2 is based on real data from real research, and thus it moves the discussion away from the imaginary. Moreover, the hypothesis explored in Figure 2—the argument that "high civilian public sector size" is sufficient for "high female labor force participation"—is substantively interesting and important. Furthermore, not all of the data points are fully consistent with the hypothesis (e.g., a couple points fall just below—not above—the diagonal line), and thus the relationship is probabilistic in nature, not deterministic. As such, the example might pique the interest of quantitative researchers, who might then ask questions about the techniques that fs/QCA analysts use to summarize the degree to which data are consistent with necessary or sufficient causation (there are currently several operations available). Moreover, the example uses continuous measurement and thus sidesteps the reservations that some researchers hold about dichotomous measurement. Indeed, examples like this one can serve to teach researchers how to recognize when continuous data might be consistent with probabilistic necessary or sufficient causation. In turn, this knowledge can be a first step toward learning more about

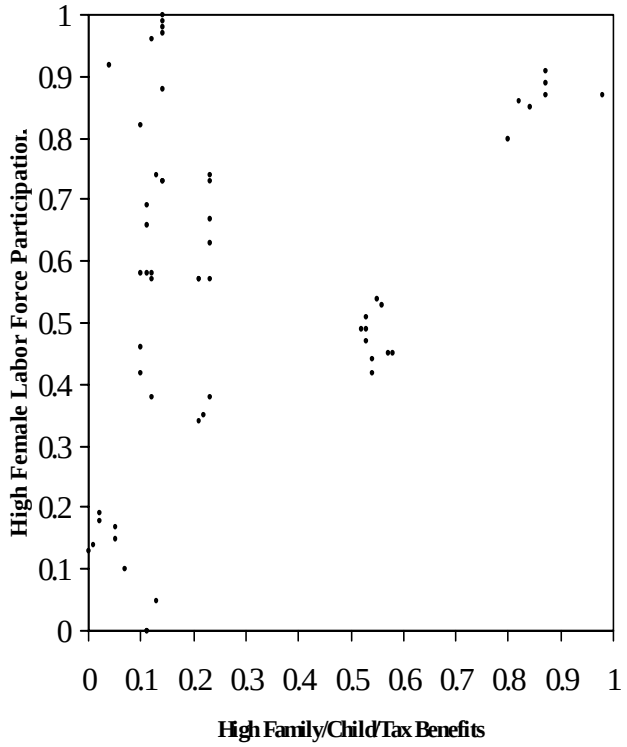
Figure 2: Example of Probabilistic Sufficient Cause: Plot of "High Female Labor Force Participation" against "High Civilian Public Sector Size"
(from Stryker and Eliason 2003)



fs/QCA methods, much as illustrations of bivariate correlations can be one simple step toward learning more about mainstream statistical methods.

The example in Figure 2 has one drawback: the data also correspond to a standard correlation between two variables. That is, as values on civilian public sector size increase, so too do values on female labor force participation (the correlation coefficient is .85). For well understood reasons, triangular patterns of necessary and sufficient causation will often represent correlations, though this is not inevitable. For example, consider the data in Figure 3, which explore the relationship between family/child/tax benefits and female labor force participation among advanced countries (adapted from Stryker and Eliason 2003, with modifications to Belgium's data). These data are consistent with the idea that "high benefits" are probabilistically sufficient for "high female labor force participation," even though a few data points fall beneath the diagonal and weaken the relationship. By contrast, the data are not assembled in a clear linear pattern, and they do not show a strong statistical correlation (r = .34). Hence, the analyst who is investigating only correlational causation would

Figure 3: Example of Probabilistic Sufficient Cause Without Correlation: Plot of "High Female Labor Force Participation" against "High Family/Child/Tax Benefits" (adapted from Stryker and Eliason 2003; Belgium data have been altered)



likely conclude that there is at best a weak relationship between the two variables; by contrast, a fuzzy-set analyst would likely conclude that the data are quite consistent with the idea that high family/child/tax benefits is usually sufficient for high female labor force participation.

State of the Art

Ragin and Rihoux offer a nice summary of the state of the art of fs/QCA work. To this, I would add two comments about the direction of some fs/QCA work. First, not enough fs/QCA work has been grounded in in-depth case study research. When compared to regression analysis, fs/QCA has a much more developed set of norms for integrating case-based research with its formal apparatus of hypothesis testing. Indeed, Ragin and collaborators developed fs/QCA in large part by specifying the kinds of procedures that case-based researchers already implicitly use. However, too much work has been concerned with employing the technical apparatus of fs/QCA without clearly linking variable measurement and model specification to detailed case knowledge. Part of the problem is that, in an article-length format, it may be nearly impossible to simultaneously pursue case-oriented research and formally employ the technical side of fs/QCA. As a result, article-length applications of fs/QCA have tended to sacrifice the case-based orientation of fs/QCA in order to employ the technical elements of the approach. By contrast, book-length studies (e.g., Wickham-Crowley 1992; Hicks 1999) have been more successful at combining both sides of the fs/QCA methodology.

The case-based orientation of fs/QCA is critical because it allows this overall methodology to address common problems that arise in non-experimental research. For example, the problem of correctly specifying a causal model, including the issue of omitted variable bias, can be mitigated if the researcher engages in the close qualitative analysis of cases. Analysts are simply much less likely to exclude key variables if they know a great deal about the cases and phenomena under investigation. Likewise, the problem of distinguishing association from causation, which arises in all modes of research, is often best addressed through a qualitative analysis that employs one or more modes of within-case analysis. Insofar as the overall methodology of fs/QCA is designed to encompass precisely this kind of qualitative appraisal, whereas regression analysis is not, fs/QCA would seem to be much better equipped to identify causation. However, capitalizing on this advantage requires employing the case-oriented side of fs/QCA as much as its technical side.

As a second observation about the state of the art, I suggest that not enough scholars have tapped into the technical apparatus of fs/QCA for the purposes of concept analysis. Both the classical approach to concepts (e.g., Sartori 1970) and the family resemblance approach to concepts (e.g., Wittgenstein 1953) are built around ideas of necessary and sufficient conditions (see Goertz 2006). Accordingly, fs/QCA is quite appropriate as a methodology for the study of concepts. Fuzzy-set analysis offers an especially useful set of tools for analyzing how the varying degree to which defining attributes are present in a case shapes the extent to which that case is a member of a category. As part of a larger group, I participated in a project that employed this approach for the measurement of political democracy in Central America (Bowman, Lehoucq, Mahoney 2006). There is the potential for more elaborate and more sophisticated research in this direction, but scholars must first view fs/QCA as a tool not only for causal analysis, but also for descriptive inference and concept analysis.

Best Practices for Users

Much of the Ragin and Rihoux piece is devoted to an excellent discussion of best practices for users of fs/QCA. In response to this commentary, I would add that it is useful to distinguish three kinds of fs/QCA users: *implementers*, *innovators*, and *acquaintances*. *Implementers* are scholars who directly employ the technical apparatus of fs/QCA in their own work, but who do not pursue the methodological development of this approach. *Innovators* are scholars with training in methodology who are interested in refining and enhancing the toolkit that composes fs/QCA. *Acquaintances*

are scholars who are influenced by certain ideas offered in the fs/QCA literature, and who are broadly sympathetic to the approach, but who do not employ its technical apparatus in their own research. Ragin and Rihoux's discussion is especially relevant for implementers and innovators, but I would argue that more attention needs to be devoted to best practices for acquaintances–i.e., scholars who are interested in certain fs/QCA ideas, but who are not likely to actually employ the formal methodology in their own research. For these scholars, we need to explore how fs/QCA can be best used in a non-formal and non-technical mode. For example, what are the main implications of fs/QCA for the comparative-historical researcher who prefers to use only narrative and thus not formally code variables? Likewise, what does fs/QCA teach us about best practices for the scholar who may code a few variables, but who is unlikely to follow all of the steps needed to actually run the fs/QCA software? I believe that fs/QCA could have important implications for the research practices of these scholars, but to spell out these implications requires thinking more seriously about informal and indirect uses of fs/QCA.

