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Contents

Letter from the Editor

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In the Fall of 2002, APSA created its 37th Organized Section, devoted to the study, development, and dissemination of qualitative methods. Since that time, I have served as the editor of this newsletter. My job, as I saw it, was to bring to the attention of our members the most interesting, innovative, and (it follows) contentious issues in the field of political methodology, regardless of whether they might be categorized conventionally as 'qualitative' or 'quantitative.' (Issues of import solely to quantitative work have been deferred to the Political Methodology section–no need to duplicate effort.) With that caveat, the mission of the newsletter was interpreted broadly to include all methodological issues of relevance to the study of politics. Symposia have ranged from broad philosophy-of-science issues to narrower debates about technique. For the most part, these topics have been chosen in response to ideas from our members and as extensions of APSA panels and roundtables. Usually, the management of a symposium was delegated to the person taking the initiative to organize a discussion on that topic.

As editor I took a laissez-faire approach to the newsletter, asking authors to follow only a few stylistic and substantive guidelines: contributions should be short, accessible to a broad readership, written with some flair, and encompassing a range of viewpoints on the chosen subject. The aim, while retaining some of the intellectual rigor associated with more traditional academic journals, was to give writers scope to opine–that is, to use the first-person pronoun and to adopt a more discursive manner than would be usual in a more formal academic venue. In this manner, I hoped to reproduce the lively and candid views exchanged with each other in emails and over cups of joe. "What do you really (in your heart of hearts) think of X?" This is the sort of conversation that I wanted to foster.

During the last three years, the newsletter has covered a lot of ground. In Spring 2003, we ran a symposium on teaching qualitative methods, which featured a comprehensive review of textbooks and discussions of courses and various approaches to the subject. In Fall 2003, our symposium addressed the knotty issue of "interpretivism," with contributions from several scholars, including Clifford Geertz. In Spring

Concluding Remarks

In this essay, I merely wanted to describe Laitin's work in terms of ethnography and rational choice, not evaluate it. The differences are clear across the three pieces. It is also obvious that each piece produces different kinds of knowledge, especially with respect to the expected generalizability of the findings. In *HC*, a rationalist theory of identity has been married to a particular set of empirical circumstances. In *IF*, a more elaborate rationalist theory of identity has been tested in a much larger, though still bounded, domain. In EIC, a universalist theory of identity has been tested nowhere, but has been demonstrated valid within a set of ultra-constraining assumptions.

Is the obvious true? Is ethnography the enemy of generalization? Perhaps in practice, but not in principle. Wedeen (2002) has recently written about the possibility of collecting intersubjective data based on phenomena such as identity, so that conceptualization of variables need not be derived exclusively from a priori theories, but rather can remain true to the ways in which concepts are understood in context. This would provide more reliable and valid data for those with statistical inclinations, for those who wish to specify survey and focus group instruments, and for those who wish to construct models with grounding in some reality.

The objective should be to return to Laitin's original insight. Theories of political action, of identity, of mobilization and identification require accounts of preferences that are not merely assigned, but theorized and empirically uncovered. And preferences themselves are not just the oral statements or written testimonies of subjects, but are embodied in their mundane daily practices. Ethnography, in this sense, is necessary for rational choice to produce creditable knowledge claims of any kind.

Notes

[1] Reminiscent of the efforts made to do so in Robert H. Bates, Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, and Barry R. Weingast, *Analytic Narratives* (Princeton: Princeton University Press 1998).

[2] This is reminiscent of Achen and Snidal's recommendation that qualitative case studies are best suited as the raw material appendages of rational choice models. Christopher H. Achen and Duncan Snidal, "Rational Deterrence Theory and Comparative Case Studies," *World Politics* 41:2 (January 1989), 167-69.

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Recognizing the Tradeoffs We Make

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David Laitin defies a famous binary classification of scholars between hedgehogs and foxes. The late Isaiah Berlin's work, following Tolstoy's, gave this distinction considerable currency in the social sciences. The hedgehog knows one thing very well; and the fox knows quite a few things, if not each in great detail.[2] Hedgehogs work on one given topic/theme/theory for an entire lifetime, adopting a cumulative research program, attempting to resolve one puzzle at a time, as they advance. Think of Arend Lijphart's lifelong pursuit of the idea of consociational democracy.

