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Qualitative Methods and Cross-Method Dialogue in Political Science

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The author accepts the basic argument that recent advances in qualitative methods have had an uneven impact on the three major empirical fields in political science. He emphasizes that scholars in all three fields have made significant contributions to qualitative methodology, but these contributions have a more profound impact on the practice of qualitative work in comparative and international politics than in American politics. The author argues that the differences between qualitative and quantitative or formal research are less pronounced than some would believe. In particular, the author argues that scholars have overstated the argument that qualitative researchers are significantly more skeptical of universal generalizations, more inclined to incorporate scope conditions into their theories, and more complex in their views of social reality than are quantitative and formal researchers.

Keywords: *cross-method dialogue; multimethod research; selection bias; least-likely case; scope conditions*

It is now commonplace to say that we have experienced a sea change in the analysis and application of qualitative methods in political science and in the social sciences more generally. Three decades ago, at a time of growing interest in case study methods, critics and even advocates of qualitative approaches regarded most case study research as descriptive, subjective, nonreplicable, nonfalsifiable, insensitive to the logic of causal inference, and generally ill suited to the cumulation of knowledge (George, 1979; Verba, 1967). Sartori's (1970) lament about the number of "unconscious thinkers" in comparative politics easily applied to the discipline as a whole.

In the past decade, however, we have witnessed a quantum leap in both qualitative methodology and in the methodological self-consciousness of applied empirical research.[1] Qualitative scholars are far more likely than in the past to frame their work around explicit theoretical questions and to select cases and construct research designs to maximize their theoretical leverage.

We see a growing recognition in the discipline that case studies can in principle play a useful role, not only in historical description but also in the development, refinement, and perhaps testing of theories. We also see an expanding dialogue between qualitative and both quantitative and formal researchers and a growing appreciation for the utility of multimethod research (Maoz, Mintz, Morgan, Palmer, & Stoll, 2004; Sprinz & Wolinsky-Nahmias, 2004).

Although these advances in qualitative methodology have contributed significantly to empirical research in the discipline, those benefits have been felt unequally across the three major empirical fields of the discipline. There are variations across American, comparative, and international politics in sensitivity to methodological and metatheoretical issues, in the particular methods that are emphasized, in the degree of cross-method dialogue, and in the impact on research practice. This symposium makes an important contribution by highlighting these differences, analyzing their implications for the cumulation of knowledge in the individual fields and in the discipline as a whole, and suggesting that more extensive cross-method dialogue would advance scholarship throughout the discipline.

Each of the contributors to this symposium has provided a thorough analysis of the state of the art in their respective fields, made all the more credible by the fact that each has made distinctive contributions to both the theory and application of qualitative methods. A general summary and point-by-point critique would provide little added value, and instead I will highlight a few general points that cut across each of these essays and each of the fields. In the process, I will endorse the basic theme of the symposium but argue that many arguments about the differences between qualitative and quantitative or formal research have been exaggerated.

The uneven development of qualitative methods across the fields is particularly evident from Pierson's (2007 [this issue]) essay. Many would not be surprised by Pierson's argument that the study of American politics has been characterized by the dominance of quantitative methods and the marginalization of qualitative approaches,[2] but Pierson's analysis of the consequences of this divide for both qualitative and quantitative research in the field is quite illuminating. Pierson is particularly concerned with the absence of an ongoing dialogue between qualitative and quantitative researchers. In combination with the marginalization of qualitative methods, this absence of dialogue has contributed to "insulated clusters of scholars in particular sub-subfields" and insider debates, "methodological sloppiness" and insufficient methodological self-consciousness, and a relative lack of cumulation of research findings in qualitative research. For quantitative research, the absence of dialogue has

contributed to a narrow substantive focus and insufficient attention to context, scope conditions, macro configurations, and interaction effects.

