lic? This durable moment in time-space was the venue for the meeting of dozens of rival and distinct cultures. Some of these cultures were composed by radically different Native American peoples (the Iroquois and the various nationalities allied against the so-called "Algonquians" such as the Fox, Kickapoo, Ojibwa [Chippewa], and Miami. At least two other cultures were the peculiar settler and imperial cultures that were spawned by the French and British presence in colonial North America. In a wonderful book by Stanford historian Richard White—*The Middle Ground: Indians, Empires and Republicans in the Great Lakes Region, 1650-1815*—the limits of our existing models for explaining centuries-long realities become clear. In a land where no organized entity (no Indian nation, no Western imperial state outpost) was in complete control, and where dependence upon the harsh environment was great for all concerned, the world was governed by a politics of mutual deference and, importantly, consistent misunderstanding. These cultural realities did not merely "affect" the fur trade and the French-Algonquian alliance—they constituted it. The language of alliance was one of "artful manipulation" (*The Middle Ground*, 152), and the alliances were held together by symbols (the calumet, or the atonement ritual) that were interpreted quite differently by different facets of the alliance.

Or consider how those who design institutions learn about them. How did eighteenth-century politicians learn from the experience of state constitutions with legislative supremacy—as narrated in Gordon Wood's magisterial *Creation of the American Republic*—and adjust their beliefs to conclude that strong executive power was necessary in a mixed regime? In some respects the historical experience under state constitutions in the years 1776-1780 served as "raw data" for later founders such as John Adams, Alexander Hamilton, and James Madison. In other respects the data came from "philosophy"—a reconsideration of the celebrated arguments in favor of executive power in a mixed regime, from Baron de Montesquieu's *Spirit of the Laws*. Yet these "data" were never available numerically to actors of the time and were never "learned" using rules of conditional probability. The evidence base consisted instead of rare events, sometimes single events, that were "observable" only symbolically, and even then were subject to various interpretations. Wood's triumph is to draw out a pattern of learning through observation, deliberation, and political conflict, a pattern constructed from a wealth of primary source materials such as pamphlets, editorials, essays, petitions, newspapers, and broadsides.

My sum point is that primary-source-driven narrative and mathematical modeling can complement one another in ways that render the status quo—modeling combined only with statistical estimation alone—a highly impoverished research agenda.[5] In order to realize the possibilities for weaving model and narrative together, practitioners of each single art will need to recognize the limits of their own approaches and how they can be complemented by more imaginative research practices.

Notes

[1] Key for me is that mathematical modeling need not be modeling that is committed in any way to the rationalist paradigm. Some of the most fascinating mathematical models are those that explore "bounded rationality," network dynamics, stochastic processes, or something else. The paucity of such non-rational formal models in political science is a material weakness of our discipline.

[2] Alternatively, their vote against the candidate or his party may comprise some sort of "trigger strategy" or generalized form of punishment for the politician's deviation from cooperative or truthful behavior. For an empirical analysis of one such scandal which shows that retrospective voting is far more complicated than our current models would suggest, see Michael A. Dimock and Gary C. Jacobson, "Checks and Choices: The House Bank Scandal's Impact on Voters in 1992," *The Journal of Politics* 57:4 (November 1995), 1143-59.

[3] For discussion of graph-theoretic approaches to causality, see Judea Pearl, "Causal Diagrams for Empirical Research," *Biometrika* 82 (1995), 669-710 (with discussion); Pearl, *Causality: Models, Reasoning and Inference* (New York: Cambridge University Press, 2000); Steffen L. Lauritzen, *Graphical Models* (Oxford: Clarendon Press, 1996).

[4] For a wonderful account of how the apparatus of experimentation can place scientists at further and further remove from the material of their inquiry, see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago: University of Chicago Press, 1997).

[5] The most direct attempt to meld narrative and rational-choice modeling was the *Analytic Narratives* volume by Bates, Greif, Levi, Rosenthal, and Weingast. The unfortunate feature of that effort was its absence of narrative–too many of the narratives were in fact data analyses, and it was difficult to separate what the authors called "analytic narrative" from "theory generates comparative statics when the meet data" exercises with which we are very familiar in modern political science.

Peril and Promise: Multi-Method Research in Practice

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There's no doubt about it: multi-method research is in vogue.[1] Perhaps the most obvious evidence of this comes from the job market. Job candidates who successfully combine multiple approaches get that ineffable "buzz" and are often showered with adulation and, ultimately, job offers. As one faculty friend opined with regard to one particularly exceptional candidate, "she is clearly a new kind of comparativist." Another remarked that the work was so good, "the talk could have been delivered in Greek." Graduate students have taken these signals to heart. Increasing numbers are attempting to master qualitative, quantitative, and formal approaches and to formulate methodologically eclectic research proposals. Yet it bears noting that the road to the Promised Land isn't so easy to navigate. Creatively and effectively combining multiple methods is time-consuming and difficult, as the article in this newsletter by current and recent PhD students doing multi-method research attests (Siegal et al., 2007). It can also be risky. In my experience, search committees prefer candidates with mastery of one method to those with mere facility in multiple methods. Poorly executed research may end up pleasing no one.

