

a dissertation committee member on 52 others, including dissertations in sociology and American studies. Six of the dissertations I have supervised have won “best dissertation” awards from the American Political Science Association, in the fields of public law, women and politics, and racial and ethnic politics. One of those also won the best dissertation prize of the Law and Society Association. Seven of the theses on which I have been a dissertation committee member have also won APSA dissertation awards, in public law, racial and ethnic politics, political philosophy, comparative politics, and federalism and inter-governmental relations. These dissertations have won other recognitions as well. Thirty-six have been published as university press books to date, with several currently in production. Most of these dissertations have primarily or exclusively used non-quantitative methods.

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## Symposium: Multi-Method Work, Dispatches from the Front Lines

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### Introduction

<https://doi.org/10.5281/zenodo.997343>

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Multi-method approaches to research have generated considerable excitement in the field of political science in recent years. This is particularly true among graduate students, who are inspired by examples of excellent multi-method work by leading scholars, exhorted by their thesis committees to consider alternative approaches, and above all spurred by the apparent success on the job market of candidates whose research proves they are adept at more than one method.

Yet there are many challenges to using more than one method well, and there is a danger that graduate students in particular will set or be held to unrealistic expectations, that researchers will apply several methods poorly rather than doing one well, and that multi-method techniques will be tacked on to research problems for which they are not necessary or even useful. Perhaps most worrisome, compared to the shelves full of books and articles on one method or another, there is only a very small (albeit growing) methodological literature on how best to combine different methods in the same research design (Lieberman 2005; Gerring and Seawright 2007). The emerging revolution in multi-method approaches has been driven not by methodologists, but by the practitioners of multi-method research, who have pioneered a diverse and innovative set of approaches and techniques. Methodologists are struggling to catch up to and synthesize general lessons from the practices of researchers doing empirical multi-method work.

This symposium reflects this state of play. With the exception of myself as its editor (!), it consists of essays from those who have put multi-method research into practice, and it

reflects their experiences from the front lines. My not-so-hidden agenda was to generate more evidence and insights for those of us either engaged in multi-method research or seeking general principles for its practice. To these ends, I asked each of the contributors to reflect on best practices in multi-method research, on their favorite examples of such research, and on the challenges of doing this demanding kind of research. Although I did not ask the contributors, ranging from current graduate students to senior faculty, to focus on issues specific to their subfields, there are contributors from each of the empirical subfields of political science. The contributors' works also represent a wide mix of different combinations of formal, statistical, and qualitative methods, although there are of course many possible combinations of methods—field work, ethnography, participant observation, experiments, statistical analysis, case studies, formal modeling, simulations, archival analysis, interviews, and others—and not all of them could be included here. Indeed, one measure of the diversity of this research is that while the contributors noted many of their favorite examples of multi-method research, few contributions mentioned the same examples. Thus, the articles represented here are not necessarily representative of the wide range of multi-method research taking place, but they do give a diverse snapshot of the state of this research.

What emerges from this is a fair degree of consensus among the contributors on the promise and difficulties of multi-method research. The authors were drawn to multi-method work by the potential for each method to offset some of the limitations of the others, a process that Thad Dunning calls “triangulating” in his essay. While some of the contributors may have started off with the intention of doing multi-method work for one reason or another, they were for the most part driven to this practice by the desire to understand a substantively important puzzle by whatever methods they could muster. The essays convey the sense that each author emerged

satisfied that in the end they did indeed understand their phenomenon better through having brought a variety of methods to bear. Yet the essays also embody a consensus on the challenges of doing multi-method work as well. Such work can be criticized from all corners on either the job market or by journal reviewers. The formal model or the statistical work might not be the very latest or most sophisticated that their respective methodological communities have devised, the case study work might not cover all the relevant sources materials in all of the relevant languages and archives, the survey or experiment may not have anticipated all possible threats to validity, and so on. There is also a danger that represents the dark side of triangulation, as Thad Dunning points out: Although each method can compensate for the limitations of another, mistakes in any one method can also cumulate when methods are applied sequentially and build upon one another. Accordingly, several contributors share Jason Wittenberg's view that mastery of one method is better than mere facility in several.

