2004, we ran two symposia, the first on techniques of field research and the second on content and discourse analysis. In Fall 2004, we tackled Charles Ragin's complex and innovative technique of Qualitative Comparative Analysis (QCA), and its recent variants. In Spring of last year we ran symposia on the qualitative/quantitative distinction (with contributions from both sides of the divide), and on the use of necessary-condition causal propositions. This past Fall we featured a discussion of where new hypotheses originate, by Richard Snyder, along with two symposia, one focused on Ian Shapiro's *The Flight from Reality in the Human Sciences*, and the second devoted to the subject of concept formation in the social sciences.

Every year we take notice of recent methodological publications that may be of interest to our readers. (For the reasons mentioned above, we don't cover work that is narrowly tailored to statistical analysis.) The *Book Notes* and *Article Notes* features are intended to list work that either has an explicit methodological focus or uses an innovative technique to good effect. If you know of a book or article published since 2000 that has not already appeared in these pages—and has a strong methodological theme or innovation—do let us know. (Self-nominations are encouraged!)

In this issue, we are fortunate to be able to feature two roundtables focused on the work of scholars who have had enormous influence on the discipline. The first examines the career of David Laitin, whose work incorporates ethnography and rational choice—methods often deemed to be antithetical—and the second solicits comments on the recent landmark publication by Alexander L. George and Andrew Bennett, *Case Studies and Theory Development* (MIT Press, 2005).

The following issue (Fall 2006) will begin the tenure of a new editor, whom I am delighted to introduce. Gary Goertz has written widely on international relations and on methodological issues and teaches regularly at IQRM, the winter graduate training institute at Arizona State University. Having engaged with both quantitative and qualitative methodological issues, he is well positioned to foster a productive debate among scholars who utilize diverse approaches to the study of politics. I know that Gary is looking forward to engaging with the QualMeth community and wishes to hear your ideas on how to maintain the newsletter as a vital part of our research community. Please join me in welcoming Gary, and please accept my thanks for your participation in the newsletter's ongoing activities. Finally, let me take this opportunity to thank Joshua Yesnowitz, who has served as our assistant editor for the past several years and will continue under Gary's tenure. Josh has done a superb job of keeping track of the details and putting everything together. We are grateful for his stewardship.

Symposium: Ethnography Meets Rational Choice: David Laitin, For Example

Introduction

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One of the more encouraging developments in political science over the last few years has been the appearance of work that is self-consciously multi-methodological. An increasing number of dissertations and publications combine formal models with statistical analysis of large-n data sets and comparative case studies.

Less evident are efforts to combine ethnography, or the recovery of the intersubjective world of actors themselves, with more mainstream traditional or formal methods. David Laitin is one of the rare scholars who has engaged in serious ethnography (*Hegemony and Culture*), combined ethnography with other methods (*Identity in Formation*), and applied rational choice techniques, with James Fearon, to issues of identity. ("Explaining Interethnic Cooperation") His work provides the opportunity for this symposium.

Each of the authors has critically engaged Laitin's work, with an eye toward assessing the merits and possibilities of combining serious ethnographic scholarship with rational choice. While conclusions are best left to readers themselves, it is fair to say that the authors share concerns with how ethnographic sensitivity to contextual realities can be squared with the a priori simplifications necessitated by rational choice approaches. But, importantly, each of the authors also believes it is a combination well worth attempting.

Each of the papers in this symposium was originally presented as a Qualitative Methods Roundtable at the September 2005 American Political Science Association meetings in Washington. David Laitin's responses to these papers concludes this symposium, but begins a long, continuing conversation with his many critical admirers.

Theory, Data, and Formulation: The Unusual Case of David Laitin

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In two influential articles David Laitin laid out a tripartite method for comparative politics and for social science more generally (Laitin 2002, 2003). The three methods that Laitin advocated were Formal Theory, Quantitative Analysis, and Narrative. In this paper I take issue with Laitin's categorization scheme for the methods, and I consider the criteria and constraints on choosing methods.

Types of Methods

Laitin's tripartite framework is an intriguing methodological model for the social sciences. While I agree with many aspects of the argument, in contrast to Laitin's tripartite framework I believe there are actually two major distinctions separating the vast array of methodologies available to social scientists: theory vs. data, and formalization vs. non-formalization. Moreover I think that these distinctions might usefully form the basis of a two-by-two categorization scheme yielding four types of methods: Theory, Formal Theory, Narrative, and Quantitative Analysis (see Table 1).

