

Mechanisms v/s Outcomes

Essay for symposium on David Laitin's work
Newsletter of the APSA Section on Qualitative Methods (forthcoming)

Kanchan Chandra
New York University
kanchan.chandra@gmail.com

Mechanisms v/s Outcomes

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.” (p. 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 studies the particularities of countries including Somalia, Nigeria, India, Spain, Estonia, Latvia, Ukraine and Kazakhstan in order to produce knowledge about other countries in other 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 – does 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 approach that tells us what to look for in the data. A forced comparison between the two suggests simply they are simply two overlapping ways of identifying mechanisms, among many other ways. They are not mutually exclusive – both can be used to identify mechanisms. Neither are they exhaustive. A scholar could use neither and still produce illuminating research. We should be arguing about what kind of knowledge to aim for,

rather than privileging one or two approaches among a multitude of possibilities for generating that knowledge.

(4) The ambition of a mechanism-oriented approach is different from that of an outcome-oriented approach – and should produce different work. An outcome-oriented approach aims to use within country studies to impose a uniform explanatory framework -- with the same independent variables explaining the dependent variable -- on as many countries as possible. The ambition of a mechanism-oriented approach is to use the study of one country to produce more sophisticated questions about new countries and get us closer to a series of unique point predictions about outcomes in other countries without the expectation that these outcomes will be the same in all countries.

Many of us in comparative politics would balk at this advocacy of point predictions. We tend to believe that “science” is about generalizing about outcomes across countries. I do not argue here that we should not aim for such generalizations. Some approaches that are, and should be, well accepted in comparative politics -- such as cross-country statistical work -- do indeed identify general patterns across countries that can be justified according to prevailing social scientific standards. But when working with within-country studies, there is a conflict between “science” and this type of generalization. When transplanting knowledge from within-country studies, it is scientifically more defensible to aim for unique point predictions in additional countries.

I read the progression of Laitin’s work in the past twenty years as an evolution from a concern with making generalizations about outcomes from within-country studies to a concern with making generalizations about mechanisms -- and in particular, mechanisms about strategic action. Correspondingly, his work has also implicitly moved away from a concern with making predictions about outcomes intended to apply to a broad universe of cases to a concern with making a series of point predictions about individual countries. In the process, it has become more scientific.

1. Outcomes and Mechanisms

Outcomes and mechanisms have been distinguished in several ways. One way of making a distinction is to say that outcome oriented analyses are about correlations (or laws), while mechanism oriented analyses are about causal paths (Elster 1989, 3-10). But what is a causal path if not a series of correlations? Another is to say that the difference between outcome and mechanism is the difference between structure and process. But that just shifts the question – what is structure and what is process? A third is to say that outcomes are “tangible, observable things”, while mechanisms are unobservable mental processes. But what seems to be a “tangible, observable thing” is often a realization of unobservable idea – “democracy,” for instance, which we routinely think of as an outcome, is an intangible idea for which we have developed measures and indicators. And what seems to be an unobservable mental process is often something that can be observed and measured – for instance, we could measure “beliefs” by developing indicators based on survey data or experimental data that test for actions consistent with some beliefs and not others.

I take the difference between an outcome-based analysis and a mechanism based analysis to be a difference in the *degree of the fineness* of the analysis rather than a difference in *what* is being explained. An outcome based analysis makes a statement about a “macro-correlation” between two variables:

1 → 2

Mechanism-based analyses are statements about the series of “micro-correlations” that constitute the logical chain linking the macro-correlation.

1 → 1.1 → 1.2 → 1.3 → 1.3 → 1.5 → → 2

I use a number series to illustrate the difference between the two because it illustrates nicely that there is no limit to the fineness of analysis that can be used in a mechanism-based approach. Just as there is an infinity of points between two numbers on a continuous scale, there is an infinity of micro-correlations that constitute the larger correlation.

But there is no difference in the essential nature of the dependent variable that either an outcome-oriented or a mechanism-oriented approach attempts to explain. The difference lies only in the distance of the explanatory variable from the dependent variable. In an outcome-based approach, the independent variable is distant from the dependent variable. The correlation between the two, therefore, is not obvious. In a mechanism-based approach, each micro-correlation links two proximate points. Thus, each micro-correlation may well be obvious. But a sequence of micro-correlations taken together travels a great distance from the initial variable that triggered them, and produces a non-obvious outcome.

