

Qualitative Methods

Newsletter of the
American Political Science Association
Organized Section for Qualitative and
Multi-Method Research

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Letter from the Editor

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At APSA 2007, the section was very active, sponsoring or co-sponsoring nearly 30 panels that were arranged by program officers Dan Slater and Randall Strahan. For APSA 2008, Craig Thomas and Hillel Soifer will serve as the section's program officers. Please send them your proposals!

The big event at the section business meeting was the discussion and vote on a proposal to change the name of the section to the APSA Organized Section for Qualitative and Multi-Method Research. The proposal was formulated by David Collier, Melani Cammett, and Andrew Bennett. After a lively discussion, the section enthusiastically approved the proposed name change. Thus, we are now the Section for Qualitative and Multi-Method Research.

The recommendations put forward by the nominating committee (Marc Howard, Rick Doner, Jacob Hacker, and Melani Cammett) were also approved. Colin Elman of Arizona State University was elected to serve as Section President in 2009–11. Margaret Keck was elected as Vice-President to serve from 2007–09. Peri Schwartz-Shea of the University of Utah and Rose McDermott of the University of California, Santa Barbara, were elected as At-Large Executive Committee members, to serve from 2007–09. At the meeting, Jim Mahoney also passed the section presidency over the John Gerring, who will serve from 2007–09. Finally, the article, paper, and book prizes were announced. The winners are reported in the back of this issue. (If I were Stephen Colbert I would have more to say).

The rapid increase in interest in qualitative methods of the last five years has been accompanied by a renewed interest in case study methodology. In this issue, we have a symposium on John Gerring's important new book, *Case Study Research: Principles and Practices* (Cambridge, 2007). The newsletter has already published a symposium on the George and Bennett volume on case studies (*Case Studies and Theory Development*) in the Spring 2006 issue. Beyond this, I have just read a very interesting volume (Jan Dul and Tony Hak, *Case Study Methodology in Business Research*, Butterworth-Heinemann, 2007). If one confronts these three books one is struck by how varied the topics and approaches to case study methodology can be. For example, Gerring emphasizes case studies and their relationship to experimental methodologies. George and

and Bennett discuss case studies and their relationship to typological theories. Dul and Hak talk about case studies and their relationship to linear probabilistic versus necessary and sufficient condition hypotheses. All of these books also express in various ways the rapidly growing interest in mixed and multiple methods and at the same time the need to connect methodology more closely to theoretical concerns.

The diversity of approaches to case studies means that there will be disagreements about core issues. The Lieshout contribution to this newsletter illustrates a natural and positive consequence of the flowering of work on case studies. King, Keohane and Verba devoted basically one chapter to philosophy of science and causation issues; George and Bennett make this topic central to their volume. Lieshout raises important concerns about the nature of causal mechanisms

and causation in George and Bennett. In the Gerring symposium, one point raised by several contributors is the nature of “single-outcome studies,” i.e., studies that focus on explaining just one case. This raises the core issue of the role of case studies in causal generalizations and the importance of this as a goal in case study research. I suspect that this will be a continuing topic of conversation among qualitative methodologists. The Casellas essay discusses the concept of representation and its relationship to case selection and typologies. It thus also illustrates how critical issues arise at the intersection of different methodological approaches.

Finally, I am still planning to have a review of qualitative methods and research design syllabi for the next issue so please email me your syllabi or the syllabi in use at your university if you have not done so. Thanks.

Symposium: John Gerring, *Case Study Research: Principles and Practice* (Cambridge, 2007)

Case Studies Are for Intensive Testing and Theory Development, Not Extensive Testing

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Case Study Research is a landmark book. This culmination of years of careful thought by John Gerring is by far the best dissection of case studies in the literature, in several ways. First, it is the most comprehensive discussion. It looks at case studies from every possible angle, and in a penetrating way that exposes the term “case study” as a handy label for what is actually a great variety of methods. It also examines case studies broadly, going beyond political science to describe variants of case studies that are done in economics, psychology, and medicine. The breadth of Gerring’s reading about this family of methods is extremely impressive. Second, it is clearly thought through and clearly explained. It corrects several mistaken notions about case studies. Third, chapter 7 is the most sensible and clear assessment of process-tracing that I have yet read. Fourth, because it is comprehensive and clear, it offers a new set of concepts for the different types of case studies and their goals and procedures, which could become a standard set of concepts that will make it easier for us all to debate these claims without getting tangled up in definitional issues. So it is a very important book. It’s probably a bit too technical for most undergraduates (although I am assigning chapter 3 to my undergrads this semester), but it should be required reading for graduate students, especially those in comparative politics.

I have only a few outright disagreements with Gerring’s arguments, and they are all about minor points. However, I do have a more significant disagreement on matters of emphasis.

If I were writing this book (which probably violates the “minimal rewrite rule” [206] because I am far less well-read than Gerring is on this topic), I would want to be more categorical in my judgments. It often seems that Gerring is trying too hard to find something nice to say about every possible kind of case study. (One exception is the “most-different cases” method, which he effectively dismisses.) I would want to state outright that some kinds of case study or cross-case analysis are very useful for certain purposes but not at all for others, and some are just not worth doing.

In particular, I would make a more rigid distinction between theory development and hypothesis testing. Gerring recognizes this distinction but does not make it stick everywhere that it should. This problem arose, I think, because he chose to define “case studies” in a way that makes generalization one of their inherent purposes. A case is an element in a sample, which is drawn from a population, he reasons, so by definition, there is no point in doing a case study unless it generalizes to the population in some way. Maybe the problem is that there is an unnoticed ambiguity in the term “generalization.” It can mean using a case to test whether a hypothesis is generally true, as Harry Eckstein and Douglas Dion have advocated doing. This, in my opinion, is impossible. There are no truly crucial cases in political science due to the multicausal and probabilistic nature of political phenomena, and our priors are not strong enough to support Dion’s prescription. There is a kind of testing we can do with a single case, which I will discuss below. But usually the kind of generalization that one does in a case study is not *testing* generalizations, but *hypothesizing* them. It is true that the case must relate to the population to be relevant, but it relates by proposing relationships that *might* be generally true. But a case study cannot tell us whether they really are generally true; that requires large-sample testing within the whole domain in which the theory applies.

That kind of testing could be called “extensive testing.” There is a different kind of testing, which is sometimes called

“intensive testing,” which is ideal for case studies, but it has a very different purpose and logic of inference. The goal of intensive testing is to judge which of several competing hypotheses does the best job of explaining a single case. It is therefore what Gerring discusses in the epilogue as “single-outcome studies,” and here and there as “internal validation,” but it doesn’t get the emphasis it deserves, because it constitutes at least half of the justification for doing case studies. Unlike extensive testing, which tests the same propositions in a large number of cases, intensive testing tests a large number of propositions in a single case. The logic is, “if my theory is true, then I would expect to observe these 20 things in this case. If the alternative theory is true, then I would expect to observe these 20 different things. Using Bayesian logic, if the 20 predictions of my theory are confirmed and the 20 alternative predictions are not, there is only a very low probability that my theory is wrong, and it becomes the better explanation for this case.” It is usually impossible to quantify these probabilities, but the logic behind them is very strong, and it makes case studies a very powerful method for explaining single outcomes.

This different emphasis would alter a few of the book’s passages. For example, I endorse Gerring’s conclusion on p. 147 that “Case studies...rest upon an assumed synecdoche: the case should stand for the population. If this is not true, or if there is reason to doubt this assumption, then the utility of the case study is brought severely into question.” I think there are *always* reasons to doubt this assumption, so it is *never* safe to generalize from one or a few cases. That’s why we should use them for theory development and intensive testing rather than for any attempt at extensive testing.

Another example: In his interesting discussion of matching as a promising alternative to specifying control variables in a regression, Gerring states that simply asserting that two cases are more or less the same for the purpose of matching “can be a huge advantage over large-N cross-case methods, where each case must be assigned a specific score on all relevant control variables—often a highly questionable procedure, and one that must impose strong assumptions about the shape of the underlying causal relationship.” (133–34). Yet it is always possible to specify at least a subjective dummy variable as a control, which would be exactly as accurate as asserting that two cases match, and it is often possible to assign more precise scores for regression variables. If assumptions about the linearity of a relationship are false, they can be modified and tested. I come away convinced that matching, which Gerring explains very clearly, is a method worth trying, but I suspect, as I think he does, that it will not be as useful in practice as it sounds in principle.

A final example concerns scope conditions. I love Gerring’s call in chapter 4 (76–85) for making scope conditions explicit and non-arbitrary; this is essential. But its implications are ambiguous unless we make it clear what the scope conditions demarcate. If it is *tested* propositions, there is little room for arbitrariness: the scope of tested propositions is exactly as large as the sample or the case used in the test; we can’t generalize beyond it, unless it was a random sample of sufficient

size, in which case we can generalize to the population. But if we are talking about how far a *hunch* might travel, then the scope of the hypothesis is hypothetical. It is essential to speculate about what the scope conditions may be, but we won’t really know until some extensive testing is done.

I also have one question that is unrelated to any of this. In chapter 6 (with Rose McDermott), which makes a beautiful, concise argument that an experimental logic undergirds all case studies, the most rigorous category, “Dynamic Comparison,” is defined as having both spatial and temporal variation. I wonder whether cross-sectional time-series analysis meets this criterion.

In conclusion, I think that in reality I agree with Gerring on just about everything and he agrees with me. I have quoted some passages in which he seems to have an opinion different from mine, but they are balanced by other passages that sound very close to what I have said on these issues. If we have differences, I believe they are only differences of emphasis.

Moving the Doormat to the Main Menu: Case Study Research Methods in the Social Science Toolkit

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John Gerring’s motivation for his book, *Case Study Research*, is the same as Harry Eckstein’s writing on the same subject three decades ago: He points out that case studies are much maligned—the methodological doormat if you will—despite their recurrence in so many influential works in our field and throughout the social sciences. To address this conundrum, Gerring hopes to “restore a sense of meaning, purpose and integrity to the case study method” (66).

And I think he largely does just that. He gives scholars the *potential* to do case studies in such a way that any social scientist could clearly see the logic through which the analysis could generate strong causal inferences.

It is a vital and lucid work that ought to appear on any graduate research methods syllabus. As much as it is a book about case studies, it is a treatise on research design and logical thinking that updates and integrates many classic and more recent contributions.¹ The book keeps its feet on the ground by examining a rich array of examples of completed work in political science, often with a healthy dose of pragmatism.

In my comments, I will highlight some of the novel insights found within various chapters in the book, and also raise some issues that I think warrant some additional attention, either by Gerring, today, or by him or other scholars in the future.

Definitional Issues

First is the question of defining the case study. If the quest is to dignify case studies, then it is necessary to know

what we are dignifying. There is a lot of ambiguity in the conventional use of the term, and in Gerring-style, we are afforded a careful, well-thought-out definitional discussion in chapter 2.

He writes that the case study is “the intensive study of a single case where the purpose of that study is—at least in part—to shed light on a larger class of cases (a population)... at the point where the emphasis of a study shifts from the individual case to a sample of cases, we shall say that a study is *cross-case*” (20).

By definitional fiat, he declares that case studies should be theoretically oriented, and not purely idiographic. Of course, others, particularly in other disciplines, may use the label “case study” for other purposes, but he makes clear that this is an enterprise that is social scientific. If one cites this book when doing a case study, this is a clear signal: the case study will be used to explore, illuminate, probe, or test broader propositions about specified causes and effects, even as it uses proper names and particularities in the discussion and analysis.

What Can Case Studies Do? How Are They Used?

In Chapter 3, Gerring puts his best foot forward and highlights the opportunities and comparative advantages of this method relative to other methods. In attempting a similar task, Eckstein may have done more harm than good for the method by *over-selling* the inferential possibilities with case studies. Gerring both tempers and expands the use of case studies by customizing the opportunities for case study work within the research cycle, and according to the type of theory one is dealing with, particularly whether the theory is of a more deterministic or probabilistic variety.

In many ways, Gerring’s book shares a vision of what constitutes convincing evidence that is *more* similar to King, Keohane, and Verba’s *Designing Social Inquiry* than many other recent works by qualitative methodologists. While it is true that Gerring has written extensively on the subject, he is clearly *not* arguing that qualitative research or case studies are in any way the “superior” empirical strategies. He is measured about what he promotes. As compared with *Designing Social Inquiry*, however, one does not finish *this* book thinking, “well, really the best thing to do is to avoid case studies, and to redesign a social inquiry such that it will produce many observations leading to a dataset amenable to quantitative analysis.” Gerring stresses that there are often key observations that shed disproportionate light on the connections between causes and effects, and that these must frequently be combined *across* different types of units in order to understand political dynamics. However, these connections are not made within the context of conventional datasets, which, by contrast, *require* unit homogeneity.

In this sense, the book really champions the idea of a larger division of labor—one that might be integrated within studies, or across studies, which is a view that many give lip service to but this book really defends in logical terms. A handful of key interviews or historical records may reveal the plausible links between cause and effect that complement correlational research of different types of observations such

that we can be convinced about the validity of a particular proposition. Brady and Collier refer to this as causal process observations, and Gerring also identifies a complementarity with dataset observations. In any case, this is a book that could be called “Mixed-Method” research as much as it is “Case Study Research.”

Issues of Case Selection

The largest chapter of the book is chapter 5: “Techniques for Choosing Cases.” I think any researcher on the verge of doing case study research ought to go through this list of nine selection strategies and make sure they can self-consciously recognize one or more of these types as characterizing their own, and if not, that should indicate that the study could probably be re-framed in a more crisp manner.

However, I do have some questions to raise. First is the notion of the “pathway” case, as a separate type of case study. A pathway case, according to the author, is one in which the causal effect of one factor can be isolated from other potentially confounding factors. As a pragmatic concern, I don’t understand how one can choose a particular case for its potential to illuminate causal mechanisms *prior* to having done the research. And how is this different from some of the other strategies, such as the “crucial case” strategy or even the “typical” case strategy, which are also used for hypothesis *testing* given particular scores on independent and dependent variables? I suppose the identification of this strategy as just one of nine makes me wonder what we are doing in the other hypothesis-testing case study strategies.

I really appreciate the attention to deliberate case selection, and I know Gerring realizes that many case studies are done with less pre-study consideration than he recommends. But I do think more needs to be said about the role of area expertise and personal experience. Traditionally, a clear justification for the choice of cases has been that some investigators develop internal databases of contextual knowledge and measurement skills that increase the reliability and validity of the study. If true, perhaps this strategy deserves its own place—it is probably the most practiced strategy in any case, and it is hard to imagine this changing.

Relatedly, there are the examples of what I might call the “convenient” case study. This is akin to available non-probability sampling, but it is also the reality of being a social researcher. Sometimes case studies find us, we don’t look for them. We may be working or traveling somewhere or reading something, and our interest is piqued, perhaps because what we observe confirms or contradicts some prevailing theory. One might respond, “well, then you did not use a case ‘selection’ strategy *per se*.” Perhaps, but I am going to guess that a sizeable portion of case studies are generated this way—they are the product of life circumstances and interests, personal and professional. Thus, it may be hyperbole to speak always of case *selection* when we talk about case studies. What we might be able to do is to engage in *case justification* and use the strategies that Gerring identifies to describe what it is we have a case of.