The bottom line from these practitioners is that despite the considerable challenges and costs involved, multi-method research is well worth doing. One measure of the high level of interest in this work is that my query to newsletter subscribers for an essay from a current or recent graduate student doing multi-method work led to over a dozen responses. Rather than choosing just one, I asked Scott Siegel, as the most senior of this group, to be the lead author of a piece co-authored by all of the respondents, and each contributor also provided a brief synopsis of their thesis for this newsletter, including their contact information for readers interested in following up on their particular mix of methods. The resulting essay concisely captures the shared experiences and concerns of this key constituency, noting the considerable professional and intellectual benefits of pursuing multi-method work but underscoring as well the tradeoffs in doing so. One challenge here is attaining appropriate training in different methods, especially those not offered at a high level in every department, and the co-authors of this essay underscore the benefits that many of them received from attending training programs at Inter-University Consortium for Political and Social Research (ICPSR), Empirical Implications of Theoretical Models (EITM), and the Institute for Qualitative and Multi-Method Research (IQMR). The authors also stress the need for greater openness to multi-method work, especially such work that includes a qualitative component, in the field's leading journals.

The other essays in this symposium reinforce and build upon these themes. Thad Dunning notes that multi-method research often involves numerous iterations among methods rather than any simple linear progression from one method to another. Some of these iterations are quick and intuitive, while others are more deliberative, methodical, and deductive. He stresses in particular that the study of individual cases can usefully inform the building of formal models. Daniel Carpenter emphasizes this point as well, challenging the use of "as if" assumptions in formal models and urging modelers, as an increasing number of them appear to be doing, to inform their modeling from and test their models in qualitative case stud-

ies, rather than just using selected cases to illustrate models.

Susanne Lohmann, drawing on her experiences as the author of more than two dozen articles (many of them multi-method) in journals such as the *American Political Science Review*, the *American Economic Review*, *International Organization*, *World Politics*, the *American Journal of Political Science*, and the *Journal of Conflict Resolution*, focuses on the problems of getting journal editors to pick appropriate reviewers for multi-method work and getting department chairs and tenure committees to judge such work appropriately. Prosaic as these problems are, Lohmann cogently argues that they are urgent matters for promoting methodological cross-fertilization in the field.

Jason Wittenberg, like several of the other contributors, highlights the importance of first developing a good question and then choosing the methods that give the most leverage on it, rather than starting with methods and asking which questions they can best address. Wittenberg reminds us that the "single country" study can often be disaggregated into multiple case studies by comparisons across sub-units, over time, or across issue areas. He illustrates this point with his own research, in which his "single-country" study became three thousand observations. Wittenberg concludes with an extremely useful summary of seven issues for scholars to consider when contemplating a multi-method project or fieldwork in a developing country.

Finally, Hein Goemans notes the importance of modeling and explaining historically important individual cases even if their underlying mechanisms do not commonly recur. Goemans stresses the importance of being careful with "off-the-shelf" dataset codings, and he urges scholars to try coding themselves five to ten of the cases in any off the shelf dataset they want to use to see how the codings do or do not fit their own concepts and purposes. Goemans enjoins the users as well as the creators of such datasets to exercise responsibility and provide feedback on codings. In this regard, he offers an extremely important and promising suggestion for creating wikis for datasets so that users can provide feedback on case codings. This would require working through several difficult challenges—for example, if codings frequently change, researchers will need to keep track of the codings operative on the date on which they were accessed and perhaps periodically re-check their results as codings change. Also, dataset creators would have to decide whether to update codings frequently or to merely provide Web space for input from users that helps other users adjust their own case codings. Nonetheless, this offers a very promising approach to getting statistical and qualitative researchers to work together on issues of common concern. Hopefully, conferences and workshops directed to this goal can be organized soon. This proposal is of sufficient importance and magnitude that dataset sponsors and field-wide organizations like the NSF and World Bank need to pool their resources and work together to develop suitable protocols for continuously improving dataset codings (for an excellent listing of dozens of datasets on international relations and politics, see <http://garnet.acns.fsu.edu/~phensel/data.html>).