Pierson (2007) is undoubtedly correct that the number of scholars, and the relative percentage of scholars, contributing to the growing literature on qualitative methods is lower in American politics than in comparative politics and international relations (IR), that qualitative empirical research in American politics is less informed by recent advances in qualitative methodology or by metatheoretical debates than is comparable research in comparative and international politics, and that in some respects there is less cross-method dialogue in American politics than in the other fields. It is important to acknowledge, however, that some the leading contributors to recent advances in qualitative methodology have been statistical methodologists in the American field. Gary King played a leading role in the writing of *Designing Social Inquiry* (King, Keohane, & Verba, 1994), which triggered the recent wave of work on qualitative methods. Henry Brady is now a key bridge figure in ongoing dialogue between qualitative and quantitative methodologists, and Brady and Collier's (2004) edited volume, *Rethinking Social Inquiry*, has contributed enormously to that dialogue.

Other statistical methodologists in American politics have also contributed to cross-method dialogues and suggested how quantitative and qualitative methods might usefully supplement each other. Christopher Achen coauthored an early article on the role of case studies in theory development (Achen & Snidal, 1979) and recently emphasized the tenuousness of the assumptions of the standard linear models that dominate quantitative studies in political science (Achen, 2002, 2005). Larry Bartels (2004) emphasized some of the limitations of the standard model of inference proposed by King et al. (1994), including its "incomplete and unrealistically optimistic" treatment of measurement error. Donald Green has written about weaknesses in applications of formal rational choice models to the study of American politics (Green & Shapiro, 1994). Among formal modelers, John Ferejohn and others have engaged metatheoretical debates about rational choice (J. Friedman, 1995), and Barry Weingast has been a leading contributor to the analytic narrative research program (Bates, Greif, Levi, Rosenthal, & Weingast, 1998), which tries to combine formal and case study methods.

Thus, some distinguished quantitative and formal scholars in American politics have made important contributions to cross-method dialogue. What is different about the study of American politics as compared to comparative and international politics is that qualitative scholars in American politics have been less inclined to contribute to this dialogue and that cross-method dialogue has had much less impact on empirical research in American politics.

Metatheoretical and methodological debates have not percolated down as much to the level of research practice in American politics as they have in other fields. This is perhaps not surprising for quantitative and formal work, which has reached a stage of normal science in which practicing scholars generally perceive metatheoretical debates as less relevant (Kuhn, 1962). It is more surprising for qualitative researchers in American politics. If one's work deviates from the standard template in a given field, we might expect a greater effort to establish the metatheoretical justification for that research, both for intellectual reasons and also for its strategic or rhetorical value in intrafield debates and turf struggles.

I was also struck by Pierson's comments about the relative lack of cumulation of research findings in qualitative work in American politics. I wonder if part of the problem is that scholars in the American field have given less thought to appropriate metrics for evaluating the cumulation of knowledge in their field than have their counterparts in comparative and international politics, where the absence of a dominant paradigm has led to greater engagement with the literature in the philosophy of science. The most commonly invoked philosopher of science in the IR field, perhaps going back to the mid-1980s, is Imre Lakatos (1970), whose methodology of scientific research programs provides a set of criteria for assessing progress in social science research. A fair number of IR scholars have applied Lakatosian metatheory in an attempt to evaluate the degree of progress in a number of research programs (both qualitative and quantitative) in the IR field (Elman & Elman, 2003). Scholars in comparative politics, although certainly concerned with metatheoretical issues (e.g., Lichbach, 2007), have given less attention to metrics for evaluating progress in research programs, though one important exception is Lustick's (1997) application of Lakatos to Lijphart's (1968) consociationalist research program.

The dialogue across methodologically defined divisions in the discipline manifests itself in a number of different ways. One is the way in which ethnographic studies in comparative politics generate insights that lead to the construction of rational choice models. Bates (1981) provides one example. Another is Laitin (1998), for whom "the source of insight has almost always been ethnographic" (Laitin, 2006, p. 27). The sequence can also be reversed, with rational choice models and equilibrium concepts guiding empirical inquiry and systematic discrepancies between theoretical expectations and empirical findings then used to shape the revision of the theory. A good example is the analytic narrative research program (Bates et al., 1998).[3] Formal models and case studies are also combined in IR, with formal rational choice models being used to guide historical case studies (Bueno de

Mesquita, 2000).[4] Most game theoretic models in IR are now combined with statistical analysis, and it is increasingly common to combine statistical analysis with case studies. This practice reflects an awareness of the limitations of any single method and the advantages of using other methods to compensate for those limitations.