Also, I am not sure where we would put Michael Burawoy’s

“extended case study” into this typology of case studies. In various works, Burawoy has sought to use extended and deep case studies to discover flaws in and then modify existing case studies. While it is true that in some sense the goal is slightly different from the stated one of Gerring’s book, which is to make inferences about a larger population of cases, it does share an orientation toward theoretical conclusions. But again, for Burawoy, the selection of the particular case tends to be based more on the prospects for depth of study. This approach is intended to highlight when variables were simply conceptualized poorly or relationships misunderstood. It strikes me that by depriving this type of case a real label, it is devalued in principle, while in practice, it provides potentially enormous contributions to knowledge. But to my knowledge one could select such a case without any prior knowledge of the case’s place in the distribution of explanatory or outcome variables.

Notwithstanding, the chapter does an excellent job of bringing together the diverse forms of case selection strategies that have been used by scholars, including the logic of inference associated with Mill’s methods, and integrating them within a single, comprehensible framework.

Process Tracing

In chapter 7, Gerring argues that process tracing is *usually* a component of case study research; it usually relies heavily on contextual evidence. He states, “the hallmark of process tracing, in my view, is that multiple types of evidence are employed for the verification of a single inference—bits and pieces of evidence that embody different units of analysis...individual observations are therefore non-comparable” (173). I like this characterization. But I wonder—and I am really putting this out here for discussion—should the properties identified for *process tracing* be *definitional* for what we mean by case studies in political science? Should we reserve the term “case study” for those studies that employ such heterogeneous evidence? Otherwise, one might call, for example, every single lab experiment a case study.

The chapter on process tracing highlights clearly some of the strong empirical findings using this approach. He makes a nice recommendation—that we ought to be able to graphically diagram an argument in a series of steps, if even in the somewhat frighteningly complex manner that Mahoney does in the case of Skocpol’s *States and Social Revolutions*. I agree that diagramming is a good heuristic technique and probably ought to be used as a standard.

But the chapter on process tracing is short. A mere 13 pages, one-fifth the page-matter afforded the chapter on case selection. I don’t want to overinterpret, but this brevity, I think, reveals some of Gerring’s own apprehension with the case study method as it is generally practiced or understood in political science. Gerring writes, “process tracing evidence is, almost by definition, difficult to verify, for it extends to evidence that is nonexperimental and cannot be analyzed in a sample-based format...” (184). He says that the mitigating factors for process tracing are that it is (1) supplemental; and (2) can be vetted by “experts.” He concludes, “despite its apparently mysterious qualities, process tracing has an im-

portant role to play in case-based social science...it deserves an honored place in the toolkit of social science” (185).

Well, I detect a touch of inner conflict in that last sentence. And I think much more needs to be said about process tracing. Doing case studies well is doing this kind of analytical detective work. It is hard. I don’t know if we can develop general rules and strategies, but I think we can try, and the George and Bennett volume on case studies offers some additional discussion of process tracing.

My own suggestion for advancing the technique of process tracing is to identify more tailored sets of guidelines according to theoretical content and the level of analysis under investigation, whether it be the mobilization of collective actors, the making of policy, or the development of institutions. Political scientists would benefit a great deal by breaking down a set of criteria which they believe would establish a reasonable baseline for convincing or at least acceptable process-tracing evidence, including, say, temporal proximity of links in a causal chain; the explicitness of actor intentions; and/or the types of sources used. These benchmarks would not be ironclad rules, but might provide some standard for how we could evaluate the robustness of a qualitative result, just as conventional statistical analysis has measures of statistical significance. Just as a 95 percent confidence interval is arbitrary, so would these standards be, but without them, we have no reference line for discussing the content of evidence, except for completely useless metrics like number of months spent in the field.

Single-Outcome Studies

Finally, there is a concluding chapter on what Gerring calls, “Single-Outcome Studies.” A single-outcome study is when a researcher seeks to explain a single outcome for a single case. This is an incredibly important chapter—it takes on the elephant in the room of much social science research: that research agendas inevitably get driven by real-world, often catastrophic, events, such as the 9/11 terrorist attacks. I offer Gerring the acronym SOS for single-outcome study.

But I must admit, I leave this chapter a bit confused. Are SOS’es case studies, or not? Oftentimes the language of the chapter *contrasts* the SOS with the case study, but in describing the studies associated with nested analysis (e.g., Lieberman 2005) as types of single-outcome studies, well, that seems to me to clearly meet the criteria for a general case study. Indeed, the very terms used to describe those studies were “model-building” and “model-testing,” suggesting a direct engagement with the types of cross-case claims identified as central to Gerring’s ideal of the case study method. More generally, it is hard to imagine a political scientist studying a “single outcome” without some view of a larger universe of cases, so I require greater elaboration of what this type of study actually is, and how it is distinct from others.

Descriptive Inference

Finally, I want to discuss, if briefly, the explicit omission from the book, which is the task of descriptive inference. It is important to recognize that in this book, *most* of the science or

method of case study research comes from the strategies of case selection.

At the outset of his book, Gerring highlights that he focuses on *causal inference* because treatment of the “descriptive task of gathering evidence is well covered by other authors” (9). Actually, I disagree. I think that political scientists have very, very few good references on gathering appropriate data, particularly for the type of enterprise Gerring describes. The collection of observations that come from heterogeneous sources and unit of actors remains fairly ad hoc, and the task of summarizing accounts has received scanty treatment in political science.

Imagine a hypothetical study of ethnic conflict: someone is doing a case study in county X, and a survey reveals no hostile negative attitudes, but five in-depth insider accounts relate a mix of characterizations, and there is a riot in one province in which various ethnic slurs were shouted. As far as I know, the methodological literature tells us very little about how to score this case. And yet, this is the ever-present dilemma for the case study researcher working with multiple sources of data.

I don’t blame Gerring for omitting this type of discussion from the book because it is really a different kettle of fish, but I did want to highlight my belief that the integrity of case study research will rest on principles of descriptive inference at least as much as on principles of causal inference. More generally, I think that the discipline has devalued case studies for the very reason that we have emphasized the value of causal inference to a much greater extent than good measurement and descriptive inference, even though we know you can’t do the former without the latter.

Going Forward

To conclude, *Case Study Research: Principles and Practices* is at its very best in relating the possibilities for case study work in logical juxtaposition to other types of inferential strategies. It is smart, and provides sturdy analytical scaffolding for the development of new case studies. I think it should encourage us to do more case study work with our heads held high. But we will still need to be explicit and self-conscious about how those studies get done so that we can have an even better handle on what it is about intensive study of a case that convinces us of the strength of a general proposition. Gerring has made a major contribution to social science by helping to systematize this genre of research.

Note

¹ Such as Campbell and Stanley (1966) on quasi-experimental research design; Eckstein (1975) on case studies; Collier’s (1991) and Sartori’s (1970) statements on the comparative method and the relationship to statistical methods; and much of the more recent qualitative methods research carried out by other scholars such as Mahoney (1999); George and Bennett (2004); Brady and Collier (2004); and King, Keohane, and Verba (1994).

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Particularizing Case Studies: A Critical Review of Gerring’s Case Study Research

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A large methodological literature addresses the topic of case studies. But much of this work focuses on issues pertaining to data collection, including techniques of data retrieval (e.g., ethnography, interviews), coding, and recording. By contrast, John Gerring’s stimulating new book, *Case Study Research: Principles and Practices*, considers the logic of case study research design. Gerring seeks to explicate the meaning, purposes, and payoffs of the case study. Although the book focuses on practices as well as principles, it is not so much a “user’s guide” as a full-blown theory of the inner workings and rationales of the case study method. That said, all scholars who read the book will discover many new ideas for carrying out better case study research in practice.

The book is delimited by the *kind* of case study research on which it focuses. Within the broad field of case study research, Gerring’s interest is very explicitly on work that seeks to make causal inferences, not work whose primary goals fall into the realm of descriptive inference. For example, the methodological issues that arise for case studies that are mainly interpretive or rooted in critical theory are not the focus of the discussion.

Among those case studies that pursue causal inference, Gerring further focuses on those that seek to generalize from the case to a larger population. He in fact defines a case study “as the intensive study of a single case where the purpose of the study is—at least in part—to shed light on a larger class of cases (a population)” (20). One implication of this definition is that work that seeks to explain a single outcome in a single case is not defined as a case study. Gerring instead considers this work to be a “single-outcome study,” and he briefly considers it in the epilogue.

Should studies that primarily try to explain particular outcomes be left out of the definition of case study? I think the answer is “no.” Indeed, I will argue that Gerring overemphasizes the generalizing aspects of case study research. And I will suggest that this overemphasis derives from his conviction that the merits of case studies are best evaluated according to the extent to which they approximate an experimental design. The overall consequence, I will suggest, is that *Case Study Research* does not address important methodological issues that apply to a significant strand of case study research in political science—namely, case studies in which the main goal of analysis is to identify the causes of a specific outcome in a particular case.

Contributions

Let me first applaud John Gerring for clarifying much of the general logic of all case studies, and especially generalizing case studies. The book includes a superb discussion of the relationship between observations, cases, samples, and population—which allows Gerring (23–25) to vividly illustrate the differences between a case study dataset, a cross-case cross-sectional dataset, and a time-series cross-sectional dataset. These distinctions, in turn, motivate an excellent discussion of different types of research design, including three types of case studies: diachronic, synchronic, and diachronic-synchronic.

The important chapter 3, “What is a Case Study Good For?,” offers great insight into the comparative strengths and weaknesses of case study research. With cross-case research as the comparison group, Gerring concludes that the case study is strong at hypothesis generating and weak at hypothesis testing, strong on internal validity and weak on external validity, strong on locating causal mechanisms and weak on specifying causal effects, and strong on working with deep propositions and weak on working with broad propositions. I agree with the general thrust of these conclusions, though I want to argue below that: (1) the relative strengths of case study research derive mainly from its effort to explain particular outcomes in specific cases, and (2) the relative weaknesses of case study research are less of a concern when we realize that the goal is often mainly to explain the particular outcome.

Chapters 4, 5, and 6 are co-authored with Jason Seawright, Rose McDermott, and Craig Thomas, respectively. The chapter with Seawright offers an excellent discussion of different techniques for choosing cases. Although researchers often have theoretical reasons for looking at certain cases, this chapter addresses the *methodological issues* that should drive

case selection. The typology of types of cases: typical, diverse, extreme, deviant, influential, crucial, pathway, most-similar, and most-different is the most sophisticated and comprehensive of its kind (of which I am aware). To boot, the chapter includes an interesting discussion of the appropriate cross-case technique for locating different kinds of cases. Likewise, the chapters with McDermott and Thomas add distinctive contributions concerning, respectively, the application of an experimental template and the use of process-tracing evidence for generating valid causal inferences. Again, they are written more with the goal of uncovering the logic of causal inference in case study research than offering a single set of procedures that analysts can or should try to follow in their research.

Concerns

My fundamental concern with the book is that it underappreciates the extent to which the primary goal of many case studies is to explain a particular outcome in a specific case. Of course, all case study researchers must draw on general knowledge, broad theory, and insights from a larger range of cases. In that sense, even the most particularizing case studies are very centrally engaged with generalization. However, the goal of the analysis is often to use general insights and individual case knowledge to *explain the particular*. Whether and how an explanation of the particular sheds direct light on a broader class of cases is a secondary issue.

The idea that many case studies seek to explain particular outcomes should not be controversial. What caused World War I? What caused the French Revolution? What caused sustained high growth in Korea? What caused the breakdown of military rule in Argentina? These kinds of questions are familiar in political science, and I suggest that they animate much of the case-study research in the discipline. I want to suggest that these sorts of questions are probably as common as alternatives: what does World War I teach us about the causes of war in general? What does the French Revolution teach us about the causes of revolutions in general? What does high growth in Korea teach us about the causes of high growth in general? What does the breakdown of military rule in Argentina teach us about the breakdown of military rule in general? I think Gerring favors the latter kinds of questions. But, as point of fact, I want to argue that many or most case-study scholars primarily address the former questions, turning to the latter briefly and often inconclusively (e.g., as speculative observations at the end of their studies).

Political scientists who favor general knowledge and who dismiss particular knowledge may find my argument to be discouraging for the case study method. Yet I think such a reaction is inappropriate. The social sciences must be oriented toward explaining the particular as well as making generalizations that apply to broader populations. Both kinds of explanations contribute knowledge. We cannot look to historians to develop valid causal inferences about particular outcomes. Many historians (though certainly not all) lack the training in theory and method to carry out this kind of research—their distinctive contribution to causal analysis rests

with the discovery and use of novel sources of data. The achievement of valid causal inference, even when particular outcomes are under analysis, requires the toolkit of social scientists.

If explaining a single outcome is one's goal, then one will naturally be concerned with internal validity, mechanisms, rich explanation, and the possibility of novel hypotheses—the strengths of case study research identified by Gerring. By the same token, the comparative weaknesses associated with case study research—e.g., testing general hypotheses, external validity, insight about average causal effects, and generalization—seem less troubling when we realize the particularizing goals of much of this research. Hence, appreciating the centrality of explaining the particular allows us to better see the source of strengths case study research that Gerring himself identifies. And it makes the weaknesses of this method appear as less problematic.

However, if much case study research has as its goal the explanation of the particular, then some of the orienting assumptions of Gerring's book need to be rethought. Most basically, questions arise about Gerring's argument (12) that "the characteristic virtues and flaws of case study research designs can be understood according to the degree to which they conform to, or deviate from, the true experiment." An experiment, after all, is intended to assess the effect (if any) of a given intervention on a dependent variable of interest. It is not designed to necessarily offer any kind of complete explanation of an outcome. Experiments teach about the average effects of interventions for populations, not about all of the factors that explain the outcome of interest in particular cases. In this sense, the experimental method is thoroughly predicated on the "effects of causes" approach. The same is true of statistical research, which tries to mimic an experiment in the context of an observational study. By contrast, case study researchers are often not concerned with the average effects of their causes across a large population. They have a very different research goal: to use reigning theoretical orientations, general knowledge, and novel inductive discoveries to explain the outcome of interest. This classic "causes of effects" approach stands in sharp contrast to an experimental design—and it has its own distinctive intellectual lineage, one that is not much discussed in Gerring's analysis.

The implication of this discussion is that still more work needs to be done on the case study method. Gerring has done a remarkably good job of discussing the inner logic and methodology of case studies that seek to generalize about causal patterns. But case studies are often only secondarily interested in producing generalizations. A crucial next step is to start where Gerring's epilogue leaves us: with case studies that primarily seek to explain particular outcomes in specific cases.