Table 1

	Analysis of Ideas	**Analysis of Data**
Non-formal	Theory (T)	Narrative (N)
Formal	Formal Theory (FT)	Quantitative Analysis (QA)

The primary difference in my formulation from Laitin's framework is the distinction between methodologies that focus on the analysis of data and those that focus on the analysis of ideas or theories, and the division of theory into formal and non-formal types.

The four methodological *labels*—Theory, Formal Theory, Narrative and Quantitative Analysis—stand in of course for a wide array of actual methods. Non-formal theory encompasses any abstract thought, philosophy, or set of rules, principles, beliefs, or ideas which has not been formalized into mathematical language. That which has been formalized into mathematical concepts, including social choice, game-theory, differential equation modeling, etc., can be called formal theory. Formal theory and theory are fundamentally about the analysis of ideas rather than data.

In contrast, narrative and quantitative analysis share an analytic focus on data, rather than ideas. These data can be derived from a variety of sources (interviews, texts, surveys, etc.), but what separates narrative from quantitative analysis is the formalization of data into quantified or numerical entities subject to statistical methods. Narrative can mean ethnography, discourse analysis, case studies, or any analysis of data that has not been formalized through quantification. Similarly, quantitative analysis is the examination of quantified data using a variety of statistical methods from simple significance tests in cross-tabulations to a wide range of regression models including OLS, probit, and Bayesians statistics.

Laitin did emphasize the distinction between formal theory and quantitative analysis, but I think his tripartite framework did not go far enough in differentiating the two. We must push understanding of the differences further. In practice there appears to be an affinity between quantitative analysis and formal theory, but it's crucial to properly understand what the two methods share, as well as what distinguishes them. Formal theory and quantitative analysis primarily share one thing: mathematical language. That is, they are both formal in the sense of relying on mathematics to work out complicated relationships among variables, and they accept the constraints imposed by that language. This shared language accounts for why it might sometimes seem that quantitative analysis scholars and formal modelers are better able to talk to each other than those who don't share their language, i.e. those working in non-formal theory or methods. But, a shared language, or shared formalization, does not bridge the ocean of difference between the two methods in terms of the object of analysis, namely theory vs. data. Quantitative analysis is fundamentally about data. It uses the science of statistics to manipulate empirical evidence. Often the goal is to empirically test existing theories, either formal or non-formal. But without data, there is no quantitative analysis.

Quantitative types hunger for large data sets, and without data sets, the operation comes to a halt. Indeed, there are those so committed to QA that if the datasets do not exist for a given problem, they do not study it; and there are some who will resort to using any data, no matter how poor, as long as they exist. The key point is that QA shares with narrative—or history, or ethnography, or discourse analysis—an analytic focus and dependence on empirical data.

These data-focused methodologies are very different from formal modeling or theory. Formal theory is fundamentally a theoretical exercise. The common substitution of *theory* for *modeling* in the title is not a coincidence. Formal theorists do not set out first and foremost to solve empirical puzzles; rather, they make their living formalizing ideas that have not yet been formalized. In confronting a topic, the formal theorist asks, has this been modeled? Or, in other words, has the logic or set of ideas been put into mathematical language? To put it more starkly, formal theory can operate without datasets, and in a world without datasets, the formal theory enterprise would hardly suffer. Ideas, not data, are the foundation of theory, both formal and non-formal. Thus, formal theory shares much with non-formal theory, and it is the analysis of ideas that links these two methods and separates them from both quantitative analysis and narrative.

How Many and Which Methods to Use?

What has turned out to be most provocative about Laitin's tripartite method was not the list of methods themselves or the categorization scheme which included the three methods, but the idea which many people have taken from the discussion of the tripartite method, namely that all social scientists should use all three methods together in all of their research projects.

At the 2005 APSA panel, Laitin argued that this was a misinterpretation of the tripartite method, and that his view was that the three methods should be used collectively by social scientists in a way such that for any given research problem all three methods get employed, and practitioners of each method appreciate in their own work the contributions of the other methods. Indeed, in the 2002 chapter, Laitin writes, "my argument is not that all comparativists should have highly cultivated statistical, formal, and narrative skills" (Laitin 2002,

659) and he goes on to argue that no one method should dominate the discipline. However, if one reads the 2002 article carefully, we see that Laitin does not actually say how many methods one person should use; he suggests three is too many for one person and he argues for collective diversity, but he then leaves it at that.