A smaller number of scholars in both comparative and international politics are beginning to integrate formal, statistical, and case study (or narrative) methods in a single study, or at least in a single research program, as reflected by Laitin's (2002) call for a "tripartite method." Examples from IR include the cumulative efforts of many scholars in the interdemocratic peace research program (George & Bennett, 2005, chap. 2), and Schultz's (2001) construction, testing, and illustration of an informational model of democratic crisis behavior. A good example from comparative politics is Mares's (2006) study of bargaining between European governments and unions over wages and social services. Some recent research on civil war also incorporates formal and particularly statistical and narrative methods (Fearon & Laitin, 2006; Kalyvas, 2006).

Another manifestation of a cross-method dialogue is the growing interest of case study researchers in the methodology of case selection. Although two or three decades ago it was quite common for qualitatively oriented scholars to select cases based on their inherent historical or substantive importance, contemporary scholars in international and comparative politics (with the possible exception of area studies in the latter) are much more inclined to select and justify their cases based on theoretical criteria. As a consequence, qualitative researchers are increasingly aware of issues of selection bias, particularly to dangers of selecting cases in which there is no variation on the dependent variable. It is now rare, for example, to find comparative case studies of the causes of war that examine only wars and ignore nonwars, or studies of deterrence that examine only cases of deterrence failure. The same is true in comparative politics, where scholars were sensitized to the dangers of selecting on the dependent variable by Geddes's (1990) identification of such problems in a number of well-known studies on social revolution, inflation, and economic growth.

The injunction against "selecting on the dependent variable" (King et al., 1994) acquired a mantra-like status, however, and its uncritical acceptance by many scholars created a problem of its own. Scholars now recognize that selecting cases with the same outcome on the dependent variable is appropriate for the testing of hypotheses based on necessary conditions (Braumoeller & Goertz, 2000; Dion, 1998). Recent work in qualitative methods also suggests that process tracing and related forms of within-case analysis are much

less vulnerable to problems of selection bias than are large-*N* statistical studies or small-*N* cross-case studies based on Mill's methods or "intuitive regression" (D. Collier, Mahoney, & Seawright, 2004).

Scholars also have a tendency to select for study a subset of events that actually occur and to neglect events that do not occur. Such events are more visible than nonevents, and puzzle-driven political science often places a premium on explaining classes of events that occur contrary to prior theoretical expectations. People are more interested in explaining the failures of deterrence and of intelligence that lead to wars than in explaining deterrence and intelligence successes that keep the peace. For the purposes of theory development and testing, however, it is not obvious that events provide greater leverage than nonevents. It was the dog that did not bark, after all, that provided the key insight for Sherlock Holmes.[5] In terms of the logic of inquiry, explaining why theoretically expected events do not occur is just as important as explaining why theoretically unexpected events do occur.[6]

Another issue involving case selection involves the theoretical utility of individual cases.[7] Although many have argued that case studies might be useful in developing theory but that they cannot by themselves test theories (Achen & Snidal, 1979), more scholars have begun to argue that a small number of case studies, and perhaps even a single case, can successfully be employed to test a hypothesis that posits necessary or sufficient conditions—or, more generally, any theory that provides precise predictions (Braumoeller & Goertz, 2000; Dion, 1998).[8]

One example of a single case study that did enormous damage to a hypothesis comes from the literature on intelligence failure. Prior to the 1960s, it was widely believed that the primary source of strategic surprise was insufficient information. Had the United States possessed ample information, it would have anticipated (and possibly avoided) the Japanese attack on Pearl Harbor. That hypothesis was seriously weakened by Wohlstetter's (1962) single case study of the American intelligence failure at Pearl Harbor. She demonstrated that the United States had ample information of the impending attack but that the information was lost in a noisy international environment and blocked by parochial bureaucratic interests. The hypothesis was further weakened by Whaley's (1973) demonstration that Stalin's failure to anticipate the German invasion in 1941 occurred in the context of ample information but one in which uncertainty was compounded by German strategic deception.