Best Practices for Responding to Critics

Ragin and Rihoux do not discuss best practices for responding to critics of fs/QCA, but I would like to touch on the issue. I find it useful to distinguish three kinds of critics: *uninformed dismissers, informed skeptics*, and *critical innovators*. *Uninformed dismissers* are scholars who are not well informed about the assumptions and logic of fs/QCA, but who nevertheless reject the approach out of hand. These scholars may believe that any approach built around necessary and sufficient causation is inherently worthless; alternatively, they may believe that fs/QCA does not offer techniques that differ from mainstream statistical analysis. *Informed skeptics* are scholars who are knowledgeable about fs/QCA practices, but who nevertheless hold reservations concerning one or more of the approach's key assumptions. For example, some scholars conclude that, on balance, mainstream regression analysis simply offers a more powerful approach to causal analysis than fs/QCA. Finally, *critical innovators* are scholars who identify important problems with fs/QCA but are still motivated to suggest partial or full solutions for these problems. These critics may end up offering valuable contributions to the fs/QCA apparatus.

In terms of best practices, I would argue that fs/QCA scholars need to respond to uninformed dismissers in a dispassionate way that focuses on explaining the actual practices, logic, and assumptions of fs/QCA. These critics cannot be simply ignored, because they end up attracting attention with their summary dismissals of the approach; indeed, they help justify the skepticism and defensiveness that certain scholars trained in mainstream statistics may feel toward fs/QCA. Hence, fs/QCA advocates should patiently and unemotionally reply to these critics.

The informed skeptics and critical innovators may raise real challenges to fs/QCA, and advocates of fs/QCA need to avoid responding to them with a knee-jerk defensiveness. These knowledgeable critics ultimately help legitimate the fs/QCA enterprise by taking it seriously, and it is appropriate for fs/QCA advocates to engage in serious academic debate with them. It is possible that limitations of the fs/QCA approach will be exposed through such debate. And if there are serious problems with fs/QCA, certainly they should be out in the open. However, I believe it is more likely that once fs/QCA enters into a forum of serious and professional academic debate, it stands a good chance of making its case to open-minded methodologists.

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Replies to Commentators: Reassurances and Rebuttals

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We appreciate this opportunity to respond to our commentators. We confront two types: defensive reactions (Stanley Lieberson and Jason Seawright) and a constructive reaction (James Mahoney). We first address the two defensive reactions.

It is necessary to offer some reassurance before rebutting their criticisms, for both seem to be haunted by the prospect that QCA may replace their preferred techniques. Lieberson states that there is "no evidence" that QCA should "supplant existing practices" and voices his preference for quantitative techniques that allow for error and randomness (i.e., conventional methods such as regression analysis). Seawright, like Lieberson, is also concerned that QCA might replace conventional quantitative methods: ". . . a comparison of underlying assumptions provides a solid base for deciding whether one method [QCA] is currently well enough developed to replace a second method [regression analysis]." Our reassurance: *supplanting regression analysis and related techniques is not our goal, nor is it the goal of others who advocate QCA*. The defensive reactions are unwarranted.

First, a reminder: QCA was developed as a way to formalize case-oriented analysis and thereby provide tools to help case-oriented researchers improve their research. In the end, the goal of the application of QCA proper (i.e., the truth table algorithm) is to help researchers represent what they have learned about their cases. QCA both disciplines the conduct of case-oriented research and facilitates the comparison of cases as configurations. The primary device for both disciplining and facilitating case-oriented comparison is the truth table. The first phase of research using QCA involves the difficult and time-consuming process of constructing a useful truth table from empirical evidence, based on case comparisons. At the end of this demanding process, the resulting truth table should be as close to contradiction-free as is reasonable. Several factors influence the success of the researcher in addressing contradictions: the nature of the research question, the nature of the evidence, the strength of the researcher's in-depth case knowledge, and the state of existing theoretical and substantive knowledge, to name a few. Useful truth tables do not fall from the sky into the laps of researchers like data sets from ICPSR. Instead, they are crafted over a long period as researchers learn about their cases and deepen their knowledge by comparing cases as configurations. Analyzable truth tables are not the starting point of comparative research; rather, they are formed near the end of a long process of case-oriented comparative investigation.

These simple principles of case-oriented comparative research are well known to researchers who use QCA. Unfortunately, they appear to be foreign to both Lieberson and Seawright. Hence, their critiques do not address the central features of QCA.

Second, their critiques ignore the stark differences between the underlying logic and goals of techniques like regression analysis and QCA. For example, the application of QCA assumes a strong base of case-oriented knowledge, while the application of regression assumes a well-specified model based in theory and constructed in advance of any knowledge of patterns in the "data," at least according to the textbooks (see also King, Keohane, and Verba 1994). The application of QCA assumes a well-crafted summary of case-oriented configurational comparisons (i.e., a useful truth table), while regression analysis assumes only a data set with variables. Of course, with regression analysis it is best if the data set is a random sample drawn from a well-defined, well-delineated population and the variables are all normally distributed and measured without error. Another key difference: QCA seeks to identify combinations of causally relevant conditions linked to an outcome, while regression analysis seeks to estimate the net, independent effect of each causal variable, considered in analytic isolation from all other causal variables. Typically, users of regression analysis try to identify which single causes are the most important and view these assessments as tests of competing theories.

We could go on. Our main point is that we see very little value in comparing QCA with conventional techniques in the way that Lieberson and Seawright offer—as methods competing for the same turf. It should be clear from our essay and from related publications that no one has tried, or should try to use QCA to supplant existing methods such as regression analysis. After all, conventional methods focus primarily on the problem of estimating the net, independent effect of each causal variable included in an analysis on the outcome. This is very clearly *not* the goal of QCA, and it would be a serious mistake to apply QCA to this task. QCA focuses on *combinations* of causal conditions. From the perspective of QCA, the idea of isolating the net, independent, context-free effect of each independent variable makes no sense. Rather than trying to "supplant" existing practices, the usual recommendation offered by QCA advocates is to use *multiple* approaches (if the data permit), including conventional quantitative analysis, and then to examine the *differences* that follow from estimating net effects (the goal of conventional quantitative analysis) versus investigating the different combinations of conditions linked to an outcome (the goal of QCA).

We turn now to rebutting specific criticisms, focusing on Lieberson first. Our first task is to correct his misrepresentation of QCA.

Lieberson focuses primarily on what he calls the deterministic aspect of QCA. *Determinism* in this context means "no explicit error vector" or "no explicit use of probability theory" and has nothing to do with theorized determinism or theorized indeterminism. It appears that despite having reviewed *Fuzzy-Set Social Science* for *Contemporary Sociology*, Lieberson is unfamiliar with the extensive sections of the book devoted to the use

of probabilistic methods with both crisp-set and fuzzy-set QCA (Ragin 2000: 109-116, 132-137, 226-229, 249-252, 271-273, 279-281, 295-297). Furthermore, three out of the four applications of QCA presented in *Fuzzy-Set Social Science* incorporate probabilistic assessments. If he has misgivings about these published methods for integrating probabilistic criteria into QCA, he should have addressed them in his self-proclaimed "careful" review or in his current essay. He should refrain from asserting incorrectly and repeatedly that they don't exist.