Rather than clarifying, in the 2003 article, Laitin starts out remarkably vague on the issue of just how many methods one researcher *ought* to use.[1] In critiquing two scholars, Bent Flyvbjerg and Stanley Tambiah, whose primary contributions are to the use of narrative methods, he writes, "the work would have much greater scientific value if placed within what I have dubbed the *tripartite method of comparative research*– a method that integrates narrative..., statistics, and formal modeling" (Laitin 2003, 164-5, emphasis in original). But what does it mean to say the narrative work should be "placed within" the tripartite method? Does it mean that Flyvbjerg and Tambiah should have added quantitative and formal modeling to their analyses? Or does it mean some other scholars should have come along and studied the same problems as Flyvbjerg and Tambiah using other methods? There is an enormous difference between saying social science research problems should be studied from a number of methodological angles by different people and saying individual researchers should use all of the methods.

Farther on in the article, Laitin suggests that he means the latter, namely that individuals should use all three methods. Laitin calls Randall Stone's work, which uses all three methods, "an exemplary model of the tripartite method" (Laitin 2003, 177). It is interesting that Laitin did not choose a collective enterprise of several scholars working on the same problem using different methods as the "exemplary model" of the tripartite method, but rather he chose an individual who had used all three methods. This choice suggests that the view that Laitin is advocating the use of all three methods by individuals is not outside the range of reasonable interpretation.

Whether or not Laitin was actually arguing for the use of multi-methods by individuals or merely advocating collective diversity (or some of each), his writings in any case still highlight the question of how many methods individual researchers should be expected to use in one project. I argue that the four methods in the framework proposed above, or even the three methods proposed by Laitin, are not necessary or desirable in every social science project. In my view, the appropriateness or choice of methods depends on four factors:

(1) the nature of the problem under investigation, and the contributions that a particular method might make to such a study;
(2) the resources available to a scholar;
(3) the disciplinary context, including norms, incentives and constraints, in which a scholar works; and
(4) the aptitude and will of a scholar to do multi-method work.

No one method is a panacea for all social science problems. In particular, it is a mistake to assume that formal theory is the only type of theory useful to social science. Many formal theorists would agree that formalization of a theoretical argument is not always required or advantageous. Sometimes a problem is so simple as to make formalization redundant. Sometimes it's been done before, and therefore another formalization would yield no new insights. And most importantly, there are classes of problems for which formalization is not appropriate (yet, or perhaps ever): these include issues such as irrational beliefs and behavior, non-transitive preferences, and interactions in which new unknown and unknowable possibilities for action exist. Formal theory is not able to solve these sorts of problems at the moment, and may never be able to. While formal theorists are currently working on expanding the range of formal models, it is very unlikely that in the near future all theoretical issues will be subject to formal analysis. Thus, sometimes a case can be made for formalization of theory, and sometimes not.

In addition to the specificity of the problem at hand, resources are another factor for scholars in choosing the appropriate set of methods. Methods are costly to learn and to do in practice, and therefore the more methods one chooses, the more costly it is for each researcher. Sometimes there are public goods such as data sets or publicly available empirical material, or existing theories or formal models which can be built upon, that lower the cost for researchers using a particular method, but this is not always the case, and thus, often, choosing methods requires trade-offs, which in practical terms might mean leaving out a method, despite it's potential benefit. Hence the following types of contributions by individual scholars: theoretical work or formal models which have no empirical component, or work which uses only narrative or quantitative analysis rather than both. That these are the contributions of individuals does not mean that others cannot add to the collective contribution by bringing additional methods to the study of the same problem, but resource constraints do and probably will continue to limit the number of methods that any individual can employ.