Another example of the use of case studies to test hypotheses involving necessary or sufficient conditions is interdemocratic peace theory. The understanding that a single case of war between democracies can falsify, or

at least seriously undermine, the proposition that "joint democracy" is sufficient for peace within a dyad has led a number of researchers to engage in detailed historical studies of "borderline" cases, in an attempt to demonstrate that a more precise coding of democracy and/or war in these cases would result in a violation of the democratic peace hypothesis. Researchers also engage in process tracing of crises between democracies, in an attempt to ascertain whether the peaceful resolution of the crisis resulted from the democratic character of the actors or whether it is better explained by power relationships or other variables that have nothing to do with democracy (Ray, 1995). This research reflects two other uses of case studies, involving the in-depth analysis of a single case to (a) gain a more precise measurement of the key hypothesized variables and (b) trace the intervening causal mechanisms.[9]

A number of qualitative researchers have also argued that "most-likely" and "least-likely" case study designs (Eckstein, 1975) can be effectively used to test theoretical propositions (George & Bennett, 2005). The basic argument, which deviates from conventional statistical approaches and adopts a Bayesian perspective, is that not all cases are created equal for the purpose of drawing inferences and that the weight of the evidence has to be judged relative to prior theoretical expectations. If a researcher believes that a particular case is quite unlikely to be consistent with a theory's predictions—either because the values of many of the theory's key variables point in other direction or because the theory's scope conditions are not fully satisfied—and the data support the hypothesis, the evidence from the case provides a great deal of leverage for increasing our confidence in the validity of the theory. Inferential leverage from such a least-likely case is increased if the theoretical priors for the leading alternative theory make it a most-likely case and if the data do not support that theory. Similarly, if one's theoretical priors suggest that a theory is highly likely to be confirmed, and if the data do not support the theory, that result can be quite damaging to a theory. It is important to note that the logic of inference is asymmetric. Although evidentiary support for a theory from a least-likely case or lack of support from a most-likely case provide substantial theoretical leverage, evidentiary support for a theory from a most-likely case or lack of support for a least-likely case provide only modest basis for generalizing.[10]

Qualitative methodologists are correct to emphasize the potential utility of most- and least-likely case study designs. What they fail to emphasize is the same inferential logic can also be useful for statistical researchers (or even for experimental research). One can pick a subset of a population based on most- or least-likely criteria and do conventional statistical tests within that subset,

recognizing that supporting evidence and disconfirming evidence carry different inferential weights. For example, Levy and Thompson (2005) argued that the past five centuries of Europe was a most-likely case for balance of power theory. Their statistical tests generated support, in the European context, for the hypothesis that great powers balance against extreme concentrations of power capabilities but not for the hypothesis that great powers balance against leading states with more modest capability advantages. They concluded that supporting evidence for the first hypothesis only slightly increased confidence in its external validity, whereas the disconfirming evidence for the second hypothesis significantly reduced confidence in its external validity.

The issue of case selection indirectly relates to another issue, one of scope conditions, which raises broader theoretical issues. Most qualitative methodologists argue that because of their in-depth focus on a smaller number of cases provides more detailed knowledge of individual cases, qualitative researchers, compared to formal or statistical modelers, are more sensitive to the importance of context and more skeptical of universal generalizations. As a result, they are more likely to recognize conceptual and causal heterogeneity and to restrict the scope of their populations to avoid "conceptual stretching" (Sartori, 1970), satisfy the "unit homogeneity" requirement for causal inference (King et al., 1994), and make meaningful comparisons. Thus qualitative researchers are more interested in establishing the scope conditions of their theories than are large-*N* researchers, who aim to establish more universal propositions based on statistical tests that incorporate the largest possible populations and hence the maximum statistical power (Mahoney & Goertz, 2004). Recall Pierson's (2007) argument about the "inattentiveness to scope conditions" in quantitative studies of American politics.