On Common Ground: Case Studies in Comparative Methodological Perspective

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It is perhaps worth noting at the outset that I am fascinated by discussions of methodology in the context of particular, actual research projects. But I find most books and articles on methodology to be rather tedious. Some are heavy-handed in selling the author's pet approach, and in the case of even the best advice, I often find myself thinking of all the intellectual and logistical reasons it would have to be modified for real projects of which I'm aware. I've nonetheless occasionally participated in more general and abstract discussions of methodology, for two reasons: I know they are a necessary part of promoting shared understandings in our field of what constitutes good or at least legitimate political science, and I know that students entering the field do need some general guidance before they can come to grips with the specific challenges presented by their own research interests.

So we do need good methodological texts, and I'm happy to report that in terms of content, accessibility, and even "anti-tedium" factor, John Gerring has written an outstanding text. It is not heavy-handed but it does have interesting, important, distinctive themes, and what's more, I have no major quarrel with them. I take at least some of these main themes to be:

- (1) especially when they are used to explore causal mechanisms and claims, case studies must be understood as, at least implicitly, instances of broader political phenomena, so that the difference between case studies and cross-case studies is one of degree;
- (2) because case studies necessarily involve analyses of many pertinent observations internal to the case, they can involve both quantitative and qualitative techniques;
- (3) the logic of good causal case studies is at bottom the same as that of experiments, in the sense that we are trying to determine whether a particular variable or set of variables produces an outcome that would not have occurred, all other things being equal (*ceteris paribus*);
- (4) the most basic methodological challenge really comes in the satisfaction of *ceteris paribus* assumptions (Gerring 2007, 171). We are looking for ways that we can plausibly say that the outcomes in which we are interested are traceable to the causes in which we are interested, but we can only really be confident of that if we have good reason to believe that, if everything else were just the same but those causes had been absent, the outcome would not have occurred. I want to stress this because, as I'll note shortly, making *ceteris paribus* claims in social science research is very difficult, whether or not we use particular quantitative or qualitative techniques.

But my main response to Gerring's book is to urge us to give greater weight to a few points that he gets right but does not stress as much as I think we should do and as I am about to do.

The points I'd emphasize are first, that *quantitative techniques are always, inescapably embedded in qualitative techniques*, a point often lost when we consider whether particular points should be addressed in quantitative or qualitative fashion; and second, that when we are assessing the larger significance of research projects, quantitative or qualitative, *our judgments always involve some reliance on what appear here as "process tracing" and "counterfactual" techniques, as well as always corrigible ceteris paribus assumptions*. I stress these points because I think we in the discipline need to remind ourselves, over and over again, that achieving greater rigor in our work can never simply be a matter of finding better statistical techniques for dealing with uncertainties or even of designing randomized field experiments, useful as those things are. Political science research inevitably rests on debatable premises about how we should define and categorize what we observe and also on debatable estimates about what particular research findings tell us about how larger political processes operate. Work can be and, I fear, often is rigorous in regard to its statistical analyses of observations but not very thoughtful about the categories used to create those observations. Research may also produce reliable causal arguments about particular patterns of political behavior, but these seem exciting only because scholars incorporate them into much broader claims about politics, explicit or implicit. Much in those broader claims is not rigorously examined or defended, or sometimes even acknowledged.

Let me elaborate those points by reference to a couple places in Gerring's text where he makes legitimate arguments in ways that I'd have him put differently. On p. 34, Gerring has a sentence that is potentially misleading: he says "only in...case studies does qualitative analysis comprise a significant portion of the research." He means that, once constructed, a large-n cross-case analysis doesn't rely on qualitative methods to explore its particular causal claims, and I agree with that. On p. 41, Gerring also has a potentially misleading claim when he says "evidence gathered from a cross-case research design can be interpreted in a limited number of ways. It is therefore more reliable." He means that once we understand the definitions and causal hypotheses deployed in a cross-case research project, it is hard to, for example, interpret findings of no statistical significance as proving statistical significance, and I also agree with that.

The reasons those sentences can nonetheless be misleading are made evident by points Gerring makes on pp. 52–53 and 69–71. There he notes that when we construct a set of observations such as "cases of democracy" or "rich" and "poor" nations and then judge particular cases as sufficiently similar to be counted together or sufficiently different to be counted apart, we are relying on theories of what political phenomena are, theories on how the world works, and judgments of what really matters that all represent what he terms "an ontological element of research design." Subsequent

quantitative research can tell us whether studies defined with certain assumptions produce statistically meaningful results, but whatever those conclusions may be, they cannot do all the work of justifying our initial categorizations and assumptions of what count as decisive similarities or differences. Our quantitative data sets are embedded in broader "visions of the world as it really is," as Gerring puts it, and that means they are imbedded in accounts that have inevitably been arrived at through processes that have been far more "qualitative" than purely "quantitative," if we must use that dichotomy. To be sure, particular assumptions can be subjected to quantitative empirical tests and it is often valuable to do so. But that doesn't alter the embeddedness of quantitative research in qualitative judgments, because it is never possible to test all the assumptions that go into constructing our quantitative tests. We run into a kind of infinite regress—in principle we'd have to test all the assumptions that go into our research designs for testing the assumptions of our main research design, and then we'd have to test all the assumptions that go into our research designs for testing the assumptions of the research designs for testing the assumptions of our main research design, etc. There's no way out from relying on some ontological judgments, as Gerring rightly stresses.

This is admittedly the old point that all research designs are "theory laden," but I think it worth making, because it means that in cross-case studies, qualitative methods *do* form a significant portion of the overall research endeavor. We make partly qualitative or at least non-empirical judgments when we deem something a "democracy" and when we draw a boundary line for what constitutes a "rich" or "poor" nation, and since those are fundamental to the whole statistical inquiry, how can they not be a significant portion of the research?

This "theory-laden" feature of all political science research also means that, though within the assumptions used to construct a particular cross-case study, there may be a limited number of ways of interpreting the results, there are likely to be lots of ways of challenging the assumptions, and therefore dismissing or reinterpreting the results. If we say, for example, that the U.S. was not truly a democracy until the 1965 Voting Rights Act, lots of results based on counting it previously as a democracy have to be reinterpreted. If we say a nation is not truly "rich" whatever its GNP unless levels of economic inequality in its population are relatively low—otherwise, we might say, some elites are rich, but the nation understood as the people is not really rich—then, again, particular statistical results might be reinterpreted in a much wider range of ways.

I have similar concerns about Gerring's discussions of "counterfactuals" and "process tracing" later in the book. He presents these as methods that, despite real limitations, deserve to be "in the toolkit of social science." Again, though I agree with his discussions of these methods and their limitations, I think his formulation may understate the role that counterfactuals and process tracing inevitably and always play, not in conducting particular research projects, but in our broader disciplinary assessments of the significance of those research projects. Just as we can't, even in principle, rigorously quantify and test all the assumptions that go into our research

designs, we can't ever test ALL the assumptions that we employ when we decide what to make of a particular, solid research finding. Take, for example, the influential finding of Green and Gerber (2004) in randomized field experiments, that voters are more likely to respond when contacted personally door-to-door than when they receive a recorded phone message or a postcard. That is a rigorously arrived-at, reliable finding; but what are to make of its significance?

For lots of campaigns that were investing heavily in recorded phone messages, the lesson has seemed clear: spend more money on door-to-door. But note what they're also assuming. They're assuming that campaigns that contact voters in some fashion are more likely to win elections than those that do not—a plausible assumption, but not one that is self-evident if most voters actually respond to certain sorts of dramatic events or media presentations or informal "word of mouth" discussions far more than any actual campaign contact. It might still be wiser to arrange the crisis (perhaps by tempting your opponent in an airport men's room) or to find the magic media message or seek to spark a "word of mouth" campaign. Another, perhaps more telling point: those involved in campaigns also assume that winning elections is important, presumably because doing so will put into power people and policies they wish to have in power. But perhaps power and policies can be more effectively won outside electoral processes, through lobbying, gifts, coercion, blackmail, or simply by becoming so essential to the economy that everybody has to pay attention to you however they got elected. If that's true (and I am not asserting that it is), it doesn't undercut the Green and Gerber finding, as far as it goes; but it greatly alters our sense of the importance of that finding for understanding politics.

And I believe that point can be generalized: any time we attribute broader significance to a research finding, we are implicitly rejecting a whole set of untested counterfactuals that might undercut not the rigor or reliability, but the broader import we are attaching to the research; and we are also implicitly adopting an understanding of how the political world works that has features like those Gerring rightly attributes to process tracing—reliance on heterogeneous sorts of evidence to support various chains in the causal claims and on many untested "contextual assumptions and assumptions about how the world works" (Gerring 2007, 185). Though we can and should seek to find evidence for some of those assumptions, we are never going to be able to test them all—some will be left at the level of "background knowledge," which, as Gerring states, "informs *all* causal analysis." When we judge the broader significance of particular research projects, we inevitably place in the context of a broader account of political causes, some of which rests on unexamined "background knowledge."

It may be said that the points I'm making—there are qualitative, judgmental, interpretive elements that are ineradicably part of all research designs and that are ineradicably part of all judgments of the significance of research findings—are true of all types of scientific research, and I agree. But the ontological, interpretive, and causal assumptions that go into con-

structing and assessing political science research tend, not surprisingly, to be more politically charged and controversial than those for many other kinds of research—so I think we have a special obligation to pay attention to these features of our work. As an example, Gerring says on p. 70 that "the social sciences are defined by their focus on decisional behavior—actions by human beings and humanly created institutions that are not biologically programmed." But lots of the social sciences *do* in fact focus on behavior that is claimed to be biologically programmed, and some social scientists in the past and present believe that all behavior is ultimately biologically programmed. The assumptions we make on just how and how far we are biologically programmed are enormously relevant to the research projects we think worth constructing and on how we interpret their significance—and again, though we can test some of our assumptions about biological determinism in specific research projects, we can never test them all. So our work is inevitably premised on and interpreted to advance different visions of human agency in ways that may well be more fraught for the conduct of our political lives than other sorts of research are likely to be.

Let me close with a final point in the same spirit. In making the preceding remarks, I may seem to have moved away from Gerring's main topic, the case study. But I take the broader significance of Gerring's elucidation of case study methods to be an indication of what case studies can and cannot contribute to social science, written, as he stresses, in part because "the methodological status of the case study is still highly suspect" (7). His book does an excellent job of presenting what case studies might and might not be good for and why in regard to causal arguments (as he acknowledges, he presumes without discussion the value of case studies in suggesting fresh descriptions of political phenomena). I think that his arguments are strengthened by stressing, even more than he does, that all social science research projects, cross-case research as much as case studies, are constructed on what most in the field would view as "qualitative" conceptual foundations and interpreted through broader understandings of how the world works that are for the most part not subjected to rigorous testing, instead representing the conventional world views that constitute "everyday" or "background knowledge" or "common sense," world views of a sort that historically have often come to be discredited over time, at least in part.

That point is only underlined by recognizing that all our research designs involve ways of making credible "*ceteris paribus*" claims—because these claims are perhaps especially difficult to make in the social sciences. Scholars like Jamie Druckman have been arguing, for example, that we should be very careful about moving from lab experiments to claims about how people process political information in the far more complex environment of actual everyday life, because the lab context is *not* likely to approximate all the factors that may influence such processing in more conventional settings. Scholars like Lynn Sanders (forthcoming) and Sarah Igo (2006) have stressed how survey research, too, is an unconventional context producing results that may well not map onto what happens in the settings of actual political decision-making (though

they may then influence behavior in those settings). Even randomized field experiments ultimately rely in part on a measure of faith—that all the unobserved, unstudied factors that might influence outcomes, of which there are inevitably a large number, are randomly distributed among our test groups. It may always turn out down the line that this wasn't the case, that *ceteris* was not *paribus*, so we cannot regard the results of even these research designs with complete confidence.

Because the sorts of characteristics and limitations I've been detailing are features of all political science research, including case study research and cross-case research, they admittedly say nothing about the general question of whether case studies or cross-case studies are preferable. But they do indicate that case studies are not necessarily inferior because they involve qualitative elements, including counterfactuals and process tracing and imperfect means of making *ceteris paribus* claims. And keeping these features of our work in mind also can help us to consider, when we get down to actual research projects, whether or not a particular case study is more wisely constructed than a cross-case study that speaks to the same general topics. I think that if we do so, we will find that many case studies continue to make valuable contributions to the progress of political science, as Gerring says. And his book will help us all to do them better.

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On the Road to Consensus? A Reply to Coppedge, Lieberman, Mahoney, and Smith

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First off, let me thank the participants in this symposium for their insightful comments. The first nightmare of every author—*no one will read my book*—is clearly assuaged. Of course, this serves to raise the second nightmare of every author: *Christ, they've actually read it!*

I am relieved to discover that there are many points of agreement among the participants of this symposium with respect to the methodology of the case study. This is not so surprising, since I appropriated the wisdom of the ages—including work by members of this symposium—in writing the book. Even so, one does not always find consensus, even

when one is seeking it. Perhaps this is a sign that we are beginning to realize a degree of cumulation in the field.

To be sure, whatever consensus may exist in the field does not yet extend to the definition of key concepts, which remains hotly contested, as this symposium attests. However, I would suggest that beneath the semantic debates there is increasing consensus over key methodological principles. In the present instance, while there is disagreement over what definition we ought to adopt for “case study,” I venture to guess that *for any given definition* the authors represented in this symposium would agree on a similar set of methodological precepts. That is, “If *case study* means X, Y follows. If, on the other hand, *case study* means Z, Q follows.”

If I am correct in this diagnosis, then much of the debate that we are now experiencing in the field is largely semantic in nature. I do not wish to imply *merely* semantic, for surely the definition of key terms is an important issue. But let us not overstate the *Sturm und Drang*.

I shall follow convention in focusing my comments on areas of apparent disagreement. This is usually more enlightening, or at any rate more entertaining. In doing so, I shall endeavor to combine issues addressed by several authors. These fall into six general categories: (1) the distinction between studies that are theory generating and those that are theory testing, (2) the distinction between case studies and single-outcome studies, (3) the problem of representativeness, (4) the experiment as a template for case study research design, (5) pragmatic considerations impinging upon the choice of cases, and (6) process tracing. In a final section I briefly discuss a grab-bag of additional issues about which I have less to say.

Theory Generating and Theory Testing

Michael Coppedge suggests that we maintain a clear distinction between work whose purpose is theory-generating and work whose purpose is theory-testing. He argues that case studies—understood here as a generalizing form of analysis in which the scope of the inference extends to a larger population—are useful for the former but not for the latter.

I agree that the case study strategy is usually more compelling when employed in an exploratory mode than in a confirmatory/disconfirmatory mode (as discussed in chapter 3). I would not go so far as to dismiss the use of case studies in the latter mode, however, even when the purpose is to validate (or invalidate) an inference that extends to a larger population of cases.