Foxes move from one big topic/theme/theory to another, each topic keeping them engaged for a few years but not more, showing enormous intellectual breadth in the process. Consider Samuel Huntington in political science, and Amartya Sen in political economy. Huntington has provoked new debates in three fields of our profession: comparative politics, American politics, and international politics. Sen ranges from rationality on one hand to famines and poverty, inequality, choice of techniques in planning, and, increasingly, identity on the other.

Laitin has worked almost entirely on ethnic politics, rarely if ever on development, economic reforms, democracy and authoritarianism, party politics, etc., let alone in other subfields of the political science discipline. Yet three things separate his work from a classic hedgehog strategy. His substantive questions have varied, even if the subfield has not; he has moved from country to country in search of answers; and what is most pertinent to this symposium, his methodological commitments have radically changed over time.

Three of Laitin's books deal with language politics. In *Politics, Language and Thought: The Somali Experience* (1977), Laitin probed the political and social consequences of maintaining a neocolonial language like English, as opposed to using a vernacular like Somali, as an official language. In *Language Repertoire and State Construction in Africa* (1992), he explained how very few African states went for linguistic rationalization in the classical European sense of having only one language, but many others went for two other linguistic strategies: a 2-language outcome, and what he came to call a 3±1 solution, a formula he found in India and has applied to other countries as well. In *Identity in Formation: The Russian-Speaking Populations in the Near Abroad* (1998)–*IF* hereafter—the central issue is how to explain the emergence of a "conglomerate identity," based primarily on linguistic adaptation, among the Russian-speaking populations of Estonia, Latvia, Ukraine, and Kazakhstan after the breakup of the Soviet Union.

His work on identity politics is, of course, not entirely driven by language issues. In his second book, *Hegemony and Culture: Politics and Religious Change among the*

Yorubas (1986)–*HC* hereafter—Laitin asked why Yorubaland's religious life was split between Muslims and Christians, but Muslim-Christian differences were not the principal cleavage in Yoruba politics. Finally, in two co-written articles with James Fearon (Fearon and Laitin 1995, 2003), he has examined the consequences of ethnic diversity for peace and violence. In the first joint article, Fearon and Laitin probed the conditions under which ethnic diversity would actually lead to peace, not violence; and in the second article, they asked whether ethnic diversity was indeed a crucial determinant of civil wars, or whether other factors were more significant.

The range of these questions makes Laitin a formidable intellectual force, indeed a central figure, in the subfield of ethnic politics. One can no longer write about language politics, identity formation, or ethnic peace and violence without engaging his arguments. Moreover, his frequent forays into new empirical terrains add greatly to his output. His case materials have come from Somalia, Nigeria, India, Sri Lanka, Catalonia, the Baltic Republics, Central Asia, and Ukraine. As my own research has become multi-country, it is now clear to me that developing intimacy with new political and cultural materials, a prerequisite for thoughtful work, is not easy. Consequently, I have developed a strong admiration for those who step beyond the existing zones of familiarity and develop ideas on that basis. Laitin has repeatedly allowed his intellectual curiosity to migrate to newer lands, also sometimes linguistically retooling himself. Many have worked on multiple countries; very few have learned new languages. Laitin may have never left the subfield of ethnic politics, but his intellectual journeys within have a fox-like quality.

Laitin's substantive achievements are not the principal issues for this symposium. Rather, our focus is on his methodological moves. The symposium seeks to assess the value of Laitin's methodological voyage from his early work based primarily on ethnography, whether in Somalia or in Ile Ife in Yorubaland, Nigeria, to his work over the last decade and a half, in which a rational choice stance has played a major role.

But before I proceed further, one should note that ethnography and rational choice are not the only methodological alternatives which should be discussed here. Rational choice methodology, which does tend to rely heavily on formal logic and a priori assumptions, as opposed to ethnography which is more empirically driven, is only one of the elements in Laitin's transformation. Some of his more recent work is heavily statistical, and we must draw a distinction between formal and statistical reasoning. If in "Explaining Interethnic Cooperation" (1995; EIC hereafter), assumptions, formal reasoning, and equations abound, and only illustrations from the real world are given but no systematic empirical evidence, in "Ethnicity, Insurgency and Civil War" (2003; EICW hereafter), there are only a few basic assumptions made and recourse to formal logic is minimal. Instead, existing theories of civil war are tested against a large statistical dataset. This kind of work is not ethnographic but it is still empirical, to be differentiated from the rational choice tilt of the EIC.