In short, the growing emphasis on multi-method research is one of the most exciting and promising developments in a field that has for far too long been defined by isolated methodological communities dining at separate tables, but fostering this development requires significant institutional changes to make it easier to carry out and publish multi-method work, especially for the graduate students who are the field's future. Multi-method approaches are not for everyone, nor are they suited to every research puzzle, so we should not set unrealistic expectations, especially for our graduate students, about how common multi-method research approaches can or should be. Yet we need to make the field more hospitable for those who do aspire to the ambitious goal of carrying out multi-method research. Journals need to find suitable reviewers for multi-method work, and they may at times need to accommodate the higher word counts and/or Web-based appendices that such work can require. Departments need to ensure methodological pluralism in hiring and promotion, and to accommodate the fact that some research agendas are better suited to articles and others to books. Departments also need to ensure that their graduate students have access to and resources for cross-method training, either in-house or through dedicated methods training programs. These programs, in turn, need to incorporate multi-method approaches not just into their curricula but into their cultures. Finally, organizations devoted to the infrastructure of the field, such as NSF and dataset providers, need to focus on getting the most out of collaborations among scholars with diverse kinds of methodological and substantive expertise. Like the pain of childbirth, the hardships endured in undertaking multi-method research appear to recede in memory as time goes on and the benefits become self-evident. It just doesn't have to be so painful in the first place.

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## *Qualitative Methods as an Essential Complement to Quantitative Methods*

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In this brief essay I will elaborate on some of the points I raised at last year's APSA panel on multi-method work. As always, I emphasize the essential complementarity of different methods. I first briefly discuss why qualitative research and formal models have much to offer each other and why scholars in each methodological tradition can gain much from a better understanding of the other tradition. I then shift to a focus on

the overlooked link between qualitative and quantitative research, to argue for a reconsideration of the requirements of the inputs of quantitative research: What constitutes good data? I close with a proposal that calls for collaboration between qualitative and quantitative researchers to set new standards for the collection and use of large-N datasets.

I propose that qualitative research is a natural partner of formal models and *vice versa* because case studies can actually track the mechanism proposed by the formal model. Until recently, scholars preferred to test the predictions of their formal models with the usual statistical methods. However, as Signorino (1999) showed, the usual statistical methods are very poorly suited to test formal models because they assume observations to be strategically independent (conditional on the explanatory variables). The strategic formal models in political science, in sharp contrast, draw their strength from the insight that decisions are strategically *interdependent*. It is clearly inappropriate to test a theory which poses strategic interdependence of observations with a method that assumes observations are independent. Signorino (1999) and Lewis and Schultz (2003) offer a way out of this predicament by showing how to develop fully structural estimators designed to address the issue. This approach is also advocated in the influential EITM (Empirical Implications of Theoretical Models) workshops. This approach, however, has two distinct drawbacks. First, it assumes that the formal model represents the "Truth" and perfectly captures the data-generating process. A slightly different formal model would require a statistical estimator of its own and might therefore produce significantly different results—even if run on the same data. This is an extremely heavy load for a model to bear—as most modelers would admit. Second, it assumes the model represents a pattern that regularly appears. However, the strength of a formal model does not derive from its ability to explain a great many cases, as long as it can explain some (hopefully important) cases that other models can not explain. Formal models of war, for example, do not necessarily claim that all wars are caused by their particular mechanism, only that the mechanism occurred at least once and could occur again.

In comparison to both the older and more recent statistical methods, the case study method seems a more fruitful and more suitable method to empirically examine formal models. First, case studies can trace the strategic interactions that form the basis of formal models. As shown in Schultz (2001) and Goemans (2000), case studies can trace not only which choices were considered and actions were taken, they can also show that some other actions were deliberately avoided in anticipation of the choices and actions of the other player(s). Moreover, case studies are not yoked to the assumption that any unavoidably simplified formal model represents *the* true data-generating process. Case studies can both recognize the inherent complexity of the real world and trace specific causal mechanisms. Case studies can trace and establish causal mechanisms in the midst of a potentially overwhelming number of otherwise confounding factors. Even if the empirical process does not exactly match the formal model, case studies can often still offer a judgment of the relative fit and relevance