Discipline or sub-disciplinary norms also play a role in which methods researchers choose, and there is variation across fields. For example, if we survey the social sciences as a whole, it is economics and political science, rather than anthropology, for example, where formal theory has been most successful. And in political science, it is the subfields of IR and American politics where formal modeling has advanced furthest. The reasons for this differential success are related to issues of problem-appropriateness and resources discussed above, but also, I believe, to the expectations and receptivity of different subfields to theoretical versus empirical work. Fields where theory is privileged and case-specific data requirements are the least rigorous are most receptive to theoretical enterprises such as formal modeling. In political science, comparative politics arguably has higher case-specific data requirements than IR. Comparativists, like anthropologists for example, are expected to know a lot about a given place, and to have experience in collecting data in that place. The comparativist who does not speak the language of his or her region of expertise is unlikely to get a job. Not so in IR. The IR area-specialist who works with regional documents in original languages is a rare species. Most IR scholars are generalists, working on a topic in many places, without deep, language-dependent knowledge of particular places. This is no criticism, it's just an observation, which I think suggests that IR as a field is less demanding of detailed case-specific data than comparative politics. This may partially explain why formal theory has made more contributions in IR than comparative politics. It is not to say that formal theory is incompatible with case-specific data or fields that rely on primary language or case-specific data, but just that as a costly method which occasionally leaves out empirical work, it is not so surprising to find it has had a more positive reception in IR than in comparative politics.

Resource constraints or availability of data and theory also interact with disciplinary norms. In fields where empirical data or datasets are readily available, and therefore the cost of providing empirical tests are lower, there tends to be a higher threshold for expectations of empirical work. For example, in American politics, the sheer number of people working in the subfield and the accumulated body of both theory and empirical evidence in narrative as well as quantitative form mean that it is much less costly for a scholar to include multiple methods in a project than it would be for someone studying a place with much less existing work, theoretical and empirical, such as Sri Lanka. Thus, is it not so surprising that on average Americanists probably employ, individually, the greatest number of methods on any given project.

Finally, disciplinary norms also effect the type of the problems most likely to be studied in particular subfields, and therefore also have an effect on the appropriateness of particular methods. Fields that focus on individual actors or unitary actors are most receptive to methodological individualism and game-theoretic models of strategic interaction common in formal modeling. While these types of problems are common in many subfields of political science, they may be most common in IR, where, not-coincidentally, formal modeling has become so widely established.

To use all four methods–theory, formal theory, narrative, and quantitative analysis–is a nice proposition to consider as an ideal type, but as the examples above demonstrate, in practice it is rarely feasible owing to the nature of the problems under investigation, resources, and disciplinary contexts. Similarly, even in the case of Laitin's tripartite method, the use of all three methods is very uncommon in practice. A telling illustration of this point is that in his discussion in support of his tripartite method, Laitin could not find three good examples in the political science literature (though perhaps he restricted the pool to IR and comparative politics). Recall that he cited Robert Bates' coffee study, which used formal theory and narrative (Bates 1998); then Adam Przeworski et al.'s work on democracy, which used quantitative analysis and narrative (Przeworski et al. 2000); and finally Randall Stone's work, which did use all three methods (Stone 2002), but which Laitin criticized for the inadequate contribution of the narrative component, claiming it was overshadowed by the quantitative analysis and the formal model. Ironically, had Laitin cited his own work, he would have had examples of the tripartite method, but to do so would have highlighted how rare such choices are outside of American politics. This point brings up the final factor in choosing methods, which is the will of individual researchers.

David Laitin's methodological breadth is not common. Over the course of his career he has not just learned new methods, but also new languages and new places to test his theories. This constant retooling is both costly and rare. After a certain point–often middle age–most scholars are not interested in moving out of their established comfort zone. Area specialists stick with their area; and quantitative people stick with quantitative analysis, for example. Sometimes people innovate on the margins: scholars working on narrative case studies expand their data analysis techniques and move into quantitative analysis, or quantitative scholars add case studies or archival material, or formal modelers expand their mathematical knowledge to move into quantitative analysis.

But David Laitin has gone far beyond these types of marginal innovations. He began his lifelong work on ethnic and language politics using narrative methods on African cases. He followed ethnic politics to Europe and Catalonia, learning new languages and places. But the end of the USSR seemed to provide a major experimental testing ground for language and identity politics so he again learned new languages, Russian and Estonian, and also new quantitative methods (content analysis and experiments), and began his study of formal theory. His collaborations with James Fearon and the Minorities at Risk dataset allowed him to do worldwide analysis of ethnic conflict, fully using formal theory, narrative, and quantitative analysis. This commitment to really learning new places (including languages) and new methods is extraordinary–and exceptional. The decision to take the time and effort to learn several languages and more than two methods is not something most scholars seem to want to do. Whether we agree or disagree with the merits of multi-method work, we have to acknowledge that beyond non-formal theory, one or two methods at most appears to be the norm in comparative politics and IR. As I have argued, resources, disciplinary norms, and the nature of the problems facing researchers all impact the methodological choices researchers make, but as the case of David Laitin shows, the will of the researcher may also be a factor. Even amongst social scientists, there's no accounting for taste.