So how should we judge Laitin's methodological transformation? My central argument in this essay is that methods always entail a tradeoff. Each method in the social sciences can handle some puzzles better, leaving others unresolved. It is not as helpful to say that ethnography is better than rational choice, as to determine what their respective strengths are, and what they can handle best. Creative imagination can allow us to blunt the edges of the tradeoff, but the tradeoffs do not altogether disappear.

In what follows, I will discuss this idea concerning each of the methods Laitin has deployed: ethnography, surveys, and formal modeling. I will primarily use his work as an illustration of tradeoffs, though in the process I will also discuss other work. All three methods have their unique mix of strengths and weaknesses, and we need to decipher what kinds of questions can be best analyzed by each. It is both a question about the strategy and substance of research. Laitin has not always been conscious of this point, nor has he consistently followed it. Since he subscribes to the notion of cumulation in social science, his recent critiques on purely methodological grounds seem quite puzzling and paradoxical. Basically, the form his critique has taken and his commitment to the idea of cumulation are not logically consistent.

Shifts of Evidence, Shifts of Method

Theoretical shifts, especially in light of changing evidence, are quite common in scholarly life. Robert Dahl became skeptical about the pluralist nature of American democracy, once the tight hold of business over American politics became clear to him in the 1970s (Dahl 1982). And as the revolutions overthrowing Communism squarely questioned his assumptions about human behavior, Jon Elster developed serious self-doubt about rational choice theories in the early 1990s (Elster 2000).

These are examples of evidence-based theoretical shifts. Are Laitin's shifts evidence-driven, or method-driven? In his thoughtful essay for this symposium, Ted Hopf (2005) suggests that the reasons are methodological.

But are methodological shifts entirely uncommon? In one of the famous interpretations of Marx's overall body of work, Louis Althusser argued that there was an "epistemological break" in Marx after his early work—before *Das Kapital* was written (Althusser 1969). According to Althusser, "early Marx" moved from the pseudo-scientific methods, when he gave too much emphasis to human consciousness, to science later when he made it unambiguously clear that the structure of production determined the relations of production and, therefore, human consciousness. Epistemology is about the ways of generating knowledge. Willy nilly, it becomes inescapably methodological.

In short, method-based theoretical turns have precedence in intellectual history. Like Marx in his later works, Laitin today tends to start with some universalist, apriori assumptions. Laitin, of course, does not leave it there, and unlike so many rational choice scholars, he does field work as well. But given that his survey questions are based on apriori theory, says Hopf, his empirical testing has become partial:

> I could not find a single case where ethnographic data

> were advanced as evidence in contradiction to the survey data, or still less, as evidence to interpret the survey data that were gathered. The intersubjective world of post-Soviet subjects was accorded far less evidentiary value than the answers to survey questionnaires, questions which were developed in light of a priori theories of the researcher, not from the ethnographic materials he gathered (Hopf 2006, 18).

This way of generating knowledge, Hopf continues, is "directly opposed" to the celebrated opening lines of *Hegemony and Culture* (*HC*):

> When we try to interpret politics in Africa (or anywhere, of course!) in terms of our own structures of preference and categories of action, we learn less about either Africans or ourselves than we do by recognizing that our political understanding is not universal, but is contingent on our sociological and historical experiences." (*HC*, ix).

Hopf is insightfully identifying the difference between surveys and ethnographies here. Though scholars select their ethnographic sites for theoretical reasons, surveys are more theory laden than ethnography. Survey questions are theoretically framed: only some questions are asked, not all possible questions. Ethnography facilitates a much more open-ended "soaking and poking" and, as Hopf puts it, "it lets the subject speak."

Hopf is right about this, but it is also worth asking whether ethnography has some limitations and surveys some advantages. Ethnography clearly allows us to deepen, but surveys make broadening possible. Deepening and broadening as categories of empirical observation generate trade-offs. Ethnography makes accuracy about a case or two possible in a way that surveys can not match; but surveys allow a broader range of observation, covering many more cases than ethnography can possibly do. I find Laitin's belief in *Identity in Formation* (*IF*) that ethnography alone would not take him forward in the Near Abroad well founded.

