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