Another problem arises in Lieberson's claim that there are "virtually no data-based comparisons between the proposed procedures and current practices." This statement simply betrays the fact that Lieberson has not looked at the literature on QCA. There are many such comparisons published in the more than250 applications of QCA, beginning with the very first application (Ragin, Mayer, and Drass 1984) and continuing to the present (e.g., Ragin, Shulman, Weinberg, and Gran 2003; Katz, vom Hau, and Mahoney forthcoming). Interested readers are invited to consult the extensive international bibliography of QCA applications available at http://www.compasss.org/Bibli%20-database.htm.

After presenting these misrepresentations of QCA, Lieberson offers three hypotheses. It should be clear from our opening paragraphs that we consider Lieberson's first two hypotheses about QCA irrelevant. His first hypothesis is that QCA cannot distinguish between random and real data. Because Lieberson apparently has not read the portions of *Fuzzy-Set Social Science* that address the incorporation of probabilistic criteria, he is, of course, completely unaware that these procedures would quickly distinguish between the two data sets. However, Lieberson's unfamiliarity with the book is not the important point here. Notice that both of his hypothetical data sets fall from the sky and are not constructed from case materials by investigators. Further, both sets, as described, would be full of unresolved contradictions. For these reasons, the two data sets would be irrelevant to QCA. As previously noted, with QCA it is the researcher's goal to resolve as many contradictions as is reasonable before analyzing a truth table. Indeed, this effort to resolve contradictions, which involves many iterations between observed cases and theoretical knowledge, is central to the spirit and the conduct of case-oriented comparative research.

Lieberson's second hypothesis is that if cases' values on causal conditions were randomly re-assigned while holding the marginal distributions constant, QCA would still produce a full account. What Lieberson does not seem to understand is that this random re-assignment of values would automatically generate a wealth of contradictions in the truth table constructed from these data. These contradictions would have to be addressed in some way before the application of QCA could proceed. The process of connecting back to the cases in order to resolve contradictions would simply expose the fact that many coding errors had been introduced. Thus, Lieberson's second hypothesis also misses both the point and the target.

Lieberson's third hypothesis is that the random assignment of 40 cases to two groups of 20 would yield QCA results that differ substantially from each other. Of course, this outcome would probably hold for quantitative analysis of these data, especially given the substantial influence that a single case may have in a small-N quantitative analysis. Because Lieberson's hypothesis probably would hold for conventional quantitative analysis, it is quite reasonable to speculate that it would also hold for QCA. But what would happen?

Assume a well-constructed truth table (i.e., contradictions have been resolved in one way or another) with 40 cases (truth table #1). Assume that its forty cases are randomly split into two groups of 20 and separate analyses using QCA are performed on the two resulting truth tables (#2a and #2b). The positive cases from #1 would be distributed to identical positive rows in tables #2a and/or #2b. The same is true for the negative cases; they would distributed to identical negative rows in tables #2a and/or #2b. In effect, after random assignment the positive rows in tables#2a and #2b would constitute subsets of the positive rows in table #1, and the negative rows in tables #2a and #2b would constitute subsets of the negative rows in table #1. Thus, both tables #2a and #2b would be likely to have more "remainder" rows (combinations of conditions lacking empirical cases) than table #1.

What happens when we apply QCA to tables #2a and #2b? Without simplifying assumptions, the results of the analyses of tables #2a and #2b will be subsets of the results of the analysis of table #1. Because more truth table rows in #2a and #2b lack cases ("remainders"), the specification of the solution will be narrower (more complex) when no simplifying assumptions are made. With simplifying assumptions, the reverse will be true. That is, the solution for table #1 will be a subset of the solution for table #2a and a subset of the solution for table #2b. In short, the solutions of tables #2a and #2b will have more complexity without simplifying assumptions and more parsimony with simplifying assumptions than the solution of table #1. With fewer combinations covered in the truth table (which would likely follow from dividing the cases in half), the complexity/parsimony dimension lengthens in both directions. Thus, the range of solutions for table #1 would be contained within the range of solutions for both tables #2a and #2b. While there is certainly no guarantee that the "intermediate" solutions (as discussed in our essay) would be identical across the three analyses, it is a distinct possibility, given the important role of substantive and theoretical knowledge in specifying intermediate solutions. In any event, these three sets of results would be in the same general family because of the set-theoretic relations among them. This outcome illustrates an important principle of QCA (and of configurational comparative analysis in general): having evidence on a greater diversity of configurations of conditions enables greater specificity in statements about relevant combinations of conditions.

At the end of his essay, Lieberson states that he has used truth tables to implement one of the five possible applications of QCA listed in our essay. We believe that his application was well within the spirit of the approach and in fact have cited it as a model for others to follow. Also, like Lieberson, we believe that it is a mistake to try to "account for everything." Almost every application of QCA involves the identification of cases, sometimes whole categories of cases that "don't fit." Likewise, there are almost always contradictions that cannot be reasonably

resolved. In applications of QCA that incorporate probabilistic criteria, these cases are shuffled into error vectors, just as they are in conventional quantitative analysis. More often, they are identified as cases that "don't fit" and are set aside for later examination as products of more or less extraneous or haphazard forces, outside the scope of the theories guiding the investigation. Our point is that QCA in practice conforms reasonably well to a principle that Lieberson and many others rightly hold dear: researchers should employ suitable allowances for error and randomness.

We turn next to Jason Seawright's essay, which is part of a paper that he is publishing in *Studies in Comparative International Development*. This version has the virtue of being more accessible. As we have already indicated, it is a mistake to compare QCA and conventional methods such as regression analysis as competitors for the same turf, but this is exactly Seawright's goal. His comparison is organized around three assumptions.

The first assumption he addresses is that of correct "functional form." He finds the two approaches similar. He is superficially correct, but he is mixing apples and oranges. For a good illustration of this, simply study James Mahoney's Figures 2 and 3 and the discussion surrounding these figures in his excellent essay in this issue.

The second is "omitted variables," and he finds QCA comes up short compared to regression analysis. It is true that an omitted variable will have a more powerful impact on the results of any configurational analysis than on the results of an analysis that seeks to isolate the net, independent impact of each causal variable. Still, we ask, which analysis is more likely to have omitted variables: one that is built up from case knowledge and explicit case-oriented configurational comparisons (QCA), or one that prefers that the "causal model" be fully and correctly specified in advance of any knowledge of patterns in the data set (regression analysis)? The process of constructing a useful truth table addresses the problem of omitted variables. It does not solve it, of course, but we believe that it is a problem that is best addressed through deeper and wider substantive knowledge.

Seawright's third assumption concerns the difficult connection between association and causation. He rightly criticizes both QCA and regression analysis for making causal claims based on associations. However, he does seem to give QCA a slight edge because of its greater potential for incorporating "outside information." Our responses: first, in our essay we emphasize the idea of using set-theoretic methods to establish "explicit connections," which can be made without causal interpretation (as they often are in qualitative research). Second, in the end, true causal interpretation can transpire only at the case level. A hallmark of QCA is that is requires researchers to get in touch with their cases. In this way, it directly facilitates causal analysis.