Though *HC* and *IF* seem to be asking the same broad question–namely, what explains the choice of certain identities as opposed to others–the scale of observation is clearly different. In *HC*, Ile Ife was studied in depth and an assumption was made that it was a microcosm of the entire Yorubaland. In *IF*, unlike *HC*, four countries were observed. Surveys inevitably had to be given greater weight than ethnography. Hopf seems to suggest that Laitin should have done in Narva, his base in Estonia, what he did in Ile Ife, but seeing all of Estonia through the prism of Narva, *let alone three other societies*, was not the purpose of Laitin's research. Nor might it have been a sensible methodological strategy.[3]

This does, however, lead to an important question: can surveys be designed in such a way that they pick up some of the strengths of ethnography? A fuller discussion of this issue will lead us too far in a cognate area. It will suffice to note that making the survey questions about ambiguities, anxieties, fear and hopes—emotions that so often accompany identity politics and in quite intense forms–open-ended, collecting narratives about them and postcoding them (once narratives have been collected) is perhaps one of the best ways to go. This survey strategy is different from following a theoretically determined finite-answers form and, therefore, a precodable format, as is typical of standard surveys. I am currently experimenting with such survey designs in my own research in four countries. The upcoming results will show how far the redesigned survey technique works. Basically, those who survey do not collect narratives, and those who collect narratives do not survey, but there is no theoretical reason to see them as irreconcilable methodological adversaries. They can be substantially combined, blunting the edges of the tradeoff.[4]

Methods and Explanations

This said, another side of Hopf's methodological critique remains. Following his point about how method is deployed in *HC* as opposed to *IF*, one could also see the basic change in Laitin's position on what structural transformations do to human choices. In *IF*, Laitin argues that after a structural transformation brought about by the fall of the Soviet Union, the Russian-speakers in the Near Abroad calculated whether linguistic assimilation was in their interest or not. In *HC*, Laitin had said something dramatically different. The Yoruba did not calculate, when faced with the clear possibility of structural transformation in their political arena. The fascinating question for Laitin's inquiry during his Ile Ife field work became the following: why would the Yoruba still stick to a tribal (ancestral city) identity rather than a religious (Muslim vs. Christian) one, even though a Civil War in Nigeria during 1967-70 attempted to redefine Nigeria into a Muslim North and Christian South, and again, when in the late 1970s, a debate on whether there should be a Federal Sharia Court of Appeal sought to do the same? In their religious life, the Yoruba acted as Muslims and Christians, but they remained politically committed to their tribal identity, refusing to react religiously to the cataclysmic political events. Why?

Laitin explains why rational choice is unable to help him answer this question:

> Rational choice theorists...cannot tell us if ultimately butter is better than guns; it can tell us that at a certain point the production of a small number of guns will cost us a whole lot of butter, and at that point it is probably irrational to produce more guns. Within a political structure, individuals constantly make marginal decisions. [Rational choice] theories can give us a grasp on how individual political actors are likely to make choices within that structure.
>
> [Rational choice] theory cannot, however, handle *long-term and non-marginal* decisions. When market structures are themselves threatened, and people must decide whether to work within the new structure or hold on to the old–without an opportunity for a marginal decision–microeconomic theory is not applicable...Structural transformations–changing the basic cleavage structure of a society–are not amenable to the tools of micro-eco-

> nomic theory... (*HC*, 148-9, parenthesis and emphasis added).

Identity choice was not a marginal, but a structural decision. Rational choice arguments, therefore, were inapplicable.[5] What would apply instead?

> ….Gramsci provides the solution….The model of he gemonic control can help explain the reification of the "tribe" in African politics–why that cleavage became the dominant metaphor for political action and why it persisted... (*HC*, 150).

To explore how hegemony was created, Laitin then goes into history, fixing his gaze on the British colonial period, starting in the late 19th century:

> Claims based on religious identity were expunged from the political arena by British administrators...British administration shied away from the promotion of Christianity… British administrators…feared the revolutionary implications of religious fanaticism (*HC*, 154).

Finally, Laitin sketches the impact of this decades-long principle of British rule on the Yoruba:

> The idea that ancestral city represents 'blood' while religion represents 'choice' is so deeply embedded into commonsense thinking that experience and data demonstrating otherwise fail to disabuse Yoruba people of this 'truth' (*HC*, 159).