Consider, for starters, that where the population of an inference is small, each individual case matters to a degree that it does not (all other things being equal) where the population is large. If one is generating insights into country-level relationships operative within Western Europe, for example, an intensive investigation of a single case, or several cases, may be sufficient to refute a theory. This is a simple matter of numbers. One case within this restricted population constitutes roughly one-twelfth of the population (depending upon how one defines “Western Europe”), two cases one-sixth, and so forth. The case study mode is virtually axiomatic. (I

presume here that the small scope of the theory can be justified by the author, and is not an arbitrary domain restriction.)

A second circumstance is found in a population where variation on key dimensions (the outcome and/or the explanatory variable of interest) is limited, i.e., a theory about rare events. Again, it seems virtually axiomatic that those cases where variation occurs (or at least some of them) should be studied intensively, i.e., in a case study fashion, and that the results of that study might lead one to alter one's priors on the theory in question.

A third circumstance involves populations that are large, with ample variation on key parameters, but where evidence (e.g., quantitative data, archival sources, access to key informants) is available only for one or several sources, effectively prohibiting a cross-case approach to the problem at hand. Here, too, the case study format may be employed in a theory-testing manner—not because it is so methodologically compelling but rather because the alternative mode of investigation is simply not feasible.

A different sort of opportunity for case study research in the theory-testing mode is provided where the theory in question elaborates specific and testable hypotheses about causal mechanisms. It is often the case that mechanisms linking the exogenous cause with the outcome of concern can only be effectively tested in a small number of cases, perhaps because of data insufficiencies. This is sometimes referred to as “pattern matching” (following Donald Campbell). (Coppedge explores the logic of this style of analysis, but does not view it as viable when testing a general theory.)

So, Coppedge and I agree that a cross-case format for theory-testing is superior. But I think Coppedge fails to acknowledge that it is not always possible, by virtue of limitations encountered in the research domain. In these circumstances, a case study mode of theory-testing is eminently justifiable; it is, indeed, the best available method. Whether it produces a high level of confidence in results—a decisive acceptance or rejection of the theory—may be doubted. But the same skepticism also usually greets theory-testing in a cross-case mode. Indeed, even where the sample is large and the treatment can be randomized across groups, results from a single test are rarely sufficient to prove or falsify a general proposition (because of doubts about replicability or external validity). Confirmation/disconfirmation is perhaps better articulated as a relative matter—a narrowing of confidence intervals or decreasing variance between prior and posterior distributions.

Case Studies and Single-Outcome Studies

Mahoney argues that a primary use of the case study method is to explain single cases rather than a population of cases. If so, my definition of the case study effectively defines out of the subject its largest and most important area of employment. On one level, the issue is semantic—how shall we choose to define the term? In French, *analyse de cas* implies the analysis of a single outcome within a single bounded unit. In English, however, when one says one is conducting a case study one is usually implying that a broader population

is at issue. A case study is a study of a subject broader than the case itself. If, let us say, the object of one's investigation is to explain the outbreak of World War I, an author will say that she is writing a *study*. If, on the other hand, her objective is to shed light on wars in general (or some subset of wars), then the same study of World War I will be articulated as a *case study*. Thus, I think that my usage is consistent with ordinary (English) language, and *a fortiori*, with language within the (Anglo-American) social sciences.

Whatever one's choice of terms, it is absolutely vital to distinguish among studies that seek to elucidate the causal features of a single outcome and those that seek to elucidate the features of a broader population of cases (using one or several cases as examples). In order to distinguish these different methodological objectives, I have coined a new term—single-outcome study—for the former. It matters not whether others adopt this term; it does matter (contra Lieberman), that we distinguish these two forms of analysis. Despite some commonalities, they are distinctive, as the epilogue makes clear.

But let us address the larger, and surely more important, point. Perhaps single-outcome studies are the best defense, and best employment, of the case study method. The point is well taken. If one's interest is to explain World War I, one really ought to conduct an intensive study of World War I. This goes more or less without saying—or so I thought. Perhaps it should have been granted greater emphasis. Even so, one should not neglect the ways in which a broader population of cases—perhaps interrogated through large-N cross-case analysis—might also help to shed light on that particular outcome, a matter that both Coppedge and Lieberman have written about. Within-case *and* cross-case techniques for evaluating single-outcomes are thus explored in the epilogue.

Before moving on, I should call attention to a pervasive ambiguity. Coppedge points out that case studies are often employed to answer the following genre of question (which he refers to as *intensive* testing): Does theory X explain case Y? Or, more open-endedly, which extant theory best explains case Y? The ambiguity is that the resulting study may be regarded either as a test of a general theory (a case study, in the sense in which I employ the term) *or* as an attempt to explain a single case (a single-outcome study, using my vocabulary). Often, both moments are present in the same study. Thus, Graham Allison's famed study of the Cuban Missile Crisis may be read as a case study or a single-outcome study.

One's view of this debate is necessarily colored by a larger question that I have not sought to resolve—namely, whether studies of individual cases *are* or *ought to be* focused on broader populations or on outcomes specific to each case. I assume that the latter style of “idiographic” analysis is typical of the humanities and of traditional historical studies. I believe that the former is more resonant with the goals of the social sciences (at least in the Anglo-American world), where the objective is usually to construct general models of human behavior, rather than (or in addition to) the accumulation of knowledge about specific cases. We are a species that is drawn—some might say prone—to *theory*. If so, then this book quali-

fies as a methodology appropriate for case studies in the social sciences, but not (or to a lesser extent) for case studies as they are approached in history and the humanities, or on the interpretive end of our discipline.

The Problem of Representativeness

Coppedge points out that there is always reason to doubt whether a sample of one (or two or three) is representative of a much larger population. This is central to his skepticism about the viability of case studies when employed to test theories (as opposed to generating hypotheses). I concur. However, it is important to clarify that the problem of representativeness (i.e., bias) is a product of three factors: the size of the sample, its method of selection (random selection, relative to the dimensions that might affect the outcome of interest, is superior), and the presumed homogeneity of the underlying population. Briefly, a larger sample is better, though increases in size bring diminishing returns. Randomization is superior to purposive selection if the sample is large, though purposive elements may be incorporated into the randomization procedure (via stratification of the sample) and purposive case selection can overcome some doubts about representativeness, as discussed in chapter 5. Finally, claims of representativeness are more questionable wherever the population of interest is highly heterogeneous; likewise, issues of sample size and sampling techniques are less important where the underlying population is highly homogeneous. Thus, Wittgenstein felt justified in choosing any native speaker of a language (e.g., his maid) in order to discover its underlying properties; there is, in other words, no value in taking a random sample of units that are effectively uniform with respect to the phenomenon under study.

I raise these familiar issues only to underline that problems of representativeness are not unique to case study forms of investigation. Consider the problem faced by crossnational studies where the population is (a) limited (to 190 or so nation-states), (b) impossible to sample from (without running into degrees-of-freedom problems and problems of sample bias), and (c) highly heterogeneous. Indeed, the whole notion of sampling (and with it the statistical theory that it undergirds) is questionable in this context.

Consider the proposition that federalism contributes to economic growth. What is the true population of this common inference? Is it (a) societies within the developing world? (If so, one is compelled to divide the world into developed and undeveloped societies, a difficult matter.) Is it (b) democracies only (in which case the division between regime types must be policed)? Is it (c) all nation-states from 1900 to 2007? Is it (d) all nation-states since the founding of nation-states (another ambiguous dividing line)? Or perhaps (e) all *possible* nation-states (including those that exist as well as those that might have existed)?

These questions are vexing because none of these putative populations can be tested in a rigorous manner. They are matters of presumption—"ontologies," to use Peter Hall's much-abused term. (The same points are made eloquently by Smith in his commentary.) All are plausible, but some seem

more plausible than others—depending upon one's theory of federalism and growth. Of course, one can test various hypotheses. My point is that such tests, by themselves, are not always definitive. One cannot conclude, for example, that because there is a strong empirical relationship between federalism and growth in the developing world—and not in the developed world (or vice versa)—that the theory is valid in the former but not the latter. One must be wary of defining the population of an inference as "that sample which yields the best results." Evidently, the author must provide a coherent theoretical argument for why the inference is valid in context A but not in context B. And this takes us beyond the realm of empirics, *tout court*.

The Experimental Template

Mahoney suggests that the experimental template, introduced in chapter 6, is inappropriate for case study analysis because case studies—unlike experimental studies—are strong on internal validity, and weaker on external validity. Yet, surely, this is a similarity rather than a difference. Indeed, experimental work is often criticized for its lack of generalizability. By contrast, large-N cross-case samples drawn from "natural" (non-experimental) data usually have a stronger claim on external validity (because samples are drawn randomly from natural settings), but weaker claims on internal validity. My usual response to claims based on large-N representative samples drawn from surveys of the general public is that while it might not be true for the sample, if it is true for the sample it is also likely to be true for the population.

(Note that because the treatment is manipulated by the researcher in an experimental research design it is usually possible—and always desirable—to have large treatment and control groups. This moves the style of analysis into a large-N cross-case format, and explains why the traditional equation of case study research with observational data is generally true—though not true by definition.)

Pragmatic Considerations

Lieberman raises the point that cases are often chosen for pragmatic reasons, e.g., because a writer has special funding, access to, or knowledge about a particular research site. The same pragmatic factors, of course, also often come into play in large-N cross-case research. One hears—informally—that research on a topic was spurred by an author's discovery of a new dataset, learning a new technique of analysis, or the development of new software (making easy what had heretofore been impossible or computationally expensive). We recognize these pragmatic imperatives, which may be justified on grounds of efficiency. We look for keys under lamp-posts because that is where the light shines.

However, they do not constitute *methodological* defenses of a particular research design. Because I happen to speak a certain language, or be conversant with a particular quantitative technique, is no reason for employing that linguistic or statistical technique in a particular instance. The latter choice must rest on methodological grounds, if it is to be justified at all. Thus, to say that I am studying Syria be-

cause I speak Arabic, or come from Syria, is akin to saying that I am employing a hierarchical linear analysis because I know how to do it (and I have a nifty software program that allows me to do it, just by punching a single key). Neither should be taken seriously, except as a matter of expedience. Both are varieties of the well-known methodological syndrome: “I have a hammer and now I am looking around for a nail to hit with it.” Problems—not techniques or special skills—should lead the way in our investigations, as Smith emphasizes in his comments for this symposium (and in other published work).

It is important, therefore, to distinguish carefully between justifications that are prudential and those that are properly methodological. This is why pragmatic concerns are given short shrift in the book.

Having said this, I realize—and the book recognizes—that case-selection strategies are sometimes justified methodologically only after the fact. This is particularly so when the point of the investigation shifts from a context of discovery to a context of verification (from theory-proposing to theory-testing). And it also the case in situations where cases are chosen by reason of expediency. I see no problem with this (though strict Popperians will demur), so long as there happens to be a strong *ex post facto* justification for choosing this case rather than another. Again, the question of interest rests on methodological issues, not expedience. So, I agree with Lieberman that the problem of (*ex ante*) case-selection sometimes resolves into the problem of (*ex post*) case-justification. The same might be said for many large-N cross-case analyses, which also exhibit a continual back-and-forth (soaking and poking) between methods and results.

Process Tracing

Lieberman cites insufficiencies in the chapter on process tracing—the last chapter in the book, the shortest, and perhaps the least satisfying. There is a mystical quality to this technique of analysis, which has an empirical component but rests largely upon background assumptions about the case and/or about human behavior in general. Because it is so heavily context-dependent, it is not clear whether any general methodological precepts apply. That is, one cannot tell a good process-tracing study from a poor one unless one knows a great deal about the case under investigation. It is these particular judgments about plausible counterfactuals—what A would have done, or could have done, at a particular point in time—that render this form of analysis convincing or unconvincing. By contrast, other aspects of the case study fit more neatly into the conventional methodological frameworks that we apply, for example, to large-N cross-case research (either experimental or observational). So, while I recognize the defects in my own treatment of this issue—and both Craig Thomas (who collaborated on this chapter) and I wish we could have gone further—our defense is that there may not be too much more to say from a general methodological perspective. Few rules or principles, beyond those that we set forth in chapter 7, seem to apply. Or we have not yet stumbled across them. (The apparent obviousness of the subject matter may prevent us from accurately perceiving what is going on.)

Lieberman also suggests that process tracing, rather than being an element of case study analysis, may be its *defining* characteristic. As with the previously vetted debate (over whether the term is properly arrogated for studies that generalize rather than particularize), there is plenty of scope for rival definitions. However, I find this particular definitional choice might not be very salutary. In effect, it would constrain the sort of evidence and analysis that one would be able to consider (by definitional fiat) whenever one undertakes the intensive study of a single case. Insofar as process tracing is a tool of case study analysis—but not *the* tool—a broader definition of the term “case study” is justified.

Additional Points, Briefly Considered

I agree with Smith’s admonition (echoing Giovanni Sartori) that a great deal of conceptualizing and theorizing goes on prior to (or coincident with) a scholar’s empirical engagement with a subject. This sort of “qualitative” exercise (in the sense that it takes place with concepts and *a priori* logic rather than with datasets) underlies all research, whether case-based or based in large-N cross-case samples. As such, it lies outside the scope of the book, which does not purport to cover the subject of methodology generally, but only one particular method. But it is an important, and neglected, point nonetheless.

I agree with Lieberman and Mahoney that the focus of this book on causal analysis omits an important category of case-based work whose purpose is largely *descriptive* (i.e., interpretive). In this, I followed the lead of the broader field of methodology, where we have yet to come to grips with the challenge posed by descriptive inference (a subject that I am currently working on).

Coppedge wonders whether a cross-sectional time-series analysis would qualify as a dynamic comparison? Of course it would, but it would not qualify as a form of case study analysis, and as such is not envisioned as part of the typology presented in chapter 6.

Lieberman wonders whether the pathway case is a truly distinct method of case selection. I have argued (in chapter 5) yes. This method builds on the logic of the crucial case, first elaborated by Harry Eckstein. However, since Eckstein envisioned the crucial case—and its softer versions, the least- and most-likely cases—as studies that would test the existence of causal relationships, it appears that we require a different term for situations in which the covariational relationship (the causal effect) has been proven through cross-case analysis, but we still don’t know much (or are uncertain) about the causal mechanisms that might be at work. In these situations, one promising technique focuses on one or a few cases with the following characteristics: (a) the causal factor of interest has a large effect on this case (as judged by the changing residuals for the case when this factor is entered and removed from the cross-case model) and (b) the case lies close to the predicted outcome in the full model (its residual is small when the causal factor of interest is included, along with other relevant controls). This case-selection technique is quite different from others sketched in chapter five. Whether it will

prove useful to scholars is another matter, and remains to be seen (see Nome 2007).

Finally, a *mea culpa*. In the book, I appropriate the term “nested analysis” (from Lieberman) for a style of single-outcome analysis that employs large-N cross-case evidence. Clearly, Lieberman has in mind both generalizing and particularizing styles of case study when he uses the term. (I should have chosen Coppedge’s term—nested induction—which is consistent with what I had in mind.) One of the gratifications of post-publication symposia is the opportunity to set the record straight. Sorry, Evan!