Finally, our reply to James Mahoney can be stated succinctly: Thank you for filling in several of the many gaps in our essay. Indeed, our two essays read well together and complement each other. The only real difference is one of emphasis. We worry that some researchers, especially those who are "more qualitative" or "more interpretive," are uncomfortable with the language of necessary and sufficient causation (or with any language of causation, for that matter). Thus, in our essay we use the more encompassing concept of *explicit connection* and try to illustrate both its fundamentally set-theoretic nature and its centrality to qualitative inquiry. As Mahoney capably demonstrates, however, the concepts of necessity and sufficiency can be usefully applied in QCA research, which is why they are addressed in detail in *Fuzzy-Set Social Science* (Ragin 2000). Further, these two concepts are important in the work of many scholars in both comparative politics and international relations. Thus, it is valuable to have two perspectives on QCA and causation side-by-side in this issue of *Qualitative Methods*: our emphasis on "explicit connections" (causal language optional) and Mahoney's emphasis on necessity and sufficiency (causal language conspicuous).

In conclusion, we believe that an informed debate about the merits and limitations of QCA requires a firm grasp of the foundations of QCA and also its specific goals, especially if it is to be confronted with the merits and limitations of mainstream statistical techniques. The two methods have different foundations and pursue different goals. In a way, it is unfortunate that many statistical methods allow researchers to obtain "solutions" so easily. With QCA it is difficult to reach a "solution" because the researcher will first face the important task of addressing contradictions. From the perspective of case-oriented research, this difficulty of finding easy "solutions" is a virtue. QCA calls for a much more demanding confrontation with data—not with "distant" data, but with real-life, thick, complex cases. So we come back to our first fundamental point: QCA is really about comparing cases, not about "data" one can play with mechanically.

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Response to Reassurances and Rebuttals

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I do not have the space to go into greater details, but my review essay of Ragin's *Fuzzy-Set Social Science* (and a later letter in *Comparative Sociology*) cites specific efforts to compare the results obtained using these proposed methods with more conventional ones. In many cases, the "comparisons" are not comparisons. For example, conventional ways of analyzing race riots are compared with research on an elementary school in St. Louis. Another is a comparison between an analysis of IMF protests based on earlier methods with fuzzy-set methods. Again, they are not comparable because the latter truncates the dependent variable to only include the highest values. To be sure, Ragin claims to use a probabilistic approach to his central usage of such concepts as necessary conditions, sufficient conditions, and necessary and sufficient conditions. I noted in my review that using probabilistic forms, such as "almost always necessary" or "almost always sufficient" and so forth, is both an oxymoron and a step towards conventional forms of probabilistic analysis. You cannot be probabilistic and at the same time employ the central concepts of QCA. I also recognize that QCA would incorporate a data set consisting of 40 cases. It would certainly end up fitting the data. My proposal about using two separate randomly-drawn sets of 20 cases is to display the unstable nature of the method. A more conventional approach would not over interpret every single feature and hence such conclusions as are drawn from the first set have a much greater chance to also be supported in the second set. Finally, to assume that my criticism of QCA is made in order to defend the existing methods is nothing more than an example of the fallacy of the consequence. If QCA has any value, it does not need fallacious arguments in its defense.

Book Notes

Amadae, S.M. 2003. *Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism*. Chicago: University of Chicago Press. $19.00 paperback.

From economics to politics, law to journalism, rational choice theory dominates debates about how individuals make decisions. But it wasn't always this way. In *Rationalizing Capitalist Democracy*, Amadae tells the remarkable story of how rational choice theory rose from obscurity to become the intellectual bulwark of capitalist democracy. He roots *Rationalizing Capitalist Democracy* in the turbulent post-World War II era, showing how rational choice theory grew out of the RAND Corporation's efforts to develop a "science" of military and policy decision making. While the first generation of rational choice theorists—William Riker, Kenneth Arrow, and James Buchanan—were committed to constructing a "scientific" approach to social science research, they were also deeply committed to defending American democracy from its Marxist critics. Amadae reveals not only how the ideological battles of the Cold War shaped their ideas but also how those ideas may today be undermining the very notion of individual liberty they were created to defend.

Behnegar, Nasser. 2003. *Leo Strauss, Max Weber, and the Scientific Study of Politics*. Chicago: University of Chicago Press. $30.00 paperback.

Can politics be studied scientifically, and if so, how? Assuming it is impossible to justify values by human reason alone, social science has come to consider an unreflective relativism the only viable basis, not only for its own operations, but for liberal societies more generally. Although the experience of the sixties has made social scientists more sensitive to the importance of values, it has not led to a fundamental reexamination of value relativism, which remains the basis of contemporary social science. Almost three decades after Leo Strauss's death, Nasser Behnegar offers the first sustained exposition of what Strauss was best known for: his radical critique of contemporary social science, and particularly of political science. Behnegar's impressive book argues that Strauss was not against the scientific study of politics, but he did reject the idea that it could be built upon political science's unexamined assumption of the distinction between facts and values. Max Weber was, for Strauss, the most profound exponent of values relativism in social science, and Behnegar's explication artfully illuminates Strauss's critique of Weber's belief in the ultimate insolubility of all value conflicts. Strauss's polemic against contemporary political science was meant to make clear the contradiction between its claim of value-free premises and its commitment to democratic principles. As Behnegar ultimately shows, values—the ethical component lacking in a contemporary social science—are essential to Strauss's project of constructing a genuinely scientific study of politics.

Bevir Mark and R. A. W. Rhodes. 2003. *Interpreting British Governance.* London: Routledge. $30.95 paperback.

How should we study government? How do we know what we know about British government? What is governance? Governance in Britain has changed enormously from the market-led Thatcher years to Blair's "joined up government." This book, written by two leading scholars, is a major contribution to our understanding of the theoretical approaches to governance and of how British politics actually works in practice. The authors provide a radical challenge to traditional accounts of British politics, such as the Westminster model, and instead offer a highly original interpretive approach.

Geddes, Barbara. 2003. *Paradigms and Sand Castles: Theory Building and Research Design in Comparative Politics*. Ann Arbor: University of Michigan Press. $20.00 paperback.

Paradigms and Sand Castles describes in a lively and provocative manner the methodological pitfalls most characteristic of the study of developing countries. Although much of this book focuses on the concrete details of empirical research, its primary goal is to aid theory building. Its central message is that theoretical knowledge could be accumulated more rapidly if certain research norms in the field of comparative development were changed. The book deals with several standard methodological practices, showing their unfortunate consequences and making suggestions for their improvement. Traditionally, big subjects of immense real-world importance, such as democratization, economic development, and ethnic mobili-

zation, have fascinated scholars of comparative politics. But the choice of a big question for study does not translate automatically into a feasible research design. Barbara Geddes offers a strategy for approaching big questions and addresses empirical issues such as effective use of evidence found in case studies to test arguments, the intricacies of operationalizing complicated and often "fuzzy" concepts, the nonquantitative "measurement" of such concepts, and determining the criteria for setting the boundaries of the domain within which an argument should apply.

Holmes, Jennifer S. (ed). 2003. *New Approaches to Comparative Politics: Insights from Political Theory*. Lanham, MD: Lexington Books. $55.00 hardcover.

The field of comparative politics traditionally has been divided into two camps: on the one hand, quantitatively driven work on a broad scale; on the other, more qualitative area studies. This edited collection promotes a new approach to comparative politics: theoretically informed comparative analysis. The contributors' essays are innovative in their interweaving of case studies of the political situations in particular regions (from the United States to Colombia to Mali) with the project of political theorizing.

James, Patrick. 2002. *International Relations and Scientific Progress: Structural Realism Reconsidered.* Columbus: Ohio State University Press. $24.95 paperback.

