

The Role of Iteration in Multi-Method Research

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Self-consciously “multi-method” research seems on the rise in many corners of the discipline. Recent political science dissertations, in particular, seem to draw increasingly on some combination of fieldwork, game theory, statistical analysis, qualitative historical-institutional comparisons, ethnography, and other approaches.

Why is multi-method work so attractive? One powerful reason may be that multi-method research appears to offer the possibility of triangulating on a given research problem, allowing scholars to leverage the distinctive but complementary strengths of different research methods to make progress on substantively important topics. Thus analysts strive to move between evidence on aggregate correlations and evidence on mechanisms, to combine broad general theory with fine-grained detail from case studies, to motivate a large-N analysis with a few well-chosen cases, or to marry “data set observations” to “causal process observations” drawn from focused qualitative research (Collier, Brady and Seawright 2004).

The particular ways in which different methods should or can be combined, however, has remained the subject of debate (Laitin 2002). For one, in multi-method work there always remains the possibility that we will get things wrong three ways (or two or four), instead of just one. A statistical analogy might suggest that that the likelihood of this occurring diminishes in the number of methods: if each method represents an independent approximation of the truth, the precision with which we estimate this “truth” should increase as the number of methods grows and sampling error diminishes. From this perspective, an N of three or four, where the N is the number of methods, should be at least a little better than an N of one.

This statistical analogy seems misleading, however, because applying different methods is not like drawing balls independently from an urn. In good multi-method work, various commentators suggest, the various methods are *supposed* to inform one another. Then “draws” from the methodological urn, rather than being independent, may instead exhibit strong dependence. At least in principle, adding a new method to a multi-method study could conceivably exacerbate rather than ameliorate the flaws of each of the others.

The dependence of each new methodological “draw” on prior methodological choices may be one reason that some writers encourage documenting the process by which scholars go about multi-method work – for instance, describing the order in which various methods were used or applied (Bennett and Braumoeller 2006). Yet if where one starts affects where one ends up, the Pandora’s box of multi-method approaches is also not quite a Polya urn. In a typical illustration of a “Polya urn process,” a ball is drawn at random from an urn filled with two balls of different colors; the selected ball and an

additional ball of the same color are returned to the urn; this procedure is then repeated a large number of times. As Pierson (2000: 253) and others have emphasized in analogies to path-dependent processes in politics, in such a process the initial sequence of at-random draws matters greatly for the ultimate distribution of balls in the urn. In addition, the ultimate outcome of any particular trial (i.e., any “large” sequence of draws) is ex-ante quite unpredictable, since we might end any trial with an urn filled with balls mostly of one color or the other.

This Polya urn analogy, as applied to multi-method research, seems too pessimistic. For one, iteration between various methods can provide ample opportunities for cross-method correction and revision. For another, even in the elaboration of any “single” method, the characteristic strengths of other kinds of research strategies can play an important role. In this way, the idea that analysts “apply” one method and then exploit another may not characterize all multi-method research. The central issue therefore remains exactly how different methods can inform each other, such that they can generate a “multi-dimensional conspiracy” (with apologies to Albert O. Hirschman) in favor of scholarly progress.

In this essay, I offer just a few thoughts in this vein, drawn from recent personal experience with conducting multi-method research. Several authors have recently discussed how case studies and large-N analysis may inform and complement one another (e.g., Lieberman 2005; Gerring and Seawright 2007), but there has been perhaps somewhat less sustained attention to the relationship between game-theoretic formal models and other methods.

I seek to make two simple points. First, I discuss the ways in which building an applied formal model – apparently an eminently “deductive” exercise – may in fact involve inferences and especially modes of concept formation usually more closely associated with other methodological approaches, including “qualitative” methods. Second, in discussing the relationship between models and case-study evidence, I briefly reflect on the challenges associated with what Skocpol and Somers (1980) called, in a different context, the “parallel demonstration of theory.” In both cases, my emphasis is on how formal models and other methods may inform each other in ways that are more iterative and even seamless than the image of sequential “draws” from a methodological urn would suggest.

Models, concepts, and cases

To pick an example not completely at random, and with apologies for a lack of greater imagination, I illustrate these points with a discussion of some of my own recent work on the impact of natural resource wealth on political regimes (Dunning 2007). It may be useful briefly to describe the overall orientation of this research, before exploring several issues and challenges that arose in the course of conducting it. A near-consensus has emerged among scholars now working in this area that oil and similar natural resources promote authoritarianism. Yet some of the most resource-rich (if not resource-dependent) countries in the world are liberal democracies, while a somewhat older case-study literature has suggested that oil historically promoted democracy in Venezuela – among Latin America’s most stable democracies for several decades in the second half of the twentieth century. My research was inspired both by the observation of an apparent

contradiction in the relevant literature and by familiarity with these several anomalous cases.