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Representation Revisited: Concepts, Typologies, and Case Selection

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Empirical scholars in political science have generally allowed normative theorists to conceptualize key concepts such as democracy, accountability, and representation (Collier and Adcock 1999; Pitkin 1967). For some empiricists, taking the time to revisit the very concepts that they are purportedly measuring and testing seems at best too philosophical, and hence out of their domain (although see Goertz 2006 for a comprehensive treatment of social science concepts). Consequently, the empirical literature on representation has focused too heavily on statistical roll-call analyses, which to a certain degree can help us ascertain the extent to which legislators represent their constituents in legislatures and Congress. Substantive representation, however, involves much more than how legislators vote. In order for political scientists to understand why, we must think carefully about what representation involves. This essay will examine the concept of representation by briefly considering what normative theorists such as Jane Mansbridge and Hanna Pitkin have said about the subject, and then analyzing the concept of representation through the lens of what more recent empirical researchers have said about concepts, typologies, and case selection.

Normative Conceptions of Representation

Starting with Pitkin (1967), political scientists have regarded representation as either descriptive, substantive, or symbolic. Descriptive representation refers to citizens being represented by legislators who share particular demographic characteristics (race, gender, or ethnicity), while substantive representation involves legislators representing citizens’ interests or particular preferences. Symbolic representation re-

fers to descriptive representation without the substantive component. Scholars of black representation have debated the merits of which type of representation is most effective, with Swain (1993) arguing that substantive representation is what really counts, while Mansbridge (2003) places more value on descriptive representation. Mansbridge, however, is a normative theorist who has argued that descriptive representation is essential for advancement of minorities and women in the American political system. Pitkin’s analysis did not really deal with minority representation, but the concept she presented has been extended to such studies. To date, no work on racial representation has challenged Pitkin’s conceptual framework or analyzed the concept of representation using more recent empirical research on methods.¹

The Concept(s)

What does it mean to be represented? What does it mean to represent others? We are always asking others to represent us either before a lawmaking body, a court, or other institution. When one is represented by an attorney before a court, the attorney acts in the material interests of her client. It does not matter whether the attorney looks like her client physically. As long as the attorney defends her client well, then the client will be satisfied. In terms of *political* representation, however, surely more is at stake than just material interests. As Aristotle observed long ago, political issues deal with how we ought to order our lives together in the larger community. Additionally, politics deals with how individuals will be treated, including policies such as affirmative action, immigration, and English Only laws, just to name a few. This distinction is crucial because when Pitkin talks about descriptive and substantive representation, she is referring to political representation. As Gerring explains, concepts are not static and “progress in the cultural sciences occurs, if it occurs at all, through changing terms and definitions” (Gerring 2001). This may seem like a minor point because a normative political theorist is obviously referring to political representation. When we are trying to think clearly about concepts, though, it is important to consider the ladder of abstraction in order to avoid conceptual stretching and, at the other extreme, narrowing our concepts to infinitesimal degrees. Additionally, issues of race and representation have dramatically changed since Pitkin’s exegesis, which requires conceptual revisions.

As Gerring points out, it matters how we define our terms when we conduct empirical research. When researcher A speaks about democracy, she may mean something completely different than researcher B. While King, Keohane, and Verba (1994) insist that researchers choose observable and thus testable concepts, many of the most intriguing questions in the study of politics involve seemingly unmeasurable concepts. One cannot deny that the study of terrorism is appropriate for political scientists, yet understanding what motivates terrorists is hardly a simple question since it defies many of the primary motives for human behavior, such as utility maximization, and self-preservation (KKV 1994). Instead of giving up on conceptualizing difficult concepts, it would be better to tackle some of the most difficult substantive political problems and develop a

clear conceptual framework by which to proceed.

Ragin’s analysis of concepts stems from his belief that cases should be seen as configurations. That is, all too often, social scientists have viewed variables as independent of each other. With regard to representation, scholars have dichotomized representation by splitting the concept into descriptive and substantive. Ragin would argue for a configuration where these two concepts are placed in a spatial continuum in a way that acknowledges the diversity, albeit limited, of the concept of representation. As Ragin explains, membership in sets is “often partial” and rarely do we find cases that are either in or out of a given category (Ragin 1999). Ragin’s solution to this dilemma is the use of “fuzzy sets,” an analytic tool aimed at studying variation by degree without sacrificing differences in kind. It is not at all clear that Ragin’s solution is the only one available to social scientists interested in making sure that such differences are accounted for in their research. More attention to conceptualization using traditional methods can achieve the same goal.²

In the area of representation, most empirical researchers have chosen to use Pitkin’s dichotomy in order to test whether given groups are being adequately represented. For the most part, scholars in this research tradition have argued that substantive representation is what really matters, although many scholars argue that there is intrinsic value in descriptive representation based on issues of justice such that certain groups, especially women, should have some parity in political institutions (Young 1997). Is representation, though, either descriptive or substantive? Are there not instances in which descriptive representation is a necessary condition for substantive representation? If so, how can we conceptualize this?

These questions have sparked my curiosity because it seems to me that many of the arguments regarding which type of representation matters deal more with conceptualization than with how representation per se is measured. Hardly anyone would disagree that descriptive representation without substantive representation is not worth very much. However, the real question is whether those who represent districts with different demographic characteristics can adequately represent their constituents despite the physical difference. In this sense, the scholarship on racial representation suffers because of a lack of attention to the conceptualization of representation.³

Figure 1 shows interactions of types of representation, and whether particular combinations yield high, mixed, or low representation. Note that the difficult combinations are the medium substantive representation cells. To some degree, those who descriptively represent their constituents and provide medium substantive representation are more representative than those with the same degree of substantive representation minus descriptive similarity. This dichotomy, then, between substantive and descriptive that persists in the literature ignores the hard cases, and lumps them into one or the other. Research in this area needs to focus on the hard cases, and consider representation not as an either/or proposition, but as a complex concept that needs to be analyzed using methods that are attuned to this complexity.

Figure 1: Interactions of Descriptive and Substantive Representation

	Descriptive	~Descriptive
Low Substantive	Low Representation	Lowest Representation
Medium Substantive	Mixed	Mixed
High Substantive	Highest Representation	High Representation

Typologies

How should representation be conceptualized? The previous section offered a critique of the prevailing norms of research regarding the issue of political representation. Given the complexity of this concept, it is fitting to explore the many different cells that comprise the “property space” of the concept of representation.⁴ A typology is a device for “partitioning events into types that share specified combinations of factors (Stinchcombe 1968).” In the context of representation, then, the table above would describe the different typologies of representation. Typologies can be complex or simple. Typologies for democracy can become quite complex, while typologies for approval ratings for the President are quite simple. In the area of representation, I am arguing that the prevailing typology is too simple, and that we need to further complicate this concept into a more encompassing typology, much in the same way Elman (2005) has done for the study of international politics. This must be done in order to understand the meaning of representation in this democratic system.

Once the typological space is established, one can systematically rule out certain combinations that do not exist in the real world. In the case of representation, I have only sketched a possible typology above, in which all cells of the rudimentary matrix exist in the real world. Of course, this typology is a simple one with few cells. For future research in this area, as well as comparative work on democratic development, it is important that theories using typologies look beyond hypotheses and examine all possible cases of a given outcome.⁵

As discussed in the previous section, Ragin has offered a framework that assesses the importance of typologies and their implications. Ragin rightly points out that many researchers do not know where to proceed once their typology is fleshed out. In the tradition started by Lazarsfeld’s property space typology construction, Ragin sets forth his configurational approach as the heir to Lazarsfeld. Through functional reduction, it is argued, researchers can narrow the domain of researchable cells. In the case of racial representation, it is conceivable to imagine various scenarios of elected representatives and their demographic characteristics, such as

black, urban, Democratic members from majority Latino districts, and so on.

Gerring offers insight into precisely how to create a typology for social scientific concepts. In his analysis of ideology, Gerring notes that typologies are usually created in several ways. First, he notes that empirical researchers can appropriate the definition of a classic work on the concept, which in the case of representation has been Pitkin's framework. Secondly, scholars can adopt a "causal-explanatory" understanding of the concept, by which he means that a concept can be described by what explains it. For example, in the case of representation, one can say that to represent and be represented is one of the key aspects of democratic systems, and thus one can only examine representation in terms of how the represented and the one representing interact. Gerring proposes a schema that focuses on "specific definitional attributes" of the given concept (Gerring 2001). Researchers should formulate a minimal definition of the concept and an ideal-type definition, as well.

What Gerring and Ragin have in common is their call for researchers to be more conscientious about the way they are using concepts in their research. Gerring calls for a criterial framework while Ragin calls for employing fuzzy set methods. Either way, the message is clear: Think carefully about conceptualization and create typologies that accurately represent the concept being measured.

Case Selection

Once one performs adequate conceptualization and typology formation, one of the most challenging tasks is to ensure that cases are chosen carefully. Very little agreement exists on how to best choose cases.⁶ As Gerring (2001) points out, one of the main goals of social science is achieving representativeness in a given sample. That is, we should avoid bias in the cases we choose. With respect to the concept of representation, then, it is crucial to choose cases that are representative of the type of representation we are trying to study. For example, it would be unrepresentative to generalize about Mexican-American members of Congress by only selecting Rep. Henry Bonilla (R-TX) for an interview because he is the only Mexican Republican in Congress and his views are not representative of many Mexican-Americans.⁷ Gerring suggests that random samples are always solutions to selection bias, but in the case of racial representation in Congress, it would be foolish to pick a random sample of a population of 20 members of Congress.

One of the cardinal sins of social science is selection bias.⁸ Selection bias occurs when selection takes place on the dependent variable, thus biasing conclusions in many instances. Because qualitative research is more susceptible to selection bias, special attention must be given to methodology in order to avoid criticism that is often unjustified. Geddes argues that many works of valuable comparative politics, such as Skocpol's work on social revolutions, have a place in political science, but that they do not advance theoretical knowledge. Much in the same way William Riker argued for a more scientific political science, Geddes (1990) approaches the is-

sue of selection bias by framing qualitative research as unscientific and inferior to more rigorous statistical research. Similarly, KKV also discuss the differences between cases and observations and criticize qualitative researchers for conflating the two in many instances. KKV prefer to use the term "observation" because of its precise single-unit connotation.

According to Collier, researchers have not considered the distinction between plurality of causes and causal heterogeneity. Plurality of causes involves an outcome, which is caused by unrelated variables, while causal heterogeneity involves the property of the data in relation to the model being tested. Simply adding more cases is not always compelling in terms of correcting selection bias. Collier points out, however, that it is not always "sinful" to select on the dependent variable as long as one does so carefully, with full knowledge of the implications. In many cases, what many deem selection on the dependent variable is merely counting mistakes, as in the case of Achen and Snidal's analysis of deterrence theory.⁹

With respect to studies of representation, Fenno's seminal work on members of Congress has formed the basis by which other studies of representation have proceeded. His research design involved carefully selecting cases in order to ensure regional and political representativeness. He did not merely interview whomever he had the opportunity to interview and observe. He carefully selected subjects based on pre-ordained criteria in order to ensure representativeness. He did not assume that more and more interviews would be the answer to selection bias. Had he only chosen Democrats, his study would have been biased based on party affiliation. In a similar way, he chose African-Americans as well as white members of Congress in his analysis.

Conclusion

To date, the literature on representation has borrowed from Pitkin's framework, which to a large extent has much utility. However, issues of race, ethnicity, and the surrounding debates on redistricting call for a reconceptualization of representation with special attention given to the various ways of defining the concept, including the typologies used, as well as careful selection of cases when researching representation.

Does representation involve more than just how legislators vote? If so, then scholars need to look at other ways of representation, including bill sponsorships, and the different manifestations of representativeness. This is not an easy concept to measure. While this paper focused exclusively on conceptualization and, to a small degree, case selection, the challenge for researchers in this area is to think carefully about concepts and how to accurately measure them.

Notes

¹ This is not the case regarding gender representation. See Celis (forthcoming) for a thorough review of representation from a women's studies perspective.

² It is outside the scope of this article to fully analyze fuzzy set methods. Ragin's work, however, calls attention to the problems with variable-oriented research.

³ This is not the case in the literature on gender and representation,

where there has been more of an effort to address these important issues. See Phillips 1998, Young 1997, and Celis (forthcoming) for explicit treatments of gender and representation.

⁴ See Lazarsfeld and Barton (1951) for more on the issue of property spaces in political science research.

⁵ At the same time, however, we should be mindful of “the risks of reification and of relabeling anomalies,” as Elman (2005) warns.

⁶ Van Evera (1997, 88) offers a list of 11 criteria for case selection, with a matrix aimed at making criteria for case selection easier for graduate students writing their dissertations.

⁷ Please indulge the unfortunate relationship of the concept of representation with representativeness.

⁸ See Achen and Snidal (1989), who refer to selection bias as an “inferential felony with devastating implications.”

⁹ Collier’s ideas given at IQRM in Tempe, AZ in January 2003.

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A Note on Causality and Causal Mechanisms

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In his Introduction to the symposium on Alexander L. George and Andrew Bennett’s *Case Studies and Theory Development in the Social Sciences*, published in *Qualitative Methods* in the spring of 2006, Jack S. Levy predicts that this book “is likely to be highly influential and widely cited—and also, quite properly, will serve as a target of criticism” (2006, 34). In this brief note, I shall raise two points of fundamental criticism not explicitly raised in the contributions to the symposium—although David Dessler touches upon them from a different angle (2006, 44)—with respect to George and Bennett’s understanding of causality and causal mechanisms against the background of David Hume’s classic treatment of causation in *A Treatise of Human Nature*.

In chapter 7, which is concerned with case studies and the philosophy of science, George and Bennett discuss the approach of explaining phenomena via causal mechanisms, which, according to them, “has gained a wide following among social scientists and philosophers of science” (George and Bennett 2004, 135). In that chapter they also pay attention to the close connection existing between the “epistemology of causal mechanisms and the methodology of process-tracing” (2004, 129; cf. also Wendt 1999, 82). This close connection derives from their conviction that explanation by means of causal mechanisms “draws on spatial contiguity and temporal succession” (2004, 140). Mechanism-based explanations “are committed to realism and to continuousness and contiguity in causal processes” (ibid.). In chapter 10, which deals with process tracing and historical explanation, they return to this subject. Process tracing has many advantages for theory development and theory testing, because it “attempts to identify the intervening causal process—the causal chain and causal mechanism—between an independent variable (or variables) and the outcome of the dependent variable” (2004, 206). They have to admit however that “process tracing provides a strong basis for causal inference only if it can establish an uninterrupted causal path linking the putative causes to the observed effects” (2004, 222).