2. Why we should not expect within-country studies to produce generalizations about outcomes across countries.

Within country studies in comparative politics now routinely present themselves as using controlled comparison to identify hypotheses about outcomes that can be generalized to outcomes in other countries -- or are evaluated on the ability to produce such generalizations. Laitin's 1986 book *Hegemony and Culture* is an example.

The outcome that Laitin wants to explain in this book is the non-politicization of religion in Yorubaland in Nigeria. Although both tribe and religion are socially salient cleavages in Yorubaland, tribe is politicized and religion is not. The key independent variable explaining this outcome is the ideological hegemony instituted by the colonial state. British colonialism in Yorubaland adopted a system of indirect rule which created a common-sensical world in which tribe was “real” and religion was not. Consequently, long after the departure of the British, the Yoruba organized their politics on the basis of tribe rather than religion. Within the context of Yorubaland, the simple bivariate correlation unearthed by the book is:

Colonial Hegemony → Post-colonial Politicization of a Cleavage

If we wanted to use this within-country study to explain outcomes in another country what should we do? At first glance, it appears that we should look for the same simple bivariate correlation between the cleavage institutionalized by the colonial state and the cleavage that becomes politicized among the public. Indeed, this is what Laitin does in his concluding chapter, where he attempts to “test” the hypothesis developed from Yorubaland in the case of Benin. The book finds that the pattern of politicization in post-colonial politics can be explained by the pattern of

politicization adopted by colonial rule. Consequently, Laitin argues that Benin “demonstrates the power ... of the model of hegemony” (p. 165).

But if we really wanted to test the theory of theory of outcomes in Yorubaland in other countries, the first step would be to note that this theory is not bivariate at all. It can be taken as a bivariate relationship only in the context of Yorubaland, when all other variables are held constant by virtue of the selection of the case. These control variables include any number of things, including the economy, the ethnic demography, the duration of colonial rule, climate, ecology, history, patterns of past violence, political leadership, and institutional structure, only some of which can be explicitly identified. These controls make it possible to isolate the impact of colonial rule in the case of Yorubaland. But when exporting the model to other cases, the researcher must also export the controls within which it is embedded, implicitly or explicitly. Thus, the test should really read:

Colonial Hegemony + Economy + Ecology + Climate + History + → Post-colonial Politicization of a Cleavage.

Unless this entire model is exported, we do not know whether it is corroborated or not. The fact that Benin corroborates the model, thus, is a false positive, since we do not know whether the control variables, on which the effect of colonial hegemony was contingent in Yorubaland, also took on the same value in the case of Benin. And had Laitin reported a case which disproved the bivariate correlation – i.e. showed that the institutionalization of cleavages during colonial rule was not associated with the post-colonial politicization of cleavages -- it would have been a false negative, since that simple correlation is not in fact the true model.

Could Laitin have tested the “correct” model? Could anyone, on the basis of a within-country study? I don’t think so. To conduct the right test would mean that all the control variables embedded in the case selection would have to be made explicit and entered into the “export” version of the model. But this is not possible, because it is not clear what those control variables are. There are an infinity of things that are controlled for implicitly when a researcher chooses a controlled design within a country. That is what makes a within-country design attractive as a natural experiment. How might we put these many things into a model? A researcher might assert that not all of these controls are relevant – that only two of the possible infinity of variables matter and thus only two need to be exported to a new country or countries. But the research design does not permit that inference, since there is no variation in the control variables on the basis of which the researcher could have determined the relevance or irrelevance of these variables. And even if a model could be specified, we could not estimate it, because it would be heavily overdetermined, with more variables than cases.