This is fascinating puzzle-solving. In many parts of the world, religion is often not seen as a matter of choice, even though it is in principle. Religion is more often seen as an unchangeable reality inherited from forefathers. Moreover, in other parts of the British Empire, the colonial authorities chose religion as a ruling strategy, for example in Northern Nigeria, but in Yorubaland, they chose a different strategy, leaving a quite different legacy. The distinctiveness of the institutionalized commonsense of Yoruba politics, Laitin argues, is thus linked to the contingencies of colonial rule.

Let us now ask how the impact of a structural transformation on identities is handled in the Near Abroad. The unraveling of the Soviet system is in many ways conceptually analogous to the Biafran Civil War. It ended a system as it existed, without making it clear what would replace it instead. In *HC*, "the politicization of communal identities cannot be fully understood by examining the logic of individual choices" (103). In *IF*, whether or not Russian speakers assimilate is based on a strategic interaction with other Russian speakers, conceptualized as a matter of individual choice in a tipping model. In *HC*, calculations about identity, if made at all, were thought to take place in normal times, not in times of structural transformation, for the latter was marked by a radical uncertainty about the future, making a cost-benefit calculus hard to practice. "The level of costs and benefits of different forms of political identification among the Yoruba is not at issue. For a Yoruba to reformulate his political identity on the basis of his religion would involve great uncertainty" (*HC*, 149). In *IF*, Russian-speakers calculate even in times of great structural transformation–namely, the collapse of the Soviet Union.

The remarkable difference between the two methods is thus clear. In *IF*, Laitin starts with an apriori theory of individual choices, as opposed to viewing choices as embedded in a structural context, as was the case in *HC*. But is it necessarily a problem? Is something lost in the process? Hopf (2006) is sure about the great loss, as discussed below. I would like to answer the question in two ways, one of which goes in the direction of Hopf's observation, but the other does not.

Hopf argues that if Laitin had allowed himself to be an ethnographer *a la HC*, he would have found that the absence of Russian ethnonational mobilization in the Near Abroad was consistent with a variable "omitted" from the surveys: Soviet identity. Decades of history had made Soviet identity a lived everyday reality for Russians in the Near Abroad: "In understanding themselves as multinational, as Soviet, the ethnonational axis of identification was simply not available, or salient, to millions of Russians living abroad. Therefore, they did not understand themselves in opposition to Kazakhs, Ukrainians and Estonians, in precisely that way, just as the Yoruba did not mobilize along Christian and Muslim axes of identification" (Hopf 2006, 18).

Notice the role structural context plays here in the exercise of individual choices. Some choices are simply not part of the institutionalized commonsense of politics because of how history played itself out. Hopf's central methodological insight is that *an a priori theory led Laitin to formulate his survey questions in a way that ruled out this explanatory possibility, and an ethnographic soaking and poaking would have made it transparent*.

If true, this is a very big conclusion, for it not only changes how we explain the absence of Russian ethnonational mobilization in the Near Abroad–as a result of each Russian calculating how other Russians will behave, as Laitin does, or as a result of a historically produced choice pattern, as Hopf proposes–but it also shows that an important potential substantial explanation is eliminated by a method relying on apriori assumptions.

In the end, the area experts will have to judge the veracity of either claim. What those of us doing surveys in different parts of the world can do is ask whether questions about a possible Soviet identity were included in Laitin's questionnaire–especially in an open-ended form which allows one to watch against excessive theoretical determination of survey questions. The way Laitin's survey questions are reported in the appendix of *IF* does not make it clear whether he did ask questions about the possibility of a Soviet identity of Russians in the Near Abroad, and in what form.

If Hopf is right, he tellingly shows us the consequences of a method driven by a priori assumptions, but I also wish to argue that the same method has also generated some big ideas. EIC by Fearon and Laitin is another example of an argument based on apriori and universalist assumptions. It proposes "in-group policing," or "self-policing," as a societal mechanism of peace between diverse ethnic groups. The idea is deductively laid out, and Hopf is right that the theory is not empirically tested.[6]

But the fact remains that it is a big and novel idea in the field, and it has a huge empirical potential. The existing theories were either primordial (ancient hatreds), instrumental (political entrepreneurs mobilizing ethnicity for self-serving ends), epochal (arrival of modernity), or institutional (consociational or liberal democracies; voting systems, etc).[7] Using a simple insight that ethnic groups can monitor their own group members much more easily than those of a different ethnic group, Fearon and Laitin turned it into a serious theoretical proposition, elaborated with game theory.