Leaving aside for the moment the question of what they mean when they speak of causal mechanisms, I wish to point out that George and Bennett are confronted with a problem when they suggest that the requirements of “spatial contiguity” and “temporal succession” with respect to causation go back to David Hume (cf. 2004, 140), because Hume makes very clear that contiguity and succession are not enough to produce the idea of causation. In *A Treatise of Human Nature*,

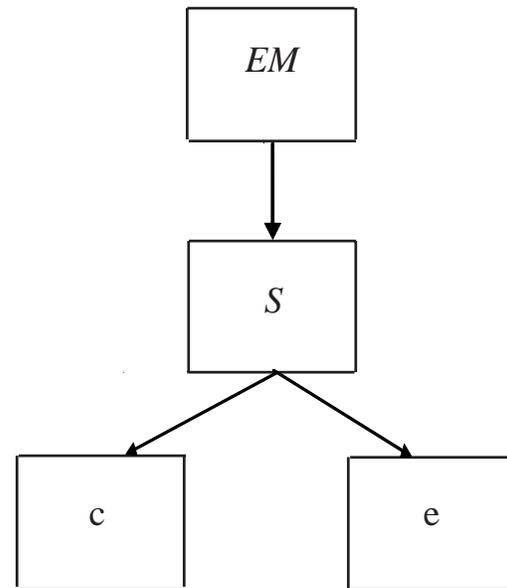
Hume explains that, although contiguity and priority in time are certainly elements of a causal relationship between two objects, “an object may be contiguous and prior to another, without being consider’d as its cause” (Hume 1978, 77). What is vital in the case of a causal relationship, so Hume argues, is that there exists a “necessary connection” between two objects, and “that relation is of much greater importance, than any of the other two above mentioned” (ibid.). George and Bennett might object that it is precisely the function of causal mechanisms to provide this notion of necessity, but the point remains that, as opposed to Hume, they have not explored the nature and implications of this necessary connection, and their failure to do so leads to a further problem.

To illustrate the relevance of process tracing, George and Bennett provide the example of the 50 numbered dominoes standing upright in a straight line on a table in a room, where only dominoes number 1 and number 50 are visible. They are asked to leave the room for a few moments and, when they return, they notice that numbers 1 and 50 have fallen with their tops pointing in the same direction. They then ask, “does this mean that either domino caused the other to fall?” (2004, 206). In view of Hume’s discussion of causation, the latter surely would have found this an odd question to ask. After having established that causation involves the idea that there exists a necessary connection between two objects, Hume wonders whence we derive this idea of necessity, and reaches the conclusion that, “when I cast my eye on the *known qualities* of objects [dominoes 1 and 50 in the George and Bennett example], I immediately discover that the relation of cause and effect depends not in the least on *them*” (1978, 77; emphasis in original). It is impossible to explain an effect (domino 50 has fallen in a certain direction) by a cause (domino 1 has fallen in the same direction) alone, for “there is no object which implies the existence of any other if we consider these objects in themselves” (1978, 86). In this way, Hume discovers that in the empirical domain necessity and inevitability do not exist.¹ As opposed to what George and Bennett apparently believe, in the empirical world there do not exist “uninterrupted paths” linking causes to effects. A certain effect cannot be explained by one or several causes alone, since it is possible to derive any kind of effect whatsoever from some cause, seeing that, in the words of Hume, the mind “can always *conceive* any effect to follow from any cause, and indeed any event to follow upon another” (1978, 650; emphasis in original).

George and Bennett assume that a certain cause or convergence of causes can be necessary and sufficient to explain the occurrence of an effect (cf. their discussion of forms of causal processes [2004, 212–3], as well as their discussion and graphic depiction of spuriousness [2004, 185–6]). Their model of a causal explanation looks like this: $(c) \rightarrow (e)$. Hume’s discovery however entails that this cannot be true. Every causal explanation of a certain kind of behaviour or state of affairs must contain a third element at least. This element enables us to reduce the number of possible effects from a certain cause. Without this third element all effects are equally likely. This third element of a causal explanation has been called a “point of view” (cf. Popper 1971, 259), an “explanatory principle” (cf.

Hayek 1967, 11–14), and a “connecting principle” (cf. Smith 1980, 45), but in this note I prefer the term “explanatory mechanism.” Such a mechanism explains why cause (c) and effect (e) belong together.² To be a little more precise: we can deduce from an explanatory mechanism (EM) the statement (S)—usually called a “hypothesis” or “law”—that describes the conjunction of (c) and (e).³ Consequently, the model of a causal explanation has to have the form shown in Figure 1.

Figure 1: The Model of Causal Explanation



Following Wittgenstein, Karl Popper summarizes Hume’s discovery that nothing is inevitable in the empirical domain in a similar fashion:

A necessity for one thing to happen because another has happened does not exist. There is only logical necessity... the necessary link between *a* and *b* is neither found in *a* nor in *b*, but in the fact that the corresponding ordinary conditional (or “material implication”)... follows *with logical necessity* from a law of nature—that it is necessary, relative to a law of nature. (Popper 1980, 438, cf. also Popper 1971, 363, emphasis in original)

We may conclude from Hume’s discovery that cause and effect do not exist independently of ourselves. Cause and effect are, to paraphrase the historian R.G. Collingwood, no mere objects, something outside the mind that knows them, but the result of an activity of thought (cf. Collingwood 1957, 218). The explanatory mechanism does not “link” cause and effect (cf. Hedström and Swedberg 2004, 7), it “creates” them. It is the researcher who decides which of the infinite number of possible observable phenomena will be causes and effects and which not. Whether the researcher is conscious of this thought process is an entirely different matter.

The explanatory mechanism introduced above explains *why* (c) and (e) go together, why things are as they are. A good ex-

ample of an explanatory mechanism is the axiom underlying almost all theorizing on human behaviour, namely that the probability that a human individual will choose a certain behavioural option is proportional to the balance of expected costs and benefits that the individual assigns to this behavioural option, and the balance of expected costs and benefits assigned to the other behavioural options subjectively available to it, and the subjectively estimated probability that these costs and benefits will materialize. This type of mechanism can moreover be equated with the hard core of a Lakatosian research programme, since it satisfies Zahar's criterion that the hard core "when taken in isolation be unfalsifiable, i.e. it ought to be metaphysical in Popper's sense" (Zahar 1983, 248).⁴

At first sight it seems obvious to equate my explanatory mechanism with George and Bennett's causal mechanisms, especially as they consider causal mechanisms "as bases for inference and explanation" (2004, 11), and approvingly quote Salmon where he states that the mechanism-based approach "makes explanatory knowledge into knowledge of the hidden mechanisms by which nature works" (2004, 134). However, a closer look makes clear that this would not be correct. Already in their first chapter, George and Bennett give an indication that their causal mechanisms have a totally different character. They state that causal mechanisms are "independent stable factors that under certain conditions link causes to effects" (2004, 8), where the term "factors" suggests that they are part of the empirical domain. In the section dedicated to defining causal mechanisms (2004, 135–45), they moreover distance themselves from Hedström and Swedberg's definition of causal mechanisms as "analytical constructs that provide hypothetical links between observable events" (2004, 135; Hedström and Swedberg 2004, 13), and explain that they "prefer a scientific realist definition that places causal mechanisms on the ontological level" (135–6). This reinforces the impression that in their opinion causal mechanisms are somewhere out there in the world, waiting to be found, and not, which is my position, of our own making as a result of our attempts to understand why things are as they are. George and Bennett are "causal mechanism realists."⁵ If we use the terminology introduced by Karl Popper (cf. Popper 1982, 180–7), then their causal mechanisms are World 1 objects (they exist on the "ontological level," as they put it), while my explanatory mechanisms, being products of the human mind, clearly belong to World 3.

Clearly, we differ with respect to our interpretation of the ontological status of mechanisms, but when they stipulate their definition of causal mechanisms, things become less clear. According to George and Bennett, causal mechanisms are "ultimately unobservable physical, social, or psychological processes through which *agents with causal capacities* operate, but only in specific contexts or conditions, to transfer energy, information, or matter to other entities" (2004, 137; my emphasis). Apparently, causal mechanisms cannot be observed, which agrees with my interpretation of explanatory mechanisms, but how can they then be part of the empirical domain? Gravitation, attraction at a distance, explains why an apple falls to the ground, but gravitation itself cannot be observed. It turns out that in their interpretation—taking their cue from John Stuart Mill's *System of Logic*—causal mechanisms are things that cannot yet be observed, but as our

instruments improve, they will become observable, and presumably will end being causal mechanisms (cf. 2004: 143).⁶ Attraction at a distance seemingly does not qualify as a causal mechanism. In view of Hume's discovery, it is of course also problematic that their definition makes clear that agents have causal capacities *independently* of the causal mechanism. When Yee argues that "causal mechanisms are 'ontologically prior' to causal effects because one cannot have a causal effect without an underlying causal mechanism," a position that comes close to the one I have taken up in this note, George and Bennett reply that this is "true but trivial," as "causal effects and causal mechanisms are equally important components of explanatory causal theories" (2004, 138). This is all very confusing. We now have on the "ontological level" agents with causal capacities, causal effects, as well as, unobservable, causal mechanisms (which presumably take up the role of causes and no longer connect causes with effects), and George and Bennett leave us without a clue how agents, effects, and mechanisms are related to one another, let alone how they can be reconciled with Hume's argument on causation in *A Treatise of Human Nature*.

One last point I wish to draw attention to is that from my observation on the metaphysical nature of the explanatory mechanism above, it can be deduced that all our explanations of empirical phenomena necessarily are as-if explanations, in the sense that there is no requirement that the mechanism itself must accord with reality.⁷ We cannot demand that the mechanism be realistic, since such a demand would involve us in an infinite regress.⁸ George and Bennett, however, reject as-if explanations (cf. also Bennett's contribution to the symposium (2006, 47).⁹ They claim that these cannot solve what they call "the barometer problem" (2004, 132–3 and 140) of mere covariance—which evidently is untrue, as an explanatory mechanism explains why things are as they are—and that "advancing beyond the boundaries of our knowledge requires that we make our assumptions [and presumably our causal mechanisms] as accurate as possible" (2004, 142). They are adamant that causal mechanisms, which are vital for the explanation of processes, should be grounded in reality. It is clear that they have failed to realize that Hume's discovery implies that the mechanisms that "lift the fog over and make transparent the world in which we live" themselves must be elements of a "phantom world" (Hernes in Hedström and Swedberg 2004, 100).¹⁰

Notes

¹ Unlike Alexander Wendt (1999, 80–1), I believe that the successes of modern sciences have not undermined the validity of Hume's sceptic conclusions one bit.

² It should be noted that the explanatory mechanism itself cannot be observed. As Hume argued, we only possess the *idea* of causation. If we wish to test the hypotheses or laws that can be derived from the mechanism, we always need to introduce certain existential assumptions of place and time.

³ An explanatory mechanism accordingly is no substitute for a scientific law; it creates such a law. Neither is it the antonym of a law (cf. Elster in Hedström and Swedberg 2004, 48). The hypotheses derived from an explanatory mechanism can moreover be stated in probabilistic terms: "whenever (c), then (e) with a certain probability." It is not necessary that a scientific law asserts that "an event of a

given type (the cause) will always produce an event of some other type (the effect)” (ibid.).

⁴ According to Popper, an assertion is metaphysical if it has no empirical content, which is true of the explanatory mechanism formulated in the text; it cannot be falsified.

⁵ I thank the editor of this newsletter for this felicitous phrase.

⁶ Mill: “If the facts are rightly classed under the conceptions, it is because there is in the facts themselves something of which the conception is itself a copy; and which if we cannot directly perceive, it is because of the limited powers of our organs and not because the thing itself is not there” (Mill 1851, 304).

⁷ It is precisely in this sense that Popper is correct in claiming that all our explanations are of the “known by the unknown” (Popper 1968, 63).

⁸ If we demand that a mechanism must accord with reality (a kind of empirical test in advance), then we run into the problem of how to test that this is indeed the case. This can only be done by means of a theory from which the mechanism in question can be deduced in the shape of a hypothesis. If it subsequently turns out that, for the time being, this hypothesis cannot be refuted, then we are allowed to use the mechanism in our explanations. However, the test theory from which the mechanism has been derived in the form of a hypothesis rests on several assumptions, which in turn need to be tested for their verisimilitude using other test theories, and so on ad infinitum. On the same basis, the demand formulated by Milton Friedman that assumptions “to be important must be...descriptively false” (the notorious “F-twist”), must also be rejected (Friedman 1966, 14). How could we establish that an assumption meets this demand? Only by formulating a test theory, etc., etc. Cf. also Jeremy Bentham’s defence of the “principle of utility”: “is it susceptible of any direct proof? It should seem not: for that which is used to prove everything else, cannot itself be proved: a chain of proofs must have their commencement somewhere. To give such proof is as impossible as it is needless” (Bentham 1982, 12–13).

⁹ Jeffrey Checkel also believes that mechanisms move us away from “correlational arguments and as-if styles of reasoning toward theories that capture and explain the world as it really works” (Checkel, forthcoming).

¹⁰ In this sense, mechanisms are indeed “surreal” (Leplin 1987, 520), but they are no surrogate for realism (cf. Lyons 2003, 896), as realism cannot do without them.

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Multi-Methods Research in the RNGS Project: Building a (Data) Bank While Crossing the Bridge

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From its creation in 1995, the Research Network on Gender Politics and the State (RNGS) has purposefully taken into account work that seeks to bridge the qualitative-quantitative divide in its large-scale study of interactions between women’s policy agencies and women’s movements—state feminism—in Western postindustrial democracies. As the lead conveners of RNGS, we describe here our journey across the bridge to a multi-methods approach and announce the release of our dataset, a major part of that journey, now available on the RNGS website at <http://libarts.wsu.edu/polisci/rngs>.

Stepping Away From the Territory of Case Studies

RNGS was formed to design and conduct a study that would contribute to theoretical knowledge on women’s policy

agencies, women's movements, and state feminism through rigorous empirical work. The network was a response to the methodological deficiencies of an initial study of women's policy agencies in 14 countries, *Comparative State Feminism* (1995). Our goal was to assess research questions, not by doing in-depth descriptive case studies of countries as a whole, but by using a single framework to gather information on individual debates across five issue areas in each country—job training, abortion, prostitution, political representation, and a “hot” issue of national importance. With this action, the network increased the potential number of cases, conformed to guidelines for causal inference, and opened the possibility for exploring variations in state feminism within countries, over time and across policy areas through a most-similar systems design.

The model developed by RNGS offered a set of hypotheses to test the extent to which governments respond to movement demands on the issues (dependent variable), what aspects of movement resources and the policy environment account for variations in these responses (independent variables) and the importance of activities and characteristics of women's policy agencies (intervening variables) in achieving state action favorable to movement demands. Once the model was finalized, the network turned to defining operationally its components and providing guidelines to researchers to select and study three policy debates in each of the policy areas for each country in the study.¹ A primary focus in this stage was the validity of definitions—whether they would permit researchers to gather the empirical information envisioned by each concept.