Although I share the skepticism about universal generalizations, I am not convinced that the relationship between methodological orientation and tendency toward parsimonious theory and universalist generalizations is a strong one. The most parsimonious theory of IR is undoubtedly Waltz's (1979) neorealism, which makes universal claims about behavior in any multistate system regardless of historical and cultural context.[11] Waltz's structural realist theory generated an enormous amount of case study research and several new variations of realist theory, including offensive realism (Mearsheimer, 2001), defensive realism (Walt, 1987), and neoclassical realism (Schweller, 2006). Although less parsimonious than Waltzian neorealism, each of these alternative realist theories is equally universalist and suggests few if any scope conditions limiting its domain of applicability.[12]

Just as a universalist orientation is shared by qualitative and qualitative scholars in IR, one can find quantitative and qualitative studies incorporating scope conditions. Two examples are Levy and Thompson's (2003, 2005) analyses of balance of power dynamics. They reject the standard view that balances and balancing are universal, argue that the dynamics of strategic interaction in the European international system differ from those in maritime systems, and confirm their key hypotheses in a quantitative study of balancing in the European and global maritime systems during the past five centuries.

One can also find quantitative (and qualitative) research in comparative politics that rejects universalist assumptions and emphasizes the importance of context, as reflected in the numerous interaction terms in their econometric models. One example is Mares (2006). Another is Clark (2003), who demonstrates that the relationship between elections and the macro economy is deeply context dependent and depends on central bank independence, the mobility of capital, the flexibility of exchange rates, and other variables.

Skepticism about the value of universal generalizations is also reflected in recent arguments by some quantitative analysts that researchers should abandon large-scale regression analyses that incorporate many dummy and control variables into the analysis of relationships in large populations. They argue instead that researchers should focus on particular subsets of cases and limit the use of control variables (Achen, 2002, 2005; Clarke, 2005; Ray, 2005). Achen (2005), for example, argues that instead of using "big, mushy linear regression and probit equations" that are "jamming together all sorts of observations that do not belong together," researchers should "separate the observations into meaningful subsets" (p. 337). This approach is logically equivalent to the incorporation of scope conditions.

Many of my arguments questioning the asserted relationship between methodological orientation and tendency toward universal generalizations applies to a similar assertion, made by several qualitative methodologists, including the contributors to this symposium, about the relationship between a scholar's methodology and his or her ontological views about the nature of reality. Following Hall's (2003) influential essay on the desirability of aligning methodology with ontology, these scholars argue that qualitative scholars have more complex worldviews than do quantitative or formal scholars. Pierson (2007), for example, associates comparativists' greater attention to context, causal complexity, and scope conditions to their collective belief that the world is complex. He argues that comparativists "have been gravitating

toward ontologies that accept that causal relationships are likely to be heterogenerous and context dependent" (p. 164). Bennett and Elman (2006) make a similar argument:

> Qualitative methodologists tend to believe that the social world is complex, characterized by path dependence, tipping points, interaction effects, strategic interaction, two-directional causality or feedback loops, and equifinality . . . or multifinality. . . . The possible presence of these complexities affects how knowledge statements can be most usefully constructed and verified" (p. 457)

These views are reflected in the phrase on the T-shirt given to participants at the Institute on Qualitative Research Methods in 2006: "Because life is complicated."