I have used *Hegemony and Culture* as an example, but many within-country studies in comparative politics fall into the same mold. Like *Hegemony and Culture*, their titles advertise simple bivariate correlations. *Making Democracy Work: Civic Traditions in Modern Italy* (Putnam 1993) advertises a bivariate correlation between civic traditions and the performance of democratic institutions. *Ethnic Conflict and Civic Life: Hindus and Muslims in India* (Varshney 2002) advertises a bivariate correlation between civic life and ethnic conflict. *Institutions and Ethnic Politics in Africa* (Posner 2005) advertises a bivariate correlation between institutions and the politicization of cleavages. *Votes and Violence: Electoral Competition and Ethnic Riots in India* (Wilkinson 2004) advertises a correlation between electoral competition and ethnic riots. My own work *Why Ethnic Parties*

Succeed: Patronage and Ethnic Headcounts in India (Chandra 2004) advertises a bivariate correlation between patronage and the politics of ethnic headcounting. The correlations are surely contextualized by the names of countries and regions – “in Italy,” “in India,” “in Africa.” But the authors aim in these within-country studies to produce hypotheses that explain outcomes in other countries, and are evaluated on the basis of this ambition.

For the reasons given above, this ambition is unlikely to be realized, and this standard of evaluation unlikely to be met. Indeed, while we can think of numerous examples of within-country studies that identify interesting, internally consistent, bivariate hypotheses, it is difficult to think of bivariate hypotheses generated from within-country studies that have actually been verified through cross-national research.

3. Why theories of outcomes within countries should produce generalizations about mechanisms across countries.

Had Laitin wanted to generalize about mechanisms rather than outcomes based on *Hegemony and Culture*, what might he have done? The first step would have been to conduct a far more fine-grained analysis. A disaggregated version of the analysis in the book as it stands is as follows:

Colonialism → Indirect Rule → “Religion ‘expunged’” from commonsensical assumptions about politics in the colonial period → Post-colonial Politicization of a Cleavage.

The greatest level of disaggregation in the book is in the analysis of colonialism. But it jumps over the many links that might conceivably link colonial politics to post-colonial outcomes. A mechanism oriented approach might have produced an analysis that looks as follows:

Colonialism → Indirect Rule → “Religion ‘expunged’” from commonsensical assumptions about politics in the colonial period → Additional Variable 1 → Additional Variable 2 → Additional Variable 3 → Additional Variable 4 → → Post-colonial Politicization of a Cleavage.

Generalizing from this study to a different country, then, would mean checking to see whether particular links in the chain of micro-correlations are replicated in another country – and, since we can almost be sure that it will not, identifying the point in the chain at which events in the other country diverge.

The purpose of such a generalization is not to “test” the hypothesis generated in the first country – a validation of that sort, I argued above, was not possible. But it is to lead the researcher to narrow the field of inquiry in the new country by taking her towards a narrower and more sophisticated set of questions.

Let me illustrate by showing how *Hegemony and Culture*, based on Nigeria, narrowed the field of inquiry for Posner’s inquiry into ethnic politics in Zambia (Posner 2005). Posner was interested in a similar question – what explains the pattern of politicization of cleavages in post-colonial Zambia? But Laitin’s book illuminated the place to start in looking for an explanation. Rather than having to start from the beginning and consider all possible hypotheses, Posner was able to narrow his field of inquiry to the effects of colonialism. He found that the first few links in the chain developed by

Laitin in Nigeria, which showed how colonial rule narrowed the set of options open to elites in post-colonial politics, worked in the same way in Zambia -- and then filled in the remaining links from colonial to post-colonial politics through a new model of institutional politics that highlighted the role of electoral and party systems:

Colonialism → Indirect Rule → "Religion 'expunged'" from commonsensical assumptions about politics → Party System → Electoral System..... → Post-colonial Politicization of a Cleavage.

This analysis could of course be disaggregated still further. But this illustrates, I think, the contribution of Laitin's study *Hegemony and Culture*, and the way in which it should be evaluated. It is not a study that yields generalizations about outcomes which validate the theory and elucidate universal (or even regional) patterns. I cannot think of any within-country study in comparative politics that has done that. But its contribution lies in its identification of generalizations about mechanisms, which narrow the field of inquiry for others and produce more sophisticated point predictions.

Before going further, let me address two immediate questions:

(1) Why isn't a mechanism-based approach subject to the same criticisms about controls as an outcome-based approach? If a macro-correlation cannot be exported to a new country without its attendant controls, why is a micro-correlation exportable without those controls?