By the end of the first foray onto the bridge, we had learned some solid lessons for narrowing the qualitative-quantitative divide. On the positive side, treating the observations as cases, not case studies, increased the theoretical power of the study, particularly given that the issue books used the model to present the debate analyses in each country and to analyze the core hypotheses in each issue area. On the negative side, the number of permutations and combinations of dependent variables, two intervening clusters of variables, and two clusters of independent variables made it virtually impossible to provide any definitive, detailed statements about which factors precisely were generating which women's movement outcomes.

Going All the Way across the Bridge to Quantitative Territory

With these limitations in mind, RNGS decided to go all the way across the bridge, turning its attention to creating a dataset that measured the model quantitatively and that would lead to a more sophisticated statistical testing of the hypotheses. The group first collaborated to reconceptualize the components of the original model in a way that would lead to quantifiable observations. Next, we designed two worksheets, one on debates and the other on women's movements, to collect from researchers information not available from their initial studies. Data collection sheets were then completed and the information was coded after an initial check for inter-

coder reliability.

The lengthy process produced a dataset with 130 observations. The four major clusters of variables from the original RNGS model expanded to 29 concepts with 160 sub-variables. The SPSS dataset is accompanied by a codebook and text appendices for each debate. Each concept in the codebook contains the following: sub-variables, nominal definitions, operational definitions, analytical questions, measurement, data collection worksheet information, and key decisions about operationalization made by network members and consultants. Since validity requires that nominal and operational definitions coincide with scholars' understanding of similar concepts, each section also cites relevant literatures.

Finding Another Path through QCA

With the issue books nearing completion, a new methodological problem arose: how to compare the results across issues, over time, and between the different countries? The initial qualitative model produced observations on 13 different variables for each case. This complexity mushroomed with over 160 standardized variables for the 130 separate debates in the dataset. The qualitative work had convinced many of us that explanations of state responses and the importance of women's policy agencies would be found by comparing smaller sets of cases and examining the interrelations among conditions. Ragin's *Qualitative Comparative Analysis*, the case-oriented approach that uses Boolean algebra to sort through different causal paths to similar outcomes, appeared to us as a potentially useful tool to assess these complex causal relationships.

Getting to the Other Side through Triangulation

With the case analyses from the issue books, a QCA “crisp set” dataset—based on dichotomies—and a numerical dataset, RNGS has moved into the last phase of analysis: a capstone study, to be published in book form, that assesses propositions from the RNGS model and from adjacent areas of theory. Case studies, statistical analysis, and QCA will be used in each thematic chapter. At the end, we will conduct a final meta-analysis of our findings and compare the results of each methodological approach. Major considerations will include: To what degree is triangulation an effective method for interpreting findings and theory building? And what happens to the validity of the measurements as one moves from case studies to QCA and to causal inference? It may very well be that while a multi-methods approach makes room for complexity, it is less likely to produce clear findings than single-method approaches.

The RNGS foray across the bridge into the territory of multi-methods research presents researchers with an arguably unprecedented large-scale example of triangulation. At the same time, the full scientific impact of our journey is still to come. Nonetheless, the RNGS project stands to make an important contribution to the ongoing dialog about bridging the qualitative-quantitative divide.

Notes

¹ An issue book was published on each of the five areas.

Book Notes

Book descriptions are excerpted from publisher's websites. If you would like to recommend a book to be included in this section, email Joshua C. Yesnowitz, the assistant editor of QM, at jcyesnow@bu.edu.

Dul, Jan and Tony Hak. 2007. *Case Study Methodology in Business Research*. Amsterdam: Elsevier.

The complete guide for how to design and conduct theory-testing and other case studies? *Case Study Methodology in Business Research* sets out structures and guidelines that assist students and researchers from a wide range of disciplines to develop their case study research in a consistent and rigorous manner. It clarifies the differences between practice-oriented and theory-oriented research and, within the latter category, between theory-testing and theory-building. It describes in detail how to design and conduct different types of case study research, providing students and researchers with everything they need for their project. The main aims are to present a broad spectrum of types of case study research (including practice-oriented case studies, theory-building case studies, and theory-testing case studies) in one consistent methodological framework; emphasize and clearly illustrate that the case study is the preferred research strategy for testing deterministic propositions, such as those expressing a necessary condition case by case and that the survey is the preferred research strategy for testing probabilistic propositions; stress the role of replication in all theory-testing research, irrespective of which research strategy is chosen for a specific test; and give more weight to the importance of theory-testing relative to theory-building. *Case Study Methodology in Business Research* is a clear, concise, and comprehensive text for case study methodology. Templates are supplied for case study protocol and how to report a case study.

Goertz, Gary and Jack S. Levy (eds.). 2007. *Explaining War and Peace: Case Studies and Necessary Condition Counterfactuals*. London: Routledge.

This edited volume focuses on the use of “necessary condition counterfactuals” in explaining two key events in twentieth-century history, the origins of the First World War and the end of the Cold War. Containing essays by leading figures in the field, this book analyzes the causal logics of necessary and sufficient conditions, demonstrates the variety of different ways in which necessary condition counterfactuals are used to explain the causes of individual events, and identifies errors commonly made in applying this form of causal logic to individual events. It includes discussions of causal chains, contingency, critical junctures, and “powder keg” explanations, and the role of necessary conditions in each. *Explaining War and Peace* will be of great interest to students of qualitative analysis, the First World War, the Cold War, international history, and international relations theory in general.

Moses, Jonathon W. and Torbjørn Knutsen. 2007. *Ways of Knowing: Competing Methodologies in Social and Political Research*. London: Palgrave.

This new textbook on methodology in social and political science focuses on the debate between positivist and constructivist approaches. It introduces a range of key issues—from the nature of knowledge to the strengths and weaknesses of the main research

methods—showing how methodological pluralism can be combined with intellectual rigor.

Smithson, Michael J. and Jay Verkuilen. 2006. *Fuzzy Set Theory: Applications in the Social Sciences*. London: Sage Publications, Inc.

Fuzzy set theory deals with sets or categories whose boundaries are blurry or, in other words, “fuzzy.” This book presents an accessible introduction to fuzzy set theory, focusing on its applicability to the social sciences. Unlike most books on this topic, *Fuzzy Set Theory: Applications in the Social Sciences* provides a systematic yet practical guide for researchers wishing to combine fuzzy set theory with standard statistical techniques and model testing.

Article Notes

Abdelal, Rawi, Yoshiko M. Herrera, Alastair Iain Johnston, and Rose McDermott. 2006. “Identity as a Variable.” *Perspectives on Politics* 4:4, 695–712.

As scholarly interest in the concept of identity continues to grow, social identities are proving to be crucially important for understanding contemporary life. Despite—or perhaps because of—the sprawl of different treatments of identity in the social sciences, the concept has remained too analytically loose to be as useful a tool as the literature’s early promise had suggested. We propose to solve this longstanding problem by developing the analytical rigor and methodological imagination that will make identity a more useful variable for the social sciences. Our paper offers more rigor and precision by defining collective identity as a social category that varies along two dimensions—content and contestation. Content describes the meaning of a collective identity. The content of social identities may take the form of four non-mutually exclusive types: constitutive norms, social purposes, relational comparisons with other social categories, and cognitive models. Contestation refers to the degree of agreement within a group over the content of the shared category. Our conceptualization thus enables collective identities to be compared according to the agreement and disagreement about their meanings by the members of the group. The final section of the paper looks at the methodology of identity scholarship. Addressing the wide array of methodological options on identity—including discourse analysis, surveys, and content analysis, as well as promising newer methods like experiments, agent-based modeling, and cognitive mapping—we hope to provide the kind of brush clearing that will enable the field to move forward methodologically as well.

Boas, Taylor C. 2007. “Conceptualizing Continuity and Change: The Composite-Standard Model of Path Dependence.” *Journal of Theoretical Politics* 19:1, 33–54.

Political scientists studying institutional development face the challenge of accounting for both continuity and change over time. Models of path dependence based on increasing returns, inspired by the example of the QWERTY typewriter keyboard, have played an important role in the analysis of institutional continuity, but they have been criticized for their inability to accommodate change. In this article I present an alternative model of path dependence inspired by the example of the Internet, a technology that has changed fundamentally since its invention. The composite-standard model of path dependence illustrates how complex political institutions subject to increasing returns can evolve gradually over time through a changing

mix of lower-level component parts. By incorporating mechanisms of institutional change, such as conversion and layering, within an increasing returns-based theoretical framework, the composite-standard model highlights new interconnections among these previously distinct processes and offers new insights into the nature of long-term political change.

Bennett, Andrew and Colin Elman. 2007. "Case Study Methods in the International Relations Subfield." *Comparative Political Studies* 40:2, 170–95.

This article reviews the key role that case study methods have played in the study of international relations (IR) in the United States. Case studies in the IR subfield are not the unconnected, atheoretical, and idiographic studies that their critics decry. IR case studies follow an increasingly standardized and rigorous set of prescriptions and have, together with statistical and formal work, contributed to cumulatively improving understandings of world politics. The article discusses and reviews examples of case selection criteria (including least-likely, least-and-most-similar, and deviant cases); conceptual innovation; typological theories, explanatory typologies, qualitative comparative analysis, and fuzzy-set analysis; process tracing; and the integration of multiple methods.

Billordo, Libia and Adina Dumitru. 2006. "French Political Science: Institutional Structures in Teaching and Research." *French Politics* 4:1, 124–34.

Previous research in this journal has analyzed publication trends in the top political science journals in France. An inventory of published articles in the *Revue Française de Science Politique* and *Politix* since 1970 has provided information on methodological preferences and subfield coverage, thus allowing for meaningful comparisons with trends in American political science. This essay identifies the scholarly output of the teaching and research institutions of French political science and examines the methodological requirements of the graduate programs of political science departments in France.

Goertz, Gary. 2006. "Assessing the Trivialness, Relevance, and Relative Importance of Necessary or Sufficient Conditions in Social Science." *Studies in Comparative International Development* 41:2, 88–109.

Political scientists of all stripes have proposed numerous necessary or sufficient condition hypotheses. For methodologists, a question is how can we assess the importance of these necessary conditions. This article addresses three central questions about the importance of necessary or sufficient conditions. The first concerns the "trivialness" of necessary or sufficient conditions. The second is how much a necessary or sufficient condition is "relevant." The third important question deals with the relative importance of necessary or sufficient conditions: for example, if X_1 and X_2 are necessary or sufficient conditions, is one more important than the other? The article develops measures to assess the importance of necessary or sufficient conditions in three related contexts: (1) Venn diagram, (2) 2x2 tables, and (3) fuzzy sets. Two empirical examples are discussed at length: (1) Skocpol's *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* and (2) Ragin's (2000) analysis of the cause of IMF riots.

Grendstad, Gunnar. 2007. "Causal Complexity and Party Preference." *European Journal of Political Research* 46.1, 121–49.

Qualitative Comparative Analysis (QCA) overlaps logistic regression in explaining events, but challenges the latter's lack of accounting for causal complexity. QCA has only to a limited degree been applied to large-N studies or individuals as cases and has not incorporated the logic of probability. QCA and logistic regression are compared with respect to logic, procedure, and outcome. Political orientations from five national surveys are adapted to the requirements of the two methods. The methods are demonstrated on explanations of individuals' party preferences. QCA and logistic regression converge and overlap in identifying degrees of causal complexity, in ascertaining model significance, and in identifying antecedents to party preference. Results differ in degree, not in kind. A slightly more nuanced picture emerges using the QCA approach, whereas logistic regression delivers greater parsimony. Choice of method(s) is not arbitrary. QCA can easily be used on any large-N research problem. It should apply probability when appropriate.

Häge, Frank M. 2007. "Constructivism, Fuzzy Sets and (Very) Small-N: Revisiting the Conditions for Communicative Actions." *Journal of Business Research* 60:3, 512–21.

Fuzzy set techniques, both as a methodological and theoretical tool, can engage in a fruitful liaison with constructivist research. Several important properties of fuzzy set analysis overlap with constructivist theorizing and research practice. In particular, fuzzy set methods are compatible with and support research based on a holistic ontology and on detailed qualitative comparisons of cases. To demonstrate the usefulness of the approach, a comparative case study investigating the conditions for communicative action using fuzzy sets is replicated and re-interpreted. The result of the replication is an improvement of the informational content, the precision, and the validity of the conclusions drawn from the empirical analysis. Furthermore, the re-interpretation points to theoretical and conceptual issues that need more consideration in future research. From a methodological point of view, the article shows that fuzzy set techniques are useful research tools even in instances where the number of studied cases is very small.

Hancock, Ange-Marie. 2007. "When Multiplication Doesn't Equal Quick Addition: Examining Intersectionality as a Research Paradigm." *Perspectives on Politics* 5:1, 63–80.

In the past twenty years, intersectionality has emerged as a compelling response to arguments on behalf of identity-based politics across the discipline. It has done so by drawing attention to the simultaneous and interacting effects of gender, race, class, sexual orientation, and national origin as categories of difference. Intersectional arguments and research findings have had varying levels of impact in feminist theory, social movements, international human rights, public policy, and electoral behavior research within political science and across the disciplines of sociology, critical legal studies, and history. Yet consideration of intersectionality as a research paradigm has yet to gain a wide foothold in political science. This article closely reads research on race and gender across subfields of political science to present a coherent set of empirical research standards for intersectionality.

Ishida, Atsushi, Miya Yonetani, and Kenji Kosaka. 2006. "Determinants of Linguistic Human Rights Movements: An Analysis of Multiple Causation of LHRs Movements Using a Boolean Approach." *Social Forces* 84:4, 1937–55.

This paper applies a Boolean approach to examine the social background of movements for linguistic human rights. Predictive determinants to explain the occurrence of LHRs movements in this study included linguistic diversity within a country, literacy rate, population size, national income as an index of affluence, and the existence of a constitution supporting those rights. Data for 159 countries were collected and analyzed using a Boolean analysis. The result of the analysis shows that there are four combinations of economic and linguistic conditions that cause LHRs movements in a country. A further analysis with varying cutoff values reveals that the combination GD (higher gross income AND linguistic diversity) is the "strongest" condition for LHRs movements in the four combinations.

Kittel, Bernhard. 2006. "A Crazy Methodology? On the Limits of Macro-Quantitative Social Science Research." *International Sociology* 21:5, 647–77.

Despite the great popularity of macro-quantitative comparative research in the social sciences during the past two decades, it has only had a limited lasting impact on the development of our understanding of social macro-phenomena. The lack of robustness appears to be symptomatic of research findings. The cause of this problem is the difficulty in dealing with complex macro-phenomena by means of statistical analysis. If international comparative research relates to independent and identical behaviour of individuals, which can be portrayed at the macro level by the idea of the representative agent, the analysis is indeed tricky, but not impossible. However, this road is closed for macro-level characteristics of social systems, since the model cannot be based on assumptions about modal behaviour. In this instance, the sole solution seems to be to accept the limits of small numbers and to improve the elaboration of a macro-narrative based on robust micro-correlations.