International Relations and Scientific Progress contends that a theory focusing on the structure of the international system explains a wider and more interesting range of events in world politics than other theories. Such theorizing appears to be out of favor as the result of the apparent failure by structural realism, the most prominent system-level theory over the last two decades, on any number of fronts—most notably an inability to anticipate the ending of the Cold War and its aftermath. This new book is put forward as the most comprehensive and innovative theoretical work on paradigms in international relations since the publication of *Theory of International Politics*, which created structural realism, more than two decades ago. With appropriate revisions, however, structural realist theory can compete effectively and reclaim its primacy. The first part of *International Relations and Scientific Progress* assesses the meaning of progress in the discipline of international relations, a process that culminates in the creation of a new concept, the scientific.

Lebow, Richard Ned. 2003. *The Tragic Vision of Politics: Ethics, Interests, and Orders*. Cambridge: Cambridge University Press. $27.00 paperback.

Is it possible to preserve national security through ethical policies? Richard Ned Lebow seeks to show that ethics are actually essential to the national interest. Recapturing the wisdom of classical realism through a close reading of the texts of Thucydides, Clausewitz, and Hans Morgenthau, Lebow argues that, unlike many modern realists, classic realists saw close links between domestic and international politics, and between interests and ethics. Lebow uses this analysis to offer a powerful critique of post-Cold War American foreign policy. He also develops an ontological foundation for ethics and makes the case for an alternate ontology for social science based on Greek tragedy's understanding of life and politics. This is a topical and accessible book, written by a leading scholar in the field.

Mansfield, Edward D. and Richard Sisson (eds). 2004. *The Evolution of Political Knowledge: Democracy, Autonomy and Politics in Comparative and International Politics*. Columbus: Ohio State University Press. $24.95 paperback.

Over the course of the last century, political scientists have been moved by two principal purposes. First, they have sought to understand and explain political phenomena in a way that is both theoretically and empirically grounded. Second, they have analyzed matters of enduring public interest, whether in terms of public policy and political action, fidelity between principle and practice in the organization and conduct of government, or the conditions of freedom, whether of citizens or of states. Many of the central advances made in the field have been prompted by a desire to improve both the quality and our understanding of political life. Nowhere is this tendency more apparent than in research on comparative politics and international relations, fields in which concerns for the public interest have stimulated various important insights. This volume systematically analyzes the major developments within the fields of comparative politics and international relations over the past three decades. Each chapter is composed of a core paper that addresses the major puzzles, conversations, and debates that have attended major areas of concern and inquiry within the discipline. These papers examine and evaluate the intellectual evolution and "natural history" of major areas of political inquiry and chart particularly promising trajectories, puzzles, and concerns for future work. Each core paper is accompanied by a set of shorter commentaries that engage the issues it takes up, thus contributing to an ongoing and lively dialogue among key figures in the field.

Monroe, Kristen Renwick (ed). 1997. *Contemporary Empirical Political Theory*. Berkeley: University of California Press. $21.95 paperback.

How can we best understand the major debates and recent movements in contemporary empirical political theory? In this volume, the contributors, including four past presidents of the APSA and one past president of the IPSA, present their views of the central core, methodologies, and development of empirical political science. Their disparate views of the unifying themes of the discipline reflect different theoretical orientations, from behavioralism to rational choice, cultural theory to postmodernism, and feminism to Marxism. Is there a human nature on which we can construct scientific theories of political life? What is the role of culture in shaping any such nature? How objective and value-free can political theories be? These are only a few of the issues the volume addresses. By assessing where we have traveled intellectually as a discipline and asking what remains of lasting significance in the various theoretical approaches that have engulfed the profession, *Contemporary Empirical Political Theory* provides an important evaluation of the current state of empirical political theory and a valuable guide to future developments in political science.

Orren, Karen and Stephen Skowronek. 2004. *The Search for American Political Development.* Cambridge: Cambridge University Press. $23.99 paperback.

In recent years, American political development has claimed the attention of a growing band of political scientists, and scholars have begun to speak of "APD" as a subfield within the discipline. This book provides a justification for studying politics historically, not only for what it reveals about the roots of political affairs at the present time but what it teaches about politics as an ongoing activity in time, anytime. Placing the character of political institutions at

the center of analysis, Orren and Skowronek survey past and current scholarship and attempt to outline a course of study for the future.

Ostrow, Joel. 2000. *Comparing Post-Soviet Legislatures: A Theory of Institutional Design and Political Conflict.* Columbus: Ohio State University Press. $28.95 paperback.

One dilemma facing new or newly independent states, such as those of the former Soviet Union, is how to design effective legislatures when political parties are weak and fragmented or even nonexistent. In this book, Joel M. Ostrow develops a comparative institutional framework to explain marked differences in behavior across three post-Soviet legislatures: the Russian Supreme Soviet, the Russian State Duma, and the Estonian legislature. He argues that these differences in ability to manage political conflict can be explained in large measure by the design of the legislatures. Most significant is the choice of whether and how to include parties or partisan factors. Legislatures are an omnipresent component of modern democracy. Ostrow's comparative institutional design confirms the presumption of many political scientists that parties are essential for legislative consensus building. However, it also reveals that parties may have paradoxical effects on a legislature's performance. In following the budget process in Russia and Estonia, Ostrow explores the consequences that different institutional designs have had on post-Soviet legislatures. He has found that how parties are included may determine how capable a legislature is at managing political conflict, both internally and with the executive branch of the government. The author was present at many of the committee hearings and legislative sessions he describes, and his firsthand observations and interviews with committee members enhance his analysis.

Pierson, Paul. 2004. *Politics in Time: History, Institutions, and Social Analysis.* Princeton: Princeton University Press. $14.95 paperback.

This groundbreaking book represents the most systematic examination to date of the often-invoked but rarely examined declaration that "history matters." Most contemporary social scientists unconsciously take a "snapshot" view of the social world. Yet the meaning of social events or processes is frequently distorted when they are ripped from their temporal context. Paul Pierson argues that placing politics in time—constructing "moving pictures" rather than snapshots—can vastly enrich our understanding of complex social dynamics, and greatly improve the theories and methods that we use to explain them. *Politics in Time* opens a new window on the temporal aspects of the social world. It explores a range of important features and implications of evolving social processes: the variety of processes that unfold over significant periods of time, the circumstances under which such different processes are likely to occur, and above all, the significance of these temporal dimensions of social life for our understanding of important political and social outcomes. Ranging widely across the social sciences, Pierson's analysis reveals the high price social science pays when it becomes ahistorical. And it provides a wealth of ideas for restoring our sense of historical process. By placing politics back in time, Pierson's book is destined to have a resounding and enduring impact on the work of scholars and students in fields from political science, history, and sociology to economics and policy analysis.

Shapiro, Ian, Rogers M. Smith and Tarek E. Masoud, eds. 2004. *Problems and Methods in the Study of Politics*. Cambridge: Cambridge University Press. $27.00 paperback.

The study of politics seems endlessly beset by debates about method. At the core of these debates is a single unifying concern: should political scientists view themselves primarily as scientists, developing ever more sophisticated tools and studying only those phenomena to which such tools may fruitfully be applied? Or should they instead try to illuminate the large, complicated, untidy problems thrown up in the world, even if the chance to offer definitive explanations is low? Is there necessarily a tension between these two endeavors? Are some domains of political inquiry more amenable to the building up of reliable, scientific knowledge than others, and if so, how should we deploy our efforts? In this book, some of the world's most prominent students of politics offer original discussions of these pressing questions, eschewing narrow methodological diatribes to explore what political science is and how political scientists should aspire to do their work.

Sperling, Valerie. 1999. *Organizing Women in Contemporary Russia: Engendering Transition.* Cambridge: Cambridge University Press. $27.00 paperback.