Conclusions

Methodological choices cannot be dictated from without. Researchers must be free and encouraged to make choices appropriate to the problems at hand given the resources they can acquire. The above discussion suggests that while we should not expect to see all four methods being used by individuals in large numbers of scholarly works anytime soon, the barriers to collective diversity are not particularly high either and therefore we may well see all four methods more evenly represented in research problems, depending of course on the problems themselves, resources, disciplinary norms, and the preferences of individual scholars.

In addition Table 1, and the two distinctions of theory vs.

data, and formalization vs. non-formalization may suggest a way to bridge the idealism of the individual-based tripartite method with that of the more common one-or two-method scholarship. If researchers were to choose a method from each column and from each row, it would force most people out of their comfort zone. To fulfill this requirement, researchers would have to include one formal component (either formal theory or quantitative analysis) and one non-formal component (either theory or narrative); similarly they would have to include one data component (either narrative or quantitative analysis) and one theoretical component (either formal or non-formal theory). This type of selection rule in choosing methodologies would introduce much greater flexibility than Latin's tripartite suggestion, by allowing for fewer methods in any one project and including non-formal theory as a choice. But, it would follow the spirit of Laitin's framework and his own work, by encouraging all researchers to bridge the mathematical and empirical divides. I hasten to add, however, that even this two-by-two framework and methodological selection scheme has to be seen as an ideal type predicated on the assumption of adequate resources, including data, theory, and skills of researchers. The most difficult and important problems that political science faces–e.g. democracy, development, and representation in inhospitable circumstances–may be areas where several types of resources are lacking, and therefore at the end of the day researchers have to make methodological choices given the demands and constraints of problems of interest.

Notes

[1] To be fair, his primary goal in the 2003 article is set up the tripartite method as a framework for social science and to place narrative within framework on equal footing with quantitative and formal modeling, and in the article Laitin spends a great deal of time discussing the value of narrative methods. So, it is possible that the vagueness is the unintentional result of a focus on other issues.

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Mechanisms vs. Outcomes

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In his book *Hegemony and Culture*, David Laitin described himself as being committed to "a comparative politics that is sensitive to the particularities of each society, yet asks broad and general questions about all societies" (1986: xii). This idea of comparative politics–that it is in part a discipline that engages in the study of individual countries mainly for the purpose of producing cross-country generalizations–is the way in which most of us define the field now. And Laitin's work, which includes a study of the particularities of Somalia, Nigeria, India, Spain, Estonia, Latvia, Ukraine, and Kazakhstan in order to produce knowledge about other countries and continents, is unprecedented in comparative politics in its ambition and accomplishments in combining depth and breadth.

But what kind of breadth should we expect depth to generate? What kinds of generalizations based on within-country studies should we value in comparative politics?

In principle, we value generalizations about *outcomes*. So, when Lijphart finds that consociationalism preserves democratic stability in the Netherlands (Lijphart 1975, 1977), we want to know if consociationalism is also associated with democratic stability in other countries–South Africa or the former Yugoslavia. When Putnam finds that social capital explains institutional performance in Italy (Putnam 1993), we want to know if it also explains the same outcome elsewhere–Russia, or the U.S.. And when Laitin finds that the hegemony introduced by colonial rule explains the non-politicization of religion in Yorubaland (Laitin 1986), we want to know whether colonial hegemony explain the non-politicization of cleavages in other places–Zambia or India. Indeed, the ability to generate correct predictions about outcomes in out-of-sample countries is often treated as a test for the validity of a theory developed from a within-country study.

Against this backdrop, I make four arguments in this essay, illustrated with reference to Laitin's work:

(1) Although I share the view that the value of within-country studies in comparative politics lies in generating knowledge about other countries, I think that we are wrong in trying to distill generalizations about *outcomes* from within–country studies. The generalizations we should look for are generalizations about the *mechanisms* linking the independent and the dependent variable.

(2) We should evaluate the quality of such generalizations, not by testing to see if the entire chain of mechanisms linking the cause and the outcome in one country is the *same* in others, but by seeing how *far* the chain of mechanisms in a new country coincides with that of the first before it diverges.

(3) Arguing about whether we should use ethnography or rational choice or both in our work is beside the point. "Ethnography" and "rational choice" are not strictly comparable–the one is an approach to how data are collected, the other an