The relationship between ontology and methodology is extraordinarily complex.[13] It includes prescriptive arguments such as Hall's (2003) and descriptive statements such as those cited in the last paragraph. I focus on the latter, which I think are misleading.[14]

For one thing, the hypothesized association of qualitative scholars with complex worldviews and quantitative scholars with less complex worldviews is hard to reconcile with the fact that more and more scholars are doing multimethod research (Bates et al., 1998; Kalyvas, 2006; Laitin, 1998), thus linking a single ontology with several distinct methodologies.[15] It is also hard to reconcile with the fact that theories associated with qualitative scholars are sometimes more parsimonious and more universalist than those associated with formal and quantitative scholars, as suggested by the example of Waltzian neorealism in international politics.

Or consider more middle-range theories. Although George's model of deterrence (George & Smoke, 1974) is more nuanced and complex than are early formal models of deterrence, it is no more complex than recent formal models of deterrence involving subgame perfect equilibria in sequential games of incomplete information or models of asymmetric deterrence in repeated games based on Markov perfect equilibria concepts (Langlois & Langlois, 2005). Or consider the complexity of formal models of redistributive politics or of electoral competition in comparative politics (Persson & Tabellini, 2000). Indeed, a long-standing critique of game theoretic models of behavior is that such models incorporate complex calculations that are far beyond the capacities of individual actors.[16]

Qualitative and quantitative or formal scholars often study the same basic phenomena from radically different perspectives. Path dependence is a good

example and involves both qualitative scholars and formal modelers (Arthur, 1994; R. B. Collier & Collier, 1991; Greif & Laitin, 2004; Page, 2006). It is not obvious that one set of scholars has a more complex worldview than the other, only that different training led them to adopt different research strategies. Similarly, scholars debate whether statistical methods can adequately capture complex causation in the form of multiple paths to the same outcome (Braumoeller, 2003; Clark, Gilligan, & Dolder, 2006; Ragin, 1987), but in the absence of more information, there is little reason to assume that one set of scholars sees a significantly more complex world than the other.

Descriptive statements about the correlation between ontology and methodology also fail to emphasize that theory serves many purposes in social science, and that a scholar with a given ontology may adopt a different methodology depending on whether his or her primary aim is description, explanation, prediction, or policy prescription. Many scholars, quantitative and qualitative, might concede that a full *description* of the world should ideally approximate the complexity of the reality it attempts to capture,[17] but argue that the task of *explanation* is different. It requires that the analyst abstract from reality to explain its central features.[18] Many, perhaps most, formal or statistical modelers construct relatively simple models not because they believe that the world is complex but instead because they believe that parsimony is an important criterion of a good explanation because it facilitates both generalization and falsification.[19] The tasks of policy prescription are different still and may be best served by a less parsimonious set of conditional generalizations (George, 1993).

It is important to note that most rational choice models of empirical behavior do not necessarily assume that in the real world people actually act rationally. Rather, they construct their models on the assumption that people behave *as if* they were rational.[20] The "as-if" assumption follows M. Friedman (1953), who emphasized the importance of prediction and who argued that for that purpose the descriptive accuracy of a model's assumptions is a secondary consideration.

Another problem with the posited link between ontology and methodology is that it focuses too much on particular methods and ignores the larger research strategies in which methods are embedded. Many structuralists see a complex world but believe that the most efficient research strategy for understanding that world is to begin with relatively simple structural models, explain as much variance as possible with those models, and then gradually add complexity as needed. Singer (1990) has been quite explicit about this research strategy with respect to the Correlates of War project. The evolution of the realist research program in IR can also be described in

these terms, as dissatisfaction with the explanatory power of Waltzian neorealism lead realists to incorporate more variables in an attempt to explain additional variance.[21] Changes in models as research programs evolve do not necessarily imply changes in underlying ontology.

Thus, the argument that quantitative and formal scholars construct parsimonious models because they see a relatively simple reality, whereas qualitative scholars develop more complex explanations because they see a more complex world, is itself an excessively parsimonious metatheoretical statement that fails to capture the complex relationship between ontology and methodology in social science. I suspect that proponents of the ontology-methodology relationship recognize the complexity of that intellectual reality but find it useful to abstract from that reality to highlight some of the essential differences between research communities. A similar logic leads many quantitative and formal scholars to construct simplified models of a complex reality.