This book offers the first comprehensive analysis of the contemporary Russian women's movement and of the social, political, economic, historical, and international contexts that surround it. Valerie Sperling paints a vivid portrait of the women's movement's formation and development, paying particular attention to the key challenges facing a social movement in post-communist society, including the virtual absence of civil society, constant flux in political institutions, wrenching economic changes, and the movement's own status in a changing transnational environment. The author also addresses the specific challenges facing women's organizations by discussing societal attitudes towards feminism in Russia. Based on participant observation, primary source materials, and dozens of interviews conducted in Moscow (as well as two smaller Russian cities), the narrative brings alive the activists' struggle to build a social movement under difficult conditions, and sheds new light on the troubled and complex process of Russia's democratization.

Sprinz, Detlef F. and Yael Wolinsky-Nahmias (eds). 2004. *Models, Numbers, and Cases: Methods for Studying International Relations*. Ann Arbor: University of Michigan Press. $27.95 paperback.

Scholars and students of international relations must contend with increasingly sophisticated methods for studying world politics. *Models, Numbers, and Cases* is a thorough assessment by the world's leading specialists of the three main approaches in international relations: case study, quantitative methods and formal methods. Clearly written chapters address the most important methodological and theoretical issues in the field today, and demonstrate the practical application of these methods to three key subfields: international political economy, environmental policy, and security. The result is a model of rigorous methodological study and exacting substantive research. *Models, Numbers, and Cases* is a concise and valuable guide to the challenging terrain of contemporary international relations study.

Wilensky, Harold L. 2002. *Rich Democracies: Political Economy, Public Policy, and Performance*. Berkeley: University of California Press. $45.00 paperback.

In this landmark work, the culmination of 30 years of systematic, comprehensive comparison of 19 rich democracies, Wilensky answers two basic questions: (1) What is distinctly modern about modern societies—in what ways are they becoming alike? (2) How

do variations in types of political economy shape system performance? He specifies similarities and differences in the structure and interplay of government, political parties, the mass media, industry, labor, professions, agriculture, churches, and voluntary associations. He then demonstrates how differences in bargaining arrangements among these groups lead to contrasting policy profiles and patterns of taxing and spending, which in turn explain a large number of outcomes: economic performance, political legitimacy, equality, job security, safety and risk, real health, the reduction of poverty and environmental threats, and the effectiveness and fairness of regulatory regimes. Drawing on quantitative data and case studies covering the last 50 years and more than 400 interviews he conducted with top decision-makers and advisors, Wilensky provides a richly detailed account of the common social, economic, and labor problems modern governments confront and their contrasting styles of conflict resolution. The result is new light on the likely paths of development of rich democracies as they become richer. Assessing alternative theories, Wilensky offers a powerful critique of such images of modern society as "post-industrial" or "high-tech," "the information age," or the alleged dominance of "globalization." Because he systematically compares all of the rich democracies with at least three million population, Wilensky can specify what is truly exceptional about the United States, what it shares with Britain and Britain abroad (Canada, Australia, New Zealand), and what it shares with all or almost all of the West European democracies, Israel, and Japan. He gives careful attention to which successful social and labor policies are transferable across nations and which are not. *Rich Democracies* will interest both scholars and practitioners. It combines the perspectives of political economy (the interplay of markets and politics) and political sociology (the social bases of politics). It will be especially useful in courses on comparative political economy, comparative politics, European politics, public policy, political sociology, the welfare state, American government, advanced industrial societies, and industrial relations.

Article Notes

Bevir, Mark, R. A. W. Rhodes, and Patrick Weller. 2003. "Traditions of Governance: History and Diversity," *Public Administration* Vol. 81, Issue 1 (Spring)

This journal issue consists of an attempt to use Bevir and Rhodes interpretive approach to illuminate the comparative study of governance. It includes essays on the United States by Richard Stillman, Britain by Bevir and Rhodes, Australia by John Walla and Patrick Weller, Germany by Werner Jann, the Netherlands by Walter Kickert, France by Robert Elgie, and Norway by Thomas Christensen, as well as a more methodological introduction and conclusion by Bevir, Rhodes, and Weller. The role of the state is changing under the impact of, for example, globalization. The changes have been variously understood as the new public management (NPM), the hollowing-out of the state and the new governance. This special issue of *Public Administration* explores the changing role of the state in advanced industrial democracies. It focuses on the puzzle of why states respond differently to common trends.

Büthe, Tim. 2002. "Taking Temporality Seriously: Modeling History and the Use of Narratives as Evidence," *American Political Science Review* 96:3 (September): 481-494.

Social scientists interested in explaining historical processes can, indeed should, refuse the choice between modeling causal relationships and studying history. Identifying temporality as the defining characteristic of processes that can be meaningfully distinguished as "history," I show that modeling such phenomena engenders particular difficulties but is both possible and fruitful. Narratives, as a way of presenting empirical information, have distinctive strengths that make them especially suited for historical scholarship, and structuring the narratives based on the model allows us to treat them as data on which to test the model. At the same time, this use of narratives raises methodological problems not identified in recent debates. I specify these problems, analyze their implications, and suggest ways of solving or minimizing them. There is no inherent incompatibility between—but much potential gain from—modeling history and using historical narratives as data.

Dallmayr, Fred. 2004. "Beyond Monologue: For a Comparative Political Theory," *Perspectives on Politics* 2: 2 (June): 249-257.

The essay advances a proposal that is addressed primarily to theorists, but with implications for the entire profession: the proposal to replace or supplement the rehearsal of routinized canons with a turn to global, cross cultural (or "comparative") political theorizing. I offer geopolitical and general intellectual reasons why the turn seems appropriate today, and I discuss a variety of theoretical or philosophical inspirations undergirding the turn. After highlighting some recent examples of comparative political theorizing, I conclude by responding to critical queries as well as indicating broader implications of the move "beyond monologues."

Finlayson, Alan, Mark Bevir, R. A. W. Rhodes, Keith Dowding, and Colin Hay. 2004. "The Interpretive Method in Political Science," *The British Journal of Politics and International Relations* Vol. 6, No. 2 (May): 129-64.

One might perhaps expect to find interpretive methodology occupying a strong and secure place in British political science. With its origins in philosophy and history, British political studies never fully or exclusively embraced behaviorism or subsequent positivist methodologies. Interpretivism represents the major alternative for social science in which, as Weber declared, "we are concerned with mental phenomena the empathic 'understanding' of which is naturally a task of a specifically different type from those which the schemes of the exact natural sciences in general can seek to solve" (quoted in Giddens 1971: 146). But interpretivism does not have a secure footing in British political studies. The systematic explication of political phenomena through interpretive concepts and methods, despite some notable attempts and a growing intensity of focus, is still at an early stage of development. In the academic study of contemporary British government interpretivism is not at all widespread.

Are things about to change? Recent methodological disputes in British political science map on to broader debates about how the structure and process of British government might or might not be changing. If we are in the midst of a shift from top-down "command and control" to a looser framework of "governance," then the time of interpretivism may well have arrived. But methodological arguments are always about more than method. They map on to and can define broader debates concerning what government is, how it works and whether or not it is changing. The publication of Mark Bevir and Rod Rhodes *Interpreting British Governance* (2003), which advocates and demonstrates the analysis of governance using interpretive theories and methods, is a significant development in these debates. They map on to and can define broader debates concerning what government is, how it works and whether or not it is changing.

Haas, Peter and Ernst B. Haas. 2002. "Pragmatic Constructivism and the Study of International Institutions." *Millennium: Journal of International Studies* 31:3, 573-601.