The critique of outcome-based approaches, which emphasized that the effect of one independent variable is contingent upon the presence of the control variables specified in the model, applies to enterprises which are trying to test a theory. If we interpreted the replication of each micro-correlation in a new country as a "test" of the argument produced by a previous within-country study, the same criticism would hold. But rather than thinking of the presence of each micro-correlation as a verification, we should think of it as a lever to identify whether or not there are variables that matter in the new country that did not in the first. If a micro-correlation is replicated, we should ask why, given the variables that characterize the new country. And when the paths begin to diverge, we should also ask why. Spotting repeated mechanisms in a new country need not mean that the same variables are at work in each country – the same mechanism might be produced by a different variable. By forcing the researcher to identify variables repeatedly in the course of building an explanation, a mechanism-approach is designed to uncover the importance of other variables – it may even be biased in favour of discovering additional variables.

(2) Why should we change our standards of evaluation from outcome-based approaches to mechanism-based approaches? Isn't a mechanism based approach simply an imperfect rendering of an outcome-based approach? If so, we should continue to aim for generalizations about outcomes, since holding on to an ideal standard is important in inspiring research.

Our standards of evaluation determine the type of research that we do. One reason to switch from standards that value generalizations about outcomes to standards that value generalizations about mechanisms is that this will lead us to look for different things. Had *Hegemony and Culture* been written in a field that valued mechanisms more or as much as outcomes, my guess is that the level of analysis would have been far more fine-grained than it was, and narrowed the field of inquiry for

other researchers more than it did. Changing our standards may improve the quality of what we do in another way. Standards that value outcomes over mechanisms impose a direct contradiction between the scientific nature of the findings, which are contingent upon controls, and the demands of generalization in comparative political “science.” This contradiction normalizes an implicit disbelief in the way that research is done in comparative politics. Authors do not believe that they are producing generalizations about outcomes that apply across countries, but insert obligatory claims about such generalizations to be uncovered by “future research” in order to prove their professional credentials. Readers do not often believe these claims but look for them anyway, as the obligatory evidence of credentials by comparative political scientists. This institutionalized disbelief has a corrosive impact on the quality of research over the long run. Bringing our beliefs about what we should do more in line with what we actually can do may produce better quality research.

4. The Movement to Mechanism-Based Approaches in Laitin’s work

I read the evolution of Laitin’s work since *Hegemony and Culture* over the last twenty years as a movement away from generalizing about outcomes to generalizing about mechanisms (Laitin 1989, Laitin 1992, Laitin 1998). The central question in this body of work is – what will the language outcome be in new states? Should we expect the development of a single national language? Laitin used a game theoretic representation of language outcomes in Western Europe to narrow the field of inquiry in India (Laitin 1989), the study of India to narrow the field of inquiry in Africa (Laitin 1992), and the studies of India and Africa, among other countries, to narrow the field of inquiry the former Soviet Union (Laitin 1998). The particular language outcomes in all these countries and regions were different. But, as I discuss below, the chain of mechanisms identified in each case helped to identify further mechanisms, and explain the particular outcomes, in every subsequent case.

The study of Western Europe generated chain of mechanisms that produced an outcome unique to Western Europe (Laitin 1992, 35-36):

Rationalizing policies of the state → Compliance by regional elites → Single language outcome

Starting with this initial path, Laitin identified a point of divergence in India:

Rationalizing policies of the state → Compliance by bureaucratic elites → Compliance by people (rather than regional elites) → Adoption of 3 ± 1 language outcome.

The point of divergence in India from the path in Western Europe was driven by the timing of language rationalization, and the presence of different sets of actors. Language rationalization in Europe, which took place in a pre-democratic framework, was a matter decided upon by regional elites and those in control of the state. But the attempt at language rationalization in India took place after the introduction of democracy, and two other sets of actors were involved – bureaucratic elites, and the people, rather than elites, of individual regions. Laitin was able to map the path in India not by starting afresh, but by starting with the path identified in Western Europe and then identifying this point of divergence.