This essay is an attempt to demystify the practice of multi-method research by illustrating how I executed the project that resulted in my dissertation and ultimately my book, *Crucibles of Political Loyalty: Church Institutions and Electoral Continuity in Hungary* (Cambridge University Press, 2006). The target audience is graduate students and others who want to employ multiple methods but find the prospect daunting and frustrating. As will become clear further below, there is no magic bullet. The practice of multi-method research involves options not taken, difficult tradeoffs, and a willingness to make mid-course improvements as circumstances demand. The bulk of the essay will elaborate the reasons for my choices and how my dissertation and book have been received. I conclude with some advice for graduate students on the peril and promise of multi-method research.

The Dissertation I Might Have Written But Didn't

The idea for my dissertation came from observing a peculiar feature of post-communist Hungarian politics: the emergence of political parties with the same names and slogans as pre-communist parties. Why should old symbols and labels reappear and gain electoral traction after four decades of communist rule? After more research I realized that similar partisan continuities appeared elsewhere in Eastern Europe and, in a different form, in newly democratized countries of Southern Europe and Latin America. This discovery opened up the possibility of a large-N analysis of all countries where democracy was interrupted by some period of authoritarian rule. I might have collected data on the duration and nature of authoritarian rule, party systems, opposition behavior, and sundry other potential predictors of political continuity with the pre-authoritarian past. Part of my thesis would have consisted of cross-national statistical analyses. I would then have elucidated the detailed workings of the argument through carefully selected case studies.

I do not recall ever having seriously considered this possibility (though it still strikes me as an excellent topic—you heard it here first!). Comparative politics was regionally subdivided, and my tacit assumption was that cross-regional comparisons, while technically possible, were of limited analytic utility. Latin American and Southern European authoritarianisms seemed too different from East European communism to permit meaningful comparison. Moreover, I had been trained to believe that one could not understand a country's politics except through mastering its language and immersing oneself in the society, typically through at least a year of fieldwork. Consequently, although I was no stranger to statistical analysis, the idea of serious research in more than one country seemed impractical and ultimately of uncertain value. I might have been swayed otherwise if someone had argued that my job prospects depended on it, but post-communist candidates seemed to be getting jobs, the market was distant, and one country seemed quite enough. I knew early on that this choice might pose problems of generalizability, but I came to realize that the only way to address the larger puzzle of long-term political continuity would be to explain how it played out in one particularly difficult case. The bulk of my empirical research would be limited to Hungary.

The Dissertation I Did Write

I began the project without any explicit intention of employing multiple methods. If I had to characterize how my thesis (and later book) came to have its particular blend of quantitative and qualitative analysis, I would have to say that I did what seemed most useful for answering the question. This is not meant to be glib. I was no less interested than contemporary students in doing good work, but the focus was on the research question rather than methodological eclecticism per se. That I nonetheless ended up employing an array of tools is evidence that then-existing folk wisdom and common sense on how to do good social science often entailed the use of mixed methods. This is not to imply that for every question there is an obvious research design. It is all too easy to err. Rather, it is an acknowledgement that many researchers were using multiple methods, at least in some form, long before they achieved their current exalted status.

Why should old patterns of mass political loyalties re-emerge after prolonged economic, social, and political disruption? My research strategy tried to gain as much leverage on this question as possible within the constraint of focusing on a single country. I pursued a three-pronged approach, each part of which was designed to address a different anticipated objection. The first and scariest (at the time) was the charge that Hungary was not an interesting place to explore the question. The "why did you study <country-name>" question is among the most common one encounters, and woe unto whomever cannot provide a satisfactory answer. My response was to situate Hungary as a "least likely" case to exhibit political continuity. Prior theory tended to focus on the less disruptive authoritarianisms of Latin America or Southern Europe, where the covert activities of parties, trade unions, and other organizations opposed to the dictatorship were invoked to account for partisan persistence. Under communism civil society was far more comprehensively destroyed or co-opted, and could not perform the same function. Thus, whatever was producing continuity in Hungary had to be different from what was causing similar outcomes elsewhere. The advantage of studying Hungary, then, lay in the potential for exposing a new transmission mechanism.

The second and related problem to avoid was what King, Keohane, and Verba (1994: 208) refer to as the "n=1 problem." One national-level observation of continuity yields precious little inferential leverage. To counter this I disaggregated the dependent variable. There had been studies of regional electoral continuity in Hungary, but changes in internal borders rendered the results suspect. A lower level of aggregation was required. I spent a good chunk of my first extended period of fieldwork attempting to gather such data. Ultimately I succeeded in collecting and matching pre- and post-communist municipality-level electoral data for the entire country. One "case" yielded nearly three thousand observations. Through basic quantitative analysis I established many patterns of political continuity and discontinuity.