This article provides a pragmatic constructivist approach for progressing study in International Relations (IR) that sidesteps the ontological differences between major IR approaches, and that is capable of influencing practices in international relations. In particular, it looks at how international institutions can be studied and the possible consequences of how they are studied. While institutions are at times, as realists and neoliberal institutionalists contend, merely the artifacts of strategically and rationally motivated state actors, they are viewed differently by pragmatic constructivists. Institutions may, at times, be willful actors on their own, but are also the venue in which reflexive new practices and policies develop. Pragmatic constructivism provides the explanatory lens through which this may be understood, as well as the methodological guidelines by which such a process may be pursued.

Lieberman, Evan. 2001. "Causal Inference in Historical Institutional Analysis: A Specification of Periodization Strategies" *Comparative Political Studies* 34: 9 (November): 1011-1035.

Although emerging streams of historical institutional (HI) analysis have generated substantial insights in the field of comparative politics, this scholarship has lacked a self-conscious approach to methodology. This article specifies the comparative historical methods that many HI scholars have implicitly used for estimating the causal effect of political institutions on key policy and other political outcomes. It demonstrates how various periodization strategies are deployed to sort out the influence of a host of hypothesized and rival explanatory factors. In addition to explicating these methods, the article critically examines recent works of HI scholarship, highlighting the analytical leverage generated through studies that might ordinarily seem to suffer from the problem of small samples. More explicit deployment of these methods would both improve the quality of HI analysis and make its findings more transparent for further evaluation and emulation.

Lieberman, Evan. 2002. "Taxation Data as Indicators of State-Society Relations: Possibilities and Pitfalls in Cross-National Research" *Studies in Comparative International Development* 36:4 (Winter), 89-115.

Cross-national research on taxation is a growth industryin political science. This article discusses key conceptual and measurement issues raised by such studies. First, it highlights the ways in which taxation has been studied as a rich and varied concept, including as a component of the state-building process, as a collective action problem, and/or as a problem of distributive justice. Second, the article identifies the central tradeoffs associated with the construction of taxation indicators used to measure such ideas. It discusses considerations such as which forms of revenues should be included and which should not, whether and how to standardize taxation measures, and how to fine-tune measures through a clear specification of units, universes, and measurement calibration. These choices have important implications for the "scoring" of countries, and for making valid inferences about the relationship between states and societies.

Ostrow, Joel M. 2002. "Conflict Management in the Russia's Federal Institutions," *Post-Soviet Affairs* 18: 1 (January-March): 49-70.

Ostrow examines factors leading to instability in the Russian legislative and executive branches. He focuses on the design of Russia's "unlinked dual-channel" institutions—particularly partisan factions and legislative committees in the Duma and the presidential administration and government apparatus in the executive branch. Media sources and the author's personal interviews with Duma, Kremlin, and ministry officials are analyzed.

Ostrow, Joel M. 2001. "Chaos in Russian Budgeting as a Product of Institutional Design: The Failure of Unlinked Dual-Channel Institutions," *Journal of Public Budgeting, Accounting and Financial Management* 13: 4 (2001): 624-652.

Chaos in the Russian federal budget process is largely a product of institutional design. Both the Russian State Duma, and the Russian executive branch are unlinked dual-channel institutions. Such institutions are particularly ill-designed for the task of creating, adopting and implementing consistent policy, most notably on the budget. The strange design of Russia's legislative and executive institutions impedes conflict management and consensus building. Ultimately, it impeded the consolidation of democracy. This article elaborates the pitfalls of Russia's institutions for the budget process and suggests simple design changes that could substantially alleviate those problems.

Sambanis, Nicholas. 2004. "Using Case Studies to Expand Economic Models of Civil War," *Perspectives on Politics* 2: 2 (June): 259-279.

This article draws on a comparative case study design to refine formal-quantitative models of civil war, expanding them to highlight political processes that lead to civil war. It uses 21 case studies of civil war onset and avoidance to show the shortcomings in prominent rationalist models of civil war that rely heavily on economic variables. These shortcomings include measurement error, unit heterogeneity, model misspecification, and lack of clarity about causal mechanisms. Additionally, the greed/grievance distinction that underlies the economic models is misguided. This article analyzes civil war not as a discrete phenomenon, but rather as one phase in a cycle of violence. Economic models of civil war, however, rely on theories that cannot distinguish effectively between civil war and other forms of political violence. To explain civil war, we must explain why various and often conflicting micro-level motives combine to produce political violence with the characteristics that we attribute to civil war. If we cannot understand why we get civil war instead of other forms of organized political violence, then we do not understand civil war.

Sekhon, Jasjeet S. 2004. "Quality Means Quantity: Case Studies, Conditional Probability, and Counterfactuals," *Perspectives on Politics* 2: 2 (June): 281-293.

Case study research methods often contrast sharply with statistical methods. Methods which only consider deterministic relationships should not be used because serious inferential errors can be avoided if the basic lessons of statistical inference are heeded. Of particular importance is the use of conditional probabilities to compare relevant counterfactuals. A number of case study methods collectively referred to as Mill's methods have been used by generations of social science researchers and they contrast sharply with statistical

methods. A prominent example of work using these methods is Skocpol's *States and Social Revolutions*. Geddes' widely assigned critique of Skocpol's claim of a causal relationship between foreign threat and social revolution is valid if this relationship is considered to be deterministic. If, however, we interpret Skocpol's hypothesized causal relationship to be probabilistic, Geddes' data support Skocpol's hypothesis. But Skocpol failed to provide the data necessary to compare conditional probabilities. Also, for causal inference conditional probabilities are of interest only insofar as they provide information about relevant counterfactuals.

Snyder, Richard. 2001. "Scaling Down: The Subnational Comparative Method," *Studies in Comparative International Development* 36: 1 (Spring): 93-110.

Subnational units of analysis play an increasingly important role in comparative politics. Although many recent studies of topics such as ethnic conflict, economic policy reform, and democratization rely on comparisons across subnational political units, insufficient attention has been devoted to the methodological issues that arise in the comparative analysis of these units. To help fill this gap, this article explores how subnational comparisons can expand and strengthen the methodological repertoire available to social science researchers. First, because a focus on subnational units is an important tool for increasing the number of observations and for making controlled comparisons, it helps mitigate some of the characteristic limitations of a small-N research design. Second, a focus on subnational units strengthens the capacity of comparativists to accurately code cases and thus make valid causal inferences. Finally, subnational comparisons better equip researchers to handle the spatially uneven nature of major processes of political and economic transformation.

Symposium: Two Paths to a Science of Politics. 2004. *Perspectives on Politics* 2: 2 (June): 295-323.
Granato, Jim and Frank Scioli. "Puzzles, Proverbs, and Omega Matrices: The Scientific and Social Significance of Empirical Implications of Theoretical Models (EITM)," 313-323.
Smith, Rogers M. "Identities, Interests, and the Future of Political Science," 301-312.

"Two Paths to a Science of Politics" presents two strongly argued points of view about where we should be going in our discipline. The authors by and large agree on the nature of science and the variety of ways to do it, but they disagree on the most fruitful way to proceed at this moment. Granato and Scioli believe that we need to do more to test theories with data and to formulate theories that are based on data. We need, in their phrase, to consider the empirical implications of theoretical models (EITM). Rogers Smith thinks we need to take history, context, and meaning more seriously in order to understand phenomena such as political identity.

Announcements

Giovanni Sartori Award

This award is granted for the best book developing or applying qualitative methods published in the previous calendar year. It honors Giovanni Sartori's innovative research on social science concepts and his leading role in developing the field of concept analysis as a component of political science methodology.