I am empirically testing this theoretical idea in my current project in 18 cities across Indonesia, Malaysia, Sri Lanka, Nigeria, and India, and the chances that some, if not all, of my materials will bear it out are very high. My previous arguments based on six cities in India had proposed a different societal mechanism of peace–interethnic civic engagement, especially in organizations (Varshney 2002). As my research moves further, we will perhaps find out the conditions under which in-group policing works as a mechanism, and conditions under which interethnic engagement does. In short, even though the theory that I will develop will not be universal, it is the universalist assumptions and a deductive mode of theorizing that produced the idea of in-group policing. Fearon and Laitin are certainly taking the world of knowledge forward.

How does this discussion relate to my central argument? The same method that produced in-group policing as an idea *perhaps* managed to rule out, if Hopf is right, the possibility of a Soviet identity for the Russians in the Near Abroad. And the method that identified the role of colonialism in producing institutionalized commonsense in Yoruba politics cannot easily tell us why despite British attempts at creating or freezing an institutionalized Hindu-Muslim divide through electoral rules in India, South Indians managed to escape *Hindu-Muslim* cleavages, instead getting *intra-Hindu* caste divisions as the master narrative of politics (Varshney 2002). Recourse to colonial practices resolves an intriguing puzzle about Yoruba politics and generates a possible idea–contingencies of colonial rule–for portability, without clinching it for all postcolonial analytic sites. Historians had begun to zero in on this idea elsewhere, but political scientists on the whole had not.[8]

Arguments and Statistical Testing

Let us now turn to statistical methods such as regression analysis, and examine the idea of tradeoffs. In EICW, Fearon and Laitin advance the argument about why civil wars occur in a very important way. A widely discussed recent theory, also based on statistical testing, had proposed that the odds of civil war were strikingly correlated with primary commodity exports (Collier and Hoeffler 2001). Primary commodities are "lootable" commodities, and it is "greed" about these resources that drive an insurgency, not "grievances" about ethnic discrimination, argued Collier and Hoeffler. Fearon and Laitin disprove this argument conclusively.

But they run into some trouble when they test another argument—the so-called modernist view of nationalism associated with Anderson (1983) and Gellner (1983), both of whom attribute the emergence of nationalism to the rise of modernity, and claim that nationalism was impossible before the modern age, though their mechanisms are somewhat different (printing press and capitalism for Anderson, and industrialization for Gellner).

For a statistical testing of the modernist argument, Fearon and Laitin needed variables that could measure modernity, or proxy for it. Higher levels of per capita income became their proxy for modernity (Fearon and Laitin 2003, 78), and they find that lower levels of per capita income increased the odds of civil wars, not the other way round (83). Anderson and Gellner, they concluded, were wrong.

Were they? There are two conceptual problems in the way Fearon and Laitin formulated the test, partially inescapable due to the requirements of regression analysis. First, both Anderson and Gellner made *epochal* arguments–arguments that focused on a transformation of human consciousness as it existed in the Middle Ages, once modernity arrived. Higher or lower income of countries today–or since 1945–is quite beside the point. Pre-modern times may have had lower per capita incomes than modern times, but Anderson and Gellner also talk about print capitalism and industrialization. *Their arguments are historically specific*. The only way to test their arguments is to explore whether before the birth of the printing press and/or industry, national consciousness existed.[9] Second, the arguments of Gellner and Anderson are about national *identities*, not nationalist civil *wars*. Having a national identity does not necessarily imply a hunger for war. Identities and wars are conceptually separable.

Thus, regression analysis is a good way to test some theories, not all. Contemporary primary exports are easily quantifiable at a large-n level, but how does one quantify the extent of printing press penetration in a large number of cases in the 17^{th}, 18^{th}, and 19^{th} centuries, and their consequences for human consciousness? And if a large-n dataset cannot be created, how can one run regressions? In the absence of large datasets for the 17^{th} through 19^{th} centuries, one will have to discover a *small number of critical cases* that show the rise of national consciousness before the birth of the printing press and/or industry. The debate between Gellner and Kedourie goes precisely in that direction (Kedourie 1993, 136-144).