Levi-Faur, David. 2006. "Varieties of Regulatory Capitalism: Getting the Most of the Comparative Method." *Governance* 19:3, 367–82.

The rationale behind this special issue's stepwise analysis of cross-sectoral and cross-national variations and similarities of regulatory reforms is explained. The processes of case selection and inference are clarified and justified. At the same time, the article offers a strategy for an increase in the number of cases without compromising the strength of case-oriented analysis. William Whewell's notion of consilience is employed to (a) justify the inclusion of sectoral as well as national cases; (b) justify different degrees of in-depth analysis according to the inferential role of the case in the research design; and (c) suggest a distinction in the inferential process between comparisons that enhance internal and external validity. The article concludes with a systematic examination of cross-sectoral and cross-national variations in a table that provides a "panoramic snapshot" and "holistic picture" of the combination of variations and commonalities of the cases analyzed.

Levy, Jack S. 2007. "Qualitative Methods and Cross-Method Dialogue in Political Science." *Comparative Political Studies* 40:2, 196–214.

The author accepts the basic argument that recent advances in quali-

tative 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.

Mahoney, James. 2007. "Qualitative Methodology and Comparative Politics." *Comparative Political Studies* 40:2, 122–44.

Leading methods for pursuing qualitative research in the field of comparative politics are discussed. On one hand, qualitative researchers in this field use a variety of methods of theory development: procedures for generating new hypotheses, tools for pursuing conceptual innovation, and techniques for identifying populations of homogeneous cases. On the other hand, they employ both within-case and cross-case methods of theory testing. Within-case methods include techniques for identifying intervening mechanisms and testing multiple observable implications of theories. Cross-case methods include a host of approaches for assessing hypotheses about necessary and sufficient causes. The article discusses the distinctive leverage offered by qualitative research for addressing questions in comparative politics.

Mills, Melinda, Gerhard G. van de Bunt, and Jeanne de Bruijn. 2006. "Comparative Research: Persistent Problems and Promising Solutions." *International Sociology* 21:5, 619–31.

The enduring importance and utility of comparative research in sociology are as old as the discipline itself. Although comparative research flourishes within this discipline, methodological problems persist. After defining comparative research, this article outlines some of its central problems, including: (1) case selection, unit, level, and scale of analysis; (2) construct equivalence; (3) variable or case orientation; and (4) causality. The discussion finishes with a brief introduction of the critical and innovative articles within this special issue that not only address these problems, but also present promising solutions.

Mjøset, Lars. 2006. "A Case Study of a Case Study: Strategies of Generalization and Specification in the Study of Israel as a Single Case." *International Sociology* 21:5, 735–66.

Debates on "case studies and generalization" have been too strongly committed to dualisms (general/specific, explanation/understanding) that polarize social science into natural-science-inspired and humanities-inspired camps. One should be aware of a third option, a *pragmatist* (participationist) attitude. Rather than relying on parallels with external academic fields, this attitude thinks about research with reference to the conduct of social science only. This article discusses these three attitudes with reference to a single case study of the Israeli-Palestinian conflict (asking why that conflict became one of the deepest and most persistent conflicts in recent history). The three attitudes imply different strategies of generalization and specification. The single case study of the Middle East conflict relies on a

pragmatist strategy of generalization, and the rest of the methodological discussion shows how this strategy transcends the general/specific or explanation/understanding dichotomies.

Moses, Jonathon, Benoit Rihoux, and Bernhard Kittel. 2005. "Mapping Political Methodology: Reflections on a European Perspective." *European Political Science* 4:1, 55–68.

This article surveys two concomitant developments in European political methodology. First, we point to a recent methodological convergence across Europe and the Atlantic. Second, we note a broadening methodological divide between explanatory and interpretive approaches to political phenomena. This survey provides a backdrop for introducing a new ECPR Standing Group in Political Methodology as an outlet for new methodological techniques and a venue for exchange across Europe's broad methodological spectrum.

Munck, Gerardo L. and Richard Synder. 2007. "Debating the Direction of Comparative Politics: An Analysis of Leading Journals." *Comparative Political Studies* 40:1, 5–31.

This article contributes to ongoing debates about the direction of comparative politics through an analysis of new data on the scope, objectives, and methods of research in the field. The results of the analysis are as follows. Comparative politics is a rich and diverse field that cannot be accurately characterized on the basis of just one dimension or even summarized in simple terms. In turn, the tendency to frame choices about the direction of the field in terms of a stark alternative between an old area-studies approach and a new economic approach relies on largely unsupported assumptions. It is therefore advisable to focus on problematic methodological practices that, as this study shows, are widespread in comparative research and thus pose serious impediments to the production of knowledge.

Pierson, Paul. 2007. "The Costs of Marginalization: Qualitative Methods in the Study of American Politics." *Comparative Political Studies* 40:2, 145–69.

In the past few decades research relying primarily on qualitative methods has been almost completely marginalized within the subfield of American politics. After outlining the unusual organizational contours of the subfield, the author demonstrates that even as it has been marginalized, qualitative work continues to make very important contributions to scholars' understanding of American politics. Much of the strength of this work comes from its focus on the substance of politics, its configurative approach to explanation, and its attentiveness to the temporal dimensions of social processes. Despite the high quality of much quantitative work in American politics, the marginalization of qualitative approaches has come at a substantial cost, introducing a range of biases and shortcomings in the subfield's main research programs.

Ragin, Charles C. 2006. "How to Lure Analytic Social Science Out of the Doldrums: Some Lessons from Comparative Research." *International Sociology* 21:5, 633–46.

The practice of quantitative research in the social sciences today is dominated by a specific research template that encourages researchers to focus on the net additive effects of independent variables on variation in a dependent variable, using samples drawn from "given" populations. Comparative research, especially case-oriented investigation, offers a number of important challenges to this template. While these challenges may appear to constitute a rejection of conventional

quantitative research, they can be viewed instead as important leads for improving quantitative analysis. The specific challenges addressed in this article center on researchers' conceptions of their populations, their dependent variables, their independent variables (especially the goal of estimating net effects), and the nature of the connections between case aspects.

Steel, Daniel. 2004. "Social Mechanisms and Causal Inference." *Philosophy of the Social Sciences* 34:1, 55–78.

Several authors have claimed that mechanisms play a vital role in distinguishing between causation and mere correlation in the social sciences. Such claims are sometimes interpreted to mean that without mechanisms, causal inference in social science is impossible. The author agrees with critics of this proposition but explains how the account of how mechanisms aid causal inference can be interpreted in a way that does not depend on it. Nevertheless, he shows that this more charitable version of the account is still unsuccessful as it stands. Consequently, he advances a proposal for shoring up the account, which is founded on the possibility of acquiring knowledge of social mechanisms by linking together norms or practices found in a society.

Steinberg, Paul F. 2007. "Causal Assessment in Small-N Policy Studies." *Policy Studies Journal* 35:2, 181–204.

The identification of cause-and-effect relationships plays an indispensable role in policy research, both for applied problem solving and for building theories of policy processes. Historical process tracing has emerged as a promising method for revealing causal mechanisms at a level of precision unattainable through statistical techniques. Yet historical analyses often produce dauntingly complex causal explanations, with numerous factors emerging as necessary but insufficient causes of an outcome. This article describes an approach that renders complex causal narratives more analytically tractable by establishing measurement criteria for ranking the relative importance of component causes. By focusing on subjectively useful measurement attributes, the approach is well suited to the policy sciences' unique combination of explicitly normative aspirations and a commitment to the systematic assessment of causal claims.

Tansey, Oisín. 2007. "Process Tracing and Elite Interviewing: A Case for Non-Probability Sampling." *PS: Political Science and Politics* 40:4, 765–72.

This article explores the relationship between the method of process tracing and the data-collection technique of elite interviewing. The process tracing method has become an increasingly used and cited tool in qualitative research, a trend that has recently accelerated with the publication of Alexander George and Andrew Bennett's text (2005), *Case Studies and Theory Development in the Social Sciences*. That book outlines and explores the process tracing method in detail, highlighting its advantages for exploring causal processes and analyzing complex decisionmaking. Yet while the book presents a rigorous and compelling account of the process tracing method and its critical importance to case study research, the value of the method itself remains contested in some quarters, and there are aspects of George and Bennett's treatment of it that require further exploration.

Thacher, David. 2006. "The Normative Case Study." *American Journal of Sociology* 111:6, 1631–76.

The case study is one of the major research strategies in contemporary social science. Although most discussions of case study research presume that cases contribute to explanatory theory, this article draws from recent literature about ethical reasoning to argue that case studies can also contribute to normative theory—to theories about the ideals we should pursue and the obligations we should accept. This conclusion suggests that contrary to some views (notably Max Weber's), social science has a vital role to play in the prescriptive study of values, particularly so-called "thick ethical concepts" like "leadership," "courage," and "neighborhood vitality."

Awards and Announcements

Giovanni Sartori Book Award for the Best Book Developing or Applying Qualitative Methods, Published in 2006

This award honors Giovanni Sartori's work on qualitative methods and concept formation, especially his contribution to helping scholars think about problems of context as they refine concepts and attempt to apply them to new spatial and temporal settings.

Recipient: Gary Goertz, *Social Science Concepts: A User's Guide* (Princeton: Princeton University Press, 2006).

Committee: Charles Ragin, University of Arizona (chair); Stephen Hanson, University of Washington; Marcus Kurtz, Ohio State University.

Citation: One of the key strengths of qualitative research is that it provides a good platform for the development and refinement of concepts through an ongoing dialogue between ideas and evidence. In this way, concept development is necessarily grounded in cases and contexts. Sartori was critical of the tendency for concepts to be watered down—"stretched," in his words—as they are extended to broader categories and larger *Ns*. Watered-down concepts lack the punch of those anchored in cases, as they lose their foundations in action orientations and causal mechanisms. Indeed, poorly constructed concepts can undermine the very heart of the scientific enterprise: hypothesis testing. This simple Sartorian insight explains political scientists' necessary and habitual "return" to cases.

This year's Sartori Prize winner, Gary Goertz's *Social Science Concepts: A User's Guide*, offers new insights into concept formation, extending many of Sartori's ideas and offering important new tools for concept development. Goertz shows how concepts lie at the core of social science theory and methodology, providing substance to theories, forming the basis for measurement, and influencing the selection of cases. An important theme is the difference between alternate means of concept construction. Goertz explores the construction of complex, multilevel, and multidimensional concepts as he contrasts the classic necessary and sufficient conditions approach to concept building with the family-resemblance approach. Along the way, he provides penetrating, critical discussions of the concepts utilized in several widely used social science theories and datasets, forcing us to reexamine debates about such key themes as the relationship between development and democracy, the causes of the "democratic peace," and the problem of case selection in comparative-historical research.

Goertz's book stimulated several spirited exchanges among the prize committee members regarding his emphasis on developing the

"negative pole" of concepts, his implied criticism of ideal-type concepts, and his approach to the role of theory in concept development.

Alexander George Award for the Best Article or Book Chapter Developing or Applying Qualitative Methods, Published in 2006

This award honors Alexander George's prominent role in developing and teaching qualitative methodology, in particular the comparative case study method.

Recipient: James Johnson, "Consequences of Positivism: A Pragmatic Assessment," *Comparative Political Studies* 39:2 (2006): 224–52.

Committee: Lisa Wedeen, University of Chicago (chair); Jeffrey Checkel, University of Oslo; Thad Dunning, Yale University.

Citation: In "Consequences of Positivism: A Pragmatist Assessment," James Johnson uses Gary King, Robert Keohane, and Sidney Verba's influential book, *Designing Social Inquiry*, to demonstrate how positivist commitments in the discipline have had troubling effects on political and social analyses. Like other positivists, KKV place a premium on observation, according to Johnson. They are therefore wary of explanation because it invokes unobservable processes. This suspicion requires the authors to redefine explanation, reducing the task of explaining to particular notions of inference or generalization. In specifying the precepts of proper research design, this redefined understanding of explanation ends up "inoculating" research from unobservables, privileging observable effects over explanatory causes and ignoring broad consensus among philosophers that "citing causes explain, while citing effects does not" (Hausman 1998: 161–3). KKV and others with positivist philosophical commitments are thus unable to specify the causal mechanisms needed to provide a cogent explanation and to build theory. For Johnson, a positivist theory of inquiry not only impedes our ability to make sense of successful quantitative analysis or to appreciate the value of case studies, it cannot unify the discipline of political science. In contrast, Johnson suggests that pragmatists insist on theory by specifying the underlying, unobservable structures or mechanisms conducive to explanation.

The committee unanimously chose this article for its carefully argued, lucid critique of KKV and its thought-provoking analysis of causal explanation. The essay raised useful, insightful points that had not been previously expressed in the debate over KKV. An important contribution to the logic of inquiry underlying qualitative-comparative methods, Johnson's critique of positivism is refreshing, fair, and constructive. His essay should be required reading for scholars interested in methods.

Sage Prize for the Best Paper Developing or Applying Qualitative Methods, Presented at the 2006 Annual Meeting of the American Political Science Association

This prize 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: Timothy Pachirat, "Ethnography from Below? Reflections from an Industrialized Slaughterhouse on Perspective, Power and the Ethnographic Voice."

Committee: David Waldner, University of Virginia (chair); Regina Abrami, Harvard Business School; Giovanni Capoccia, Radcliffe Institute for Advanced Studies.

Citation: Timothy Pachirat's wonderfully written essay makes a powerful case for bringing ethnography into the qualitative methods repertoire. He argues that the fear of contagion and bias leads most quantitative and qualitative methods to deliberately sequester and distance the researcher from the agents and worlds they study. Ethnography, in contrast, self-consciously seeks to capture the lived experience of those they seek to understand. Pachirat is well versed in current theories of ethnography that highlight the power-laden and hence inevitably partial perspective role of the ethnographer. At the same time, he avoids the controversial claim that positivist methods are mere scientific pretensions. In Pachirat's vision, a self-reflexive political ethnography has an important and independent role to play in the study of politics and power and it can perform that role in tandem with, not in opposition to, non-ethnographic quantitative and qualitative methods.

Announcing a New Award for "Best Dissertation in the Field of Politics and History"

The American Political Science Association's Organized Section on Politics and History is pleased to announce the establishment of a new prize for the "Best Dissertation in the Field of Politics and History." The first such award will be presented at the section's business meeting at the 2008 APSA convention. We welcome nominations of outstanding dissertations from Ph.D.s awarded in either 2006 or 2007. To nominate a dissertation for this award, please send an abstract of the dissertation and a supporting letter from the advisor or other faculty member of the dissertation committee to the section's Secretary/Treasurer, David Robertson, at daverobertson@umsl.edu. The deadline for initial nominations is March 1, 2008. The awards committee—Theda Skocpol (chair, Harvard University), Paul Frymer (University of California, Santa Cruz), and Sheri Berman (Barnard College)—will subsequently be in touch with advisors for full copies of dissertations selected from among this initial pool of nominations.

Qualitative Methods

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