Conclusion

The view that recent advances in qualitative methods have had an uneven impact on the three major empirical fields in political science (Bennett & Elman, 2007 [this issue]; Mahoney, 2007 [this issue]; Pierson, 2007) is a persuasive one, but I qualify the argument in a number of ways. Leading quantitative methodologists in the American politics field have engaged in a fairly extensive dialogue with qualitative methodologists, but that dialogue has had minimal impact on applied qualitative research in American politics. Qualitative scholars in comparative and international politics have been much more involved in debates about methodology and metatheory than have their counterparts in American politics, and those debates have had a significant impact on research practices in those disciplines.

I have also argued that scholars have exaggerated some of the differences between qualitative and quantitative or formal research. Scholars often portray most- and least-likely case designs as something distinctive in qualitative research, but I see no reason why similar logic cannot be applied to case selection and causal inference in quantitative analysis. Researchers can select samples based on most- or least-likely criteria, conduct statistical analyses on those samples, and then use most- or least-likely logic to condition their inferences from those samples.

I also think that qualitative methodologists overstate the argument that qualitative researchers are significantly more skeptical of universal generalizations and more inclined to incorporate scope conditions into their theories than are quantitative and formal researchers. The most parsimonious

and universalist theory in IR, for example, is Waltz's (1979) neorealism, and neither neorealism nor other leading variations of realist theory incorporate significant scope conditions into their primarily qualitative analyses. Meanwhile, more and more quantitative research throughout the discipline is beginning to incorporate scope conditions. This is reflected in the increased use of interaction terms rather than strictly additive models. It is also reflected in recent analyses of potential problems with massive regressions and many control variables and pleas for more empirically bounded statistical analyses (Achen, 2002, 2005; Ray, 2005).

Finally, I question the argument (Bennett & Elman, 2006; Pierson, 2007) about the descriptive relationship between ontology and methodology and particularly the assertion that qualitative scholars tend to have more complex worldviews than do quantitative or formal scholars. Many formal and/or econometric modelers construct and test relatively simple models not because they believe the world is simple but rather because they are more interested in explanation than in description and because they believe that parsimonious explanations are preferable because they are more generalizable and more falsifiable. Many qualitative scholars have similar conceptions of social science explanation.

Notes

1. Advances qualitative methodology have probably outpaced advances in actual research practices. The gap is probably narrower in quantitative research.

2. The relative frequency of qualitative articles in American politics is lower than in the other fields (Bennett, Barth, & Rutherford, 2003).

3. This interplay between theory and evidence is also central to George's research program on deterrence and coercive diplomacy (George & Smoke, 1974) and to his case theory methodology more generally (George & Bennett, 2005).

4. Formal game theoretic models influence qualitative research in international relations in less structured ways as well. It is striking how much of the case study literature on peace, war, or security is framed around concepts of signaling, credible commitments, private information, commitment problems, and indivisible issues, which emerged from game theoretic research on signaling models and on the "bargaining model of war" (Fearon, 1995).

5. For an application of this case selection logic to international relations, see Ripsman and Levy (in press), who attempt to explain why a preventive war did not occur in the 1930s, when most of the optimal conditions for such a war appear to have been met.

6. One problem in studying of nonevents, or "negative cases," is the determination of which ones to study because they are large or potentially infinite in number. For a useful set of criteria, see Mahoney and Goertz (2004).

7. Although most of the qualitative literature focuses on the utility of case studies for theory construction and testing, some argue that understanding cases is also valuable as an end in itself (Gerring, 2006).

8. As Mahoney (2007 [this issue]) argues in his brief discussion of "smoking guns," "For qualitative researchers, a theory is usually only one key observation away from being falsified" (p. 132).