I hope it is now clear why we should not damn methods as intrinsically superior or inferior, neither ethnography, nor surveys, nor for that matter deductive work, whether conceptualized formally (as in game theory) or informally (as in the writings, let us say, of a Rawls or an Elster). We should simply recognize the potential and limits of each method, and we should see whether the method proposed is suitable for the problem at hand.[10]

This is true even in the natural sciences. Einstein famously argued several decades back that physics and meteorology will neither have the same methods, nor the same degree of predictiveness. Physics typically studies a few variables in interaction, allowing parsimony and predictive accuracy. Meteorology has so many variables that having more powerful computers, which he saw coming, would only allow us to predict whether a broad area would get hurricanes in the week or so ahead, not predict that months in advance and if closer to time, not predict whether a specific town or village would be hit and with what intensity. Einstein's reasoning was clear: it is the number of variables affecting the path and intensity of a hurricane (or a snow storm) and their very complicated interaction that was at issue here, not our computing powers. Meteorological problems cannot be reduced to a few variables, as in physics. Likewise, some problems in politics may well allow the parsimony of physics, but others may be more like meteorology, requiring very different kinds of conceptualization and measurement.

I should add that my argument about methodological choices entailing trade-offs is consistent with some new work on methods, both of the quantitative and qualitative sort (Brady and Collier 2004). It also underlines the value of methodological pluralism in the social sciences. Methodological pluralism is defensible not because anything goes, but because different methods will do different things well.[11]

Further Implications

For Laitin, this argument has some further implications. His movement from ethnography and case studies to surveys, formal reasoning, and statistical testing should allow him to deal with puzzles of a large variety. At the same time, his denunciation of other people's work in his more recent scholarly phase is puzzling and hugely paradoxical. Consider two examples, one about case studies, another about selection on the dependent variable.

Laitin finds case studies unacceptable unless the study of a single country is "transformed into a high-n research design, thereby increasing the scientific leverage" (Laitin 2003, 180). My argument with this reasoning is not that turning a country study into a high-n design is wrong. Rather, I have problems with Laitin's insistence that that is the *only way* to save case studies, or by extension, ethnographies, which tend to study a village or a town.

Paradoxically, Laitin's argument today amounts to denouncing his old scholarly self, so evocatively in evidence in *HC*. More generally, Laitin's insistence ignores the fact that case studies can contribute to cumulation by producing intriguing ideas, even when the n is equal to one. This is true of *critical* cases, which even the more statistical view of King, Keohane, and Verba (1994) accepts as valid. To recall, critical cases are those that, given theory, you would least expect to have outcomes that they do. With extensive low incomes and widespread illiteracy, India should not have been democratic, but it is. With little sanctity of private contracts, virtually no restrictions on the powers of the state, and a highly underdeveloped capital market, China should not have been an economic dynamo for two and a half decades, but it has been. India and China thus become critical cases for the theories of democracy and economic growth. Theoretically unselfconscious case studies are a problem, not country studies that are not transformed into a high-n design.

Laitin's critique of studies that select on the dependent variable is also oddly self-defeating (Laitin 2003, 179). It ignores, first of all, the value of his own *IF*, where he studies identity formation only in those parts of the former Soviet Union where conflict was absent or low in the immediate post-Soviet phase: Kazakhstan, Estonia, Latvia, and Ukraine. He does not study the Chechen region, which had a lot of conflict and could have had very different identity outcomes.

Moreover, some of the most instructive social science work in recent decades selects even more on the dependent variable than Laitin does in *IF.* Sen's *Poverty and Famines* (1981) and Bates' *Markets and States in Tropical Africa* (1981) are the best examples. Both are widely viewed as classics of the development field, and justly so. Sen's theory of famines was based on five famines; there were no half-famines or non-famines in his research design. And as Rogowski pointed out long ago (1995), Bates only studied agricultural stagnation in Sub-Saharan Africa, not cases of agricultural success.[12]