At the time I began this research, my disquiet about the claim that oil only hinders democracy was also motivated by my study of recent game-theoretic work on the influence of redistributive conflict on the emergence and persistence of democracy (e.g., Acemoglu and Robinson 2006). If resources really shaped the fiscal basis of states in the way the literature on “rentier states” suggests – that is, if oil and other resources displace non-resource taxation – then in such models one might expect more mixed effects of resource wealth, since resources could help ease the redistributive pressures democracy may sometimes impose on elites. The idea that resources could thus have mixed effects on the regime type matched intuitions that I had drawn from countries where I had done brief initial field visits, such as Botswana, Chile and Venezuela.

In conjunction with reading the literature on the politics of rentier states and with further fieldwork, I began to develop a game-theoretic model to help me analyze these issues. There are always many analytic choices that go into the specification of an applied formal model. In the case of my research, for instance, should resource rents appear as a term in the government budget constraint, or in a function giving the wealth or income of different societal actors (or both)? In the model’s underlying economic structure, what should be the relationship of resources to the non-resource sectors of the economy? These are just a few of the important questions that had to be answered before a model could be solved or its equilibria analyzed. The point I wish to make is that knowledge of case studies, the previous literature, and other sources of prior information

can inform answers to such questions; a bevy of “multi-method” approaches may play a crucial role in helping to motivate and inform the structure of a given formal analysis.

In my own case, the previous literature provided some helpful guidance on the analytic choices mentioned above. For one, the literature on “rentier states” suggested that resources like oil tend to provide a ready source of government revenue and also to flow into the fiscal coffers of the state (i.e., the government budget constraint) like “manna from heaven,” without substantial intermediation of numerous societal actors. Following this logic, resources should appear only in the government budget constraint of the model and not in a function giving the (pre-transfer) income of private citizens, at least as an initial matter. For another, the work of Hirschman and others had long suggested that “enclave” natural resource sectors lacked extensive “forward” and “backward” linkages to non-resource economic sectors. This idea suggested that resource and non-resource economic sectors might plausibly be modeled as independent, linked only through the channel of government spending. I found that a key to developing a useful applied model was to find means of formalizing the contrast between rents and other sources of fiscal income in ways faithful to the claims of the rentier state literature (Dunning 2007, Chapter Three).

These examples may go to suggest that the process of developing a game-theoretic model can itself be a “multi-method” process. Because analysts may draw on well-developed concepts or previous results in the field to stipulate core assumptions, developing a model may be considered a process that is both “inductive” and “deductive.” This also implies, as mentioned above, that some of the distinctive strengths

of “qualitative” methods, including especially tools for concept formation, can and often should inform the development of applied formal models.

However, this discussion raises the important issue of how to evaluate model-derived hypotheses empirically and, more generally, the relationship of models to various forms of empirical inquiry, including case studies. If cases and concepts illuminated by previous studies help to motivate models, how can those models in turn be empirically “validated?”

A common and justified complaint about the merging of formal theory and case studies in many instances is that the case studies seem chosen merely to “illustrate” the theory. It might be useful to remember that this issue is far from limited to discussions of the interaction of qualitative and quantitative methods. Indeed, the point is reminiscent of what Skocpol and Somers (1980: 179) called in another context the “parallel demonstration of theory:” a form of empirical inquiry in which “the reason for juxtaposing cases is to persuade the reader that a given, explicitly delineated hypothesis or theory can repeatedly demonstrate its fruitfulness – its ability to convincingly order the evidence – when applied to a series of relevant historical trajectories.”

Such “parallel” strategies should probably be an important part of evaluating a theoretical model, formal or not; theories have observable implications, and at least a necessary if not sufficient condition for a valid theory should be that those implications tend, in fact, to be observed where the theory says they should be. Yet such parallel demonstrations can also be unsatisfying, for precisely the reasons Skocpol and Somers suggest: cases can end up seeming simply being a way of underscoring the “plausibility”

of a theory, its ability to “order the evidence” without, however, helping to refine or push the theory forward.

Analysts might strive for a more fruitful marriage of formal and empirical, particularly case-study, research in several ways. As Skocpol and Somers (1980: 191-92) also emphasize, the parallel demonstration of theory can avoid “repetitiveness” (in which the same theory is simply applied to multiple cases) when a theory predicts different outcomes across different cases – i.e., when the cases help to elucidate what a formal theorist would call the “comparative statics” of a model. Evaluating these comparative statics through analysis of new cases that did not originally motivate the work, or new within-case evidence drawn from cases that did, can also provide an important vehicle for assessing the predictions of theoretical models empirically. Another point is that for those oriented towards formal work, case studies can not only provide evidence on the observable implications of a theory but can also help to motivate new models, an advantage of case studies that I found especially useful in my own work (Dunning 2007, Chapter Seven).

An ongoing iteration between methods thus probably better characterizes most multi-method work than does the idea of one methodological “draw” followed by another. If one finding or methodological approach does condition the next, multi-method research hardly reproduces the non-ergodicity of a Polya urn process. Instead, the strengths of different methods may inform each other at every stage of the research process, serving to balance and correct each other. It may therefore be worth reflecting further on how apparently disconnected research strategies, such as concept analysis and

game-theoretic modeling, may in fact complement each other in useful and possibly unexpected ways.

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