9. Qualitative methodologists often claim that qualitative researchers are more concerned than their quantitative counterparts with conceptual validity and with avoiding measurement error. This may be true, but the democratic peace literature is one example of quantitative researchers giving a great deal of thought to the conceptualization of key theoretical concepts, namely, what democracy is and how to measure it (Ray, 1995). To the extent that qualitative researchers are more interested in conceptual validity, however, we must recognize the important methodological trade-offs involved (Gerring, 2001; Goertz, 2006). The more one restricts the population of cases to ensure conceptual homogeneity and avoid conceptual stretching, the less one is able to generalize. Qualitative researchers give more weight to internal validity, whereas quantitative researchers give more weight to external validity. Both are necessary for the development and validation of good and useful theory, which is an important advantage of multimethod research programs.

10. Allison's (1971) study of the Cuban Missile Crisis fits this logic. This acute international crisis was a least-likely case for the bureaucratic politics and organizational process models and a most-likely case for a rational state actor model. The logic of least-likely case design is based on the "Sinatra inference"—if I can make it there, I can make it anywhere. Similarly, the logic of most-likely case design is based on the inverse Sinatra inference—if I cannot make it there, I cannot make it anywhere (Levy, 2002, p. 442).

11. Most system-level statistical studies associated with the Correlates of War project are also universalist in orientation, as are both qualitative and quantitative studies based on a liberal institutionalist paradigm.

12. It is worth noting that both defensive and neoclassical realism have important domestic components, so that the universal character of most realist theories cannot be traced to their structuralism. In fact, many psychological models of foreign policy—and of human behavior more generally—are universalist. Though the content of individual belief systems varies enormously across cultures, psychologists generally assume that their experimentally based findings regarding the processes of perception, reasoning, memory, and decision making apply to all human beings. Nisbett (2003) argues that this reigning psychological paradigm is flawed and that the thought processes of East and West are fundamentally different.

13. Pierson (2004, p. 293) and Bennett and Elman (2006, p. 457) recognize some dimensions of this complexity.

14. A systematic test of this hypothesized relationship would first require much more attention to the concept of complexity, which has multiple dimensions.

15. The dichotomy between qualitative and quantitative methods is also problematic, and it is not always clear why some methods have come to be classified as qualitative rather than quantitative. Boolean and fuzzy set methods (Ragin, 1987, 2000) are good examples.

16. Those models have become increasingly complex, as scholars attempt to incorporate beliefs, information, and learning (Hirshleifer & Riley, 1992); develop new equilibrium concepts based on evolutionary models (Fudenberg & Levine, 1998; Weibull, 1996); and abandon simplifying assumptions to model bounded rationality (Gigerenzer & Selten, 2001). In some respects, even more complex are attempts to use chaos theory (Kiel & Elliott, 1996) or other nonlinear models (Richards, 2000) to capture the complexity of political life.

17. Historians, far more than qualitative political scientists (perhaps excepting some area specialists in comparative politics), prefer complex explanations that attempt to account for a set of events in their entirety and explain how everything is connected to everything else (Levy, 2001).

As Hobsbawm (1997) argues, "Basically all history aspires to what the French call 'total history,'" where the historian "cannot decide to leave out *any* aspect of human history *a priori*" (p. 109) and ideally all aspects of an episode must be included in an historical explanation.

18. In international relations, for example, I doubt that Waltz and Bueno de Mesquita differ much on this issue.

19. Parsimony is conventionally defined as a property of theories, not of the real world. One theory is more parsimonious than another if the first explains as much empirical phenomena as the second but with fewer theoretical assumptions. The aim is to "explain more with less." Preferences for parsimonious theories go back to Occam's Razor in the 14th century and to Popper's (1965) argument that simpler theories are easier to falsify and consequently contain more explanatory power. This is what King, Keohane, and Verba (1994) call "maximizing leverage." Their conception of parsimony as a belief that the world is simple (p. 20) is not particularly useful.

20. This is clear in Bueno de Mesquita (1981) and Morrow (1994), among others.

21. These include more nuanced conceptions of threat, the offense-defense balance, and then domestic factors.

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