It may be true that in most cases, it will be hard to clinch an argument we want to make if we have no variation on the dependent variable. But that is not the only way to contribute to knowledge. Both Sen and Bates did two notable things. First, they thoroughly undermined an existing conventional wisdom: food availability decline as a cause of famine, and Africa's cultural taste for leisure over work, producing backward bending supply curves instead of upward sloping ones, leading to agricultural stagnation.[13] Second, they put a new idea on the table for others to work with: entitlement failures as a cause of famine, as in Sen's argument, and self-seeking behavior of urban politicians, buying the rural rich through subsidies, and running policies that hurt the countryside as a whole, as in Bates.[14]

Research designs that select on the dependent variable can often do both of these, and that is reason enough to see them as contributions. Clinching theories in an ideal fashion is one way to contribute; undermining existing popular theories and presenting elements of a new are another way. Interestingly, in *IF*, there is a point where Laitin says something similar (Laitin 1998, 325), but he nonetheless attacks such studies elsewhere for they do not contribute to cumulation (Laitin 2003, 179).

Conclusion

Laitin has made remarkable contributions to our knowledge, becoming a central figure in the subfield of ethnic politics. The kind of methodological evolution he has undergone is also uncommon in the profession. For both of these reasons, substantive and methodological, his scholarly output inspires admiration. The admiration would be infinitely greater if he could view methodological choices as consisting of tradeoffs and could thereby view work emanating from methods not currently favored by him as also contributing to the cumulation of knowledge, for which the intellectual case is quite clear. It will also save him from self-inflicted paradoxes and contradictions.

Notes

[1] I would like to thank Anna Grzymala-Busse, Ira Katznelson, David Laitin and Daniel Posner for some penetrating comments on an earlier version of the argument presented here.

[2] For a fuller development of this distinction, see Varshney (2003b). See also Isaiah Berlin, 1979.

[3] Whether Laitin should have engaged in four ethnographies, selecting a central site in each country, is an important issue, and worth thinking about. But studying all four societies from the microcosm of Narva would not have been methodologically valid.

[4] For some early thoughts on these lines, see Varshney, 2002, 19-20.

[5] Whether this critique of rational choice theory is right is a different point altogether. On what kind of rationality might apply to non-marginal decisions like identity choices, see Sen (1982) and Varshney (2003a).

[6] Only examples of an approving sort are listed by Fearon and Laitin. See the empirical critique of Horowitz (2001, 475-6).

[7] A detailed elaboration of these traditions can be found in Varshney (2002), Chapter 2.

[8] With the exception of Benedict Anderson (1983), especially in his account of "Creole Pioneers."

[9] Alternatively, can better proxies be developed for epochal arguments? One should, of course, remain open to such possibilities.

[10] In one of his recent essays, Laitin (2003) appears to have partly moved in this direction. He argues for a tripartite method: formal reasoning, statistical testing, and narratives. But it is unclear whether the ideal set forth has ever been realized, or can be. Moreover, considerable paradoxes in that position also remain, as discussed later.

[11] On this matter, also see Laitin (2003) for a different view.

[12] To be fair to Bates, he does mention Kenya and Ivory Coast as cases of relative agricultural success, but that account comes at the end and is very brief. Basically, variation in outcomes is not the centerpiece of the argument.

[13] See Varshney, 1995, Ch. 2, for how popular the theories of backward bending supply curves were.

[14] This idea did have a prior lineage in Lipton (1977) and Schultz (1980), but Bates provided the most convincing links between politics and economic outcomes. Lipton and Schultz assumed that the urban bias of the political structure produced anti-agricultural outcomes. Bates showed exactly the links worked.

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Ethnography and/or Rational Choice: A Response from David Laitin

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As Ted Hopf presented his paper at the symposium held at the 2005 annual APSA meeting, provocatively titled "Being David Laitin," I felt as if I were in a chute on the 7½th floor of the Marriott Wardman Park, ready to be discharged onto the New Jersey turnpike. But I survived, enough so to offer the following remarks.

The key substantive theme raised by the papers in the symposium is the relationship of ethnography and a theory of purposive action. In the 1950s, the eminent anthropologist Frederic Barth encountered the work of John von Neumann and Oskar Morgenstern, and immediately saw the deep implications of their game theory for anthropology. He then wrote a game theoretic essay (Barth 1959) analyzing chieftaincy politics among the Pathans. This was one of the few lead balloons that Barth let fly in his distinguished career, and the anthropological field has steered clear of game theory ever since. But the scholarly relationship between game theory and ethnogra-