Recipients: James Mahoney and Dietrich Rueschemeyer, eds., *Comparative Historical Analysis in the Social Sciences* (Cambridge University Press, 2003).

Committee: Gerardo Munck, University of Southern California; Jack Snyder (chair), Columbia University; and Nina Tannenwald, Brown University.

Citation: James Mahoney and Dietrich Rueschemeyer's edited volume offers a bold, encompassing, and optimistic statement regarding the potential of a research tradition—comparative historical analysis—that relies fundamentally on qualitative research. It presents a synthesis of the achievements made in addressing various substantive questions: revolutions, social policy, and regimes and democracy. It tackles ongoing debates on issues of theory and method, offering insightful analyses regarding the study of temporality, institutions, social structure, and the status and uses of case studies and small-N designs. Finally, it offers programmatic statements that seek to orient future research. Each and every contribution is a carefully crafted piece of scholarship. And the whole is greater than the sum of the parts. In short, Mahoney and Rueschemeyer have presented us with a book that clarifies the aims and means of qualitative research, and that raises the standards of what we will come to expect from qualitative researchers in the years ahead. This book is a deserving recipient of the first Giovanni Sartori Book Award of the qualitative methods section of APSA.

Alexander George Award

This award is granted for the best article or book chapter developing or applying qualitative methods, published in the previous calendar year. This award honors Alexander George's prominent role in developing and teaching qualitative methodology, in particular the comparative case study method.

Recipient: Peter A. Hall, "Aligning Ontology and Methodology in Comparative Research," in James Mahoney and Dietrich Rueschemeyer, eds., *Comparative Historical Analysis in the Social Sciences* (Cambridge University Press, 2003).

Committee: David Dessler, William and Mary; Julia Lynch, Harvard University; and Daniel Kryder (chair), Brandeis University.

Citation: This is a particularly fortuitous choice for this section's first Alexander George Award, in that it both recommends and deploys systematic and theoretically aware historical and sequential analysis: process analysis, or process tracing. Hall deftly and efficiently reviews the evolution of the relationship between ontology and method from the fifties to the present—on the grounds that method must be congruent with prevailing ontologies, i.e., prevailing assumptions about causal structures in the social or political world. He does this with an appreciation for the enduring legacies of the innovators of the past.

Hall demonstrates how comparativists have continually recast methods to better analyze various ontologies or concepts of politics over the last five decades: first, relatively narrowly conceptualized legal and political institutions; in a second stage, holistically conceptualized national polities; and third, causal regularities within variable based images. Finally, for today's academy, he points to our responsibility to develop new methods to capture our more complex notions about the nature of political causality, and our idea of politics as the outcome of unfolding chains of iterative strategic interactions and choices.

Our current theories "see the world . . . as a branching tree whose tips represent the outcomes of events that unfold over time" in sequences, within specific national or community contexts (385). Such new ontologies, like those of the past, have "outrun" our methods and thus imply new standards of explanation; our view of what is an acceptable explanation has in turn shifted toward the historical. Just underneath this intellectual history and analysis is the intriguing suggestion that our shifting ideas about the nature of politics actually drive innovation in method.

In exceptionally lucid and crisp prose, Hall recommends we give renewed attention to perfecting the small n comparative approach, both because it retains a recognizable variable orientation and because it is best able to assess causal relationships posited by the new ontologies. He recommends "systematic process analysis"– what George and Bennett have called process tracing–to precisely delineate the sequence by which various associated causes affect processes of elite and mass and institutional position taking in public and in private. In doing so, he reminds us that the explanatory power of a theory results from its specification of an intervening process in addition to outcomes.

As one committee member noted, these recommendations are not new. But of course the best advice never is. By framing the argument within the past and current tensions between ontology and method he has shown perhaps more clearly than ever why strategic interaction and path dependent reasoning demand a renewed emphasis on rigorous case-based process analysis. Hall has given us a stellar article, and it is a most worthy and appropriate recipient of the first Alexander George Award of the Section on Qualitative Methods.

Sage Award

This award is granted for the best paper developing or applying qualitative methods, presented at the previous Annual Meeting of the American Political Science Association. This award honors the contribution of Sara and George McCune to the field of qualitative methods, through their role in founding Sage Publications and developing it into a leading publisher in the field of social science methodology.

Recipient: John Gerring, "Causation: A Unified Framework for the Social Sciences."

Committee: Elizabeth Kier (chair), University of Washington; Eric Schickler, Harvard University; and Deborah J Yashar, Princeton University.

Citation: Gerring's paper contrasts a unitary and plural view of causation, and after raising several difficulties with the pluralist view, argues for a minimal definition of causality. He then sets out criteria to assess causal arguments across fields and across methods. Gerring discusses sixteen criteria that apply to the formal properties of the argument, such as "strength" and "relevance," as well eight that apply to the choice of research design, such as "independence" and "transparency." He concludes by arguing that his framework will help promote cumulation in the social sciences.

The award selection committee agreed that by synthesizing what are often viewed as disparate views of causation and providing standards for assessing causal arguments, Gerring's paper is an important contribution to the the literature on qualitative methods. This accessible and provocative piece would be an especially useful addition to a qualitative methods syllabus.

International Political Science Review

Kay Lawson and James Meadowcroft, Co-Editors of the International Political Science Review, wish to call your attention to the fact that the IPSR now publishes only "open" issues (no more thematic issues) and that strong articles using qualitative methods are always welcome. All articles are refereed by experts in the field, drawn from a database of nearly 500 reviewers. To submit a manuscript, see the inside back cover of the journal for directions, and email to klawson@sfsu.edu and j.meadowcroft@sheffield.ac.uk.

Western Political Science Association

Program Chair, Ron Schmidt (Cal-State Long Beach), has added a new section to the March 2005 annual meeting: Interpretation and Method. Chaired by Perigrine Schwartz-Shea (Utah) and Dvora Yanow (Cal-State Hayward), the section has nine sessions:

1. Pitkin, Wittgenstein, and Interpretive Social Science
2. The Practice of "Reflexivity" in Political Science: Field Research, Science Studies, and Activism Considered
3. Studying the Politics of Labor: Interpretive and Qualitative Approaches

4. Meanings, Concepts, and Ideal Types
5. Interpretation and/in IR
6. Reading in a Different Way: Reflexive Historical Analysi
7. What Good is Political Theory?
8. Doing Theory
9. Consult a Specialist: Methodological Brainstorming.

The latter is a new type of session which will bring together a number of scholars specializing in various types of interpretive analysis, who will make themselves available singly or in pairs to anyone with a question about doing that kind of a study or using that kind of method. As the subtitle indicates, we envision these exchanges as informal conversations.

"Specialists" and their fields are:
Ethnographic Research: Samer Shehata
Field Research: Dorian Warren
Discourse Analysis: Cecelia Lynch
Narrative Analysis: Emery Roe
Semiotics/Rhetorical analysis/Ethnomethodology: Martha Feldman
Reflexive Historical Analysis: Ido Oren
New Historical Institutionalism/Science Studies: Pamela Brandwein
Category Analysis/Metaphor Analysis/Space Analysis: Dvora Yanow

We would like to extend an invitation in particular to doctoral students looking for assistance with any aspect of a research project from design to execution to writing, as well as to faculty with questions about researching and/or teaching these subjects. The full conference program is available at http://www.oir.csus.edu/wpsa/browse.asp. This is section 7.

A listserv has also been set up to discuss questions of interpretive methodology and methods. Further information is at http://listserv.cddc.vt.edu/mailman/listinfo/interpretationandmethods.

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