Finally, the price of eschewing cross-national breadth had to be paid in explanatory depth. The comparative statics were novel and fascinating, but for social science the real value-added of the project was in illuminating how pre-communist partisan attachments were successfully transmitted into the post-communist period. This required returning to Hungary in the hope that I could find materials that would shed light on the differences between localities where there was partisan persistence and those where there wasn't. Constraints on archival research precluded the possibility of hand-picking a sample of settlements that might best illustrate the process. There was no guarantee of access or that useful materials even existed. The general dearth of information made this qualitative piece by far the most challenging part of the project. I spent most of my second extended period of field research exploring provincial archives, where I discovered that the survival of right-wing attachments was rooted in the successful efforts of Catholic parish priests to preserve local church institutions against communist encroachment. We are conditioned to think of archival materials as inherently qualitative. In this case, however, they yielded invaluable local-level data on mass loyalty to the churches. I was thus able to demonstrate clerical influence both quantitatively, for a smaller sample of settlements, and qualitatively through interpretive analysis of Communist Party and church reports.

From Dissertation to Book

I received many suggestions for improvement as I endured the job market and prepared the book. One of the more common was that I should add another post-authoritarian case. The best reason to do this came from my publisher, who wryly informed me that books on Hungary were not best-sellers, and that without including other countries I had no hope of getting a paperback. Tempting as it was, I could see no theoretically compelling reason for the considerable extra effort. Although there was certainly a payoff to knowing that the basic argument held up in a different political context, in the end the primary unit of analysis was locality, not country. Including settlements from a different polity would not add variance that did not already exist within Hungary. Another frequent suggestion was that I include an in-depth narration of how the struggle between parish priests and local party cadres played out in a single village. I seriously considered this because it would have improved the argument's rhetorical force, but intellectually it had even less to recommend it than going cross-national. Such a narrative would not have revealed information that was not already available in more encapsulated form elsewhere.

The most potentially damaging criticisms suggested that I got Hungary all wrong. Some with country knowledge claimed that my findings merely reflected the fact that the region I had focused on had always been among the most conservative and Catholic in the country, and was thus not representative of Hungary as a whole. Quantitative people questioned some model specifications and my reliance on ecological data. I took these criticisms very seriously because I felt like if I didn't have Hungary nailed down I was doomed. Consequently, I devoted significant effort to increasing confidence in my findings. On the qualitative side, I replicated the archival research in a predominantly Protestant region. This showed that my initial results were not a fluke and provided leverage on confessional effects that I could not explore with materials that focused only on Catholic activity. On the quantitative side, I established that the ecological results were robust to many different specifications and corroborated any ecological inferences with comparable survey data. In the end I got the book contract, but not the paperback edition!

Advice for Graduate Students

My experience may not be wholly representative, but I do think it offers a few lessons for those contemplating or already engaged in mixed-method research.

1. Choose a question, then a method. It sounds obvious, but the availability of automated tools allows us to generate output even in the absence of a research question. Resist the temptation to crunch numbers before nailing down the purpose of the analysis.

2. If your research is primarily on one country, make sure you get that country right and are prepared to defend your choice. Cross-national researchers are not expected to have equal mastery over their cases, even those they investigate more thoroughly as part of a nested design.

3. If your research is primarily on one country, make sure you have sufficient within-country variation across subunits, over time periods, or across functional issue areas. Make really sure others know that the unit of analysis is not simply the country. Correct those who dismiss your work as a "case study."

4. Be prepared to get hit from all methodological sides. Good departments will expect you to use all your methods equally well.

5. If you work on developing countries, do not assume that others appreciate the difficulties of data collection. People who google their data may require special enlightenment.

6. If you work on developing countries, do not expect forgiveness for lacking the kind of data that are available to those who address similar questions in developed countries. People who have done fieldwork will sympathize with your plight, but others may penalize you for asking a question that could not be fully answered.

7. If you work on developing countries, do not expect much extra credit for overcoming obstacles to data collection. Those who have done fieldwork will laud your ingenuity, but in the end good departments are more interested in what you have done with the data than in the data themselves.

I conclude by re-emphasizing the importance of starting with a good question. If your topic is truly compelling, you may be forgiven some minor sins, but no amount of methodological razzle-dazzle can compensate for a poorly posed problem.

Notes

[1] I am grateful to Andrew Bennett, David Collier, and Nick Ziegler for numerous helpful suggestions.

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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 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 and the 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, in the iteration between various methods there are often 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 the ways in which case studies and large-N analysis can 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.