The path in India then illuminated a more complex path in African states, with several additional variables:

Rationalizing policies of the state → Compliance by bureaucratic elites → Compliance by people
→ Number and distribution of languages → Administrative Structure → Political Leadership →
Unique Language outcomes (One language (Somalia), two languages (southern Africa), 3 ± 1
(possibly Nigeria) and so on))

As we see, the precise path varies from country to country. But it is not completely different – several mechanisms recur across all pairs of countries, and there is at least one mechanism that recurs in all countries, taken together. This recurrent mechanism is the “private subversion of a public good.” Although individuals all cheer the adoption of a language rationalization policy on paper, it is not individually rational for them to switch to a single language themselves or educate their own children in it. Consequently through a series of individual decisions they subvert the policy in practice.

The result is a set of unique outcomes in each country, illuminated, but not determined by, the path of the previous countries studied. Indeed, as Laitin notes: “The most important general finding of the game-theoretic analysis is that the “players” involved in state construction are different over the centuries, leading to differently constituted language games and of different equilibrium outcomes (Laitin 1992, 119). This is a very different type of analysis from *Hegemony and Culture*, where the existence of different equilibrium outcomes across countries would have been seen as a failure of the analysis rather than its most important finding.

In his most recent book *Identity in Formation*, Laitin takes just one link in the chain of mechanisms identified in previous work– the compliance of ordinary people with policies aimed at language rationalization – and disaggregates it still further, going down to an individual level of analysis which was missing from the first two books (Laitin 1998). This study, perhaps to a greater degree than any of the others, illustrates the extent to which adhering to the scientific standards of controlled comparison within a country makes it difficult to generalize about outcomes across countries. The book, which compares different linguistic strategies of Russian-speakers across the republics Kazakhstan, Estonia, Latvia and Ukraine makes good use of the strategy of controlled comparison to generate its findings – it focuses on the same linguistic group with the same history in the same region, in similar settings – and shows how variation in individual strategies is explained by a difference in the expected payoffs of compliance and the proportion of co-ethnics who comply across the four countries. The same controls that make this study scientific also make it difficult to generalize about outcomes across countries. Indeed, Laitin is careful not to make predictions about language assimilation outside the former Soviet Union. But what this study contributes to scholars studying other countries is an individual-level mechanism which may well recur across countries, and, when linked to other chains of mechanisms, explain unique outcomes in those countries.

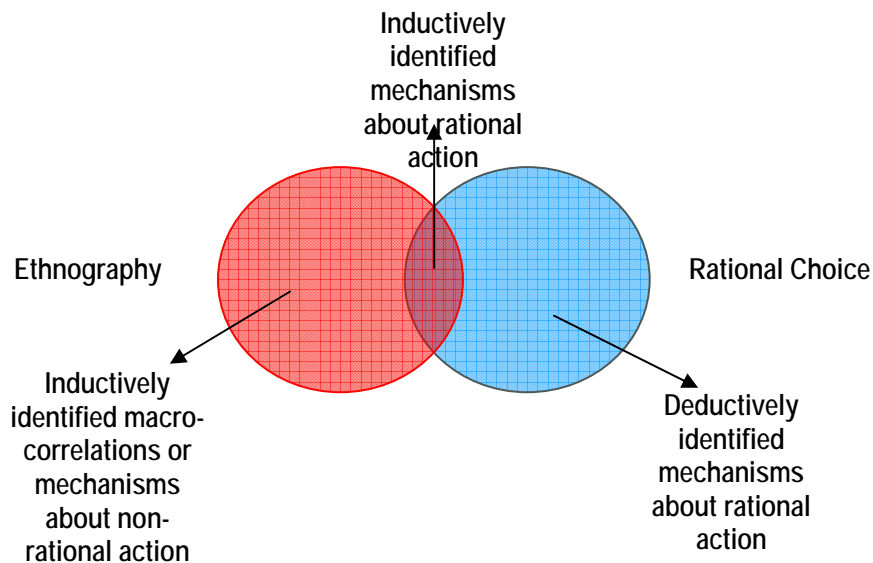
5. Ethnography and Rational Choice are two overlapping and non-exhaustive ways to identify mechanisms.

“Ethnography” and “Rational Choice” are not strictly comparable. Ethnography is the firsthand personal study of a small group of people in a local cultural setting. As such it is simply an approach to how data should be collected, without assumptions about the kind of behaviour the data should reveal. Rational choice, on the other hand, tells us how to model behaviour, or how to

interpret data on behaviour, rather than telling us how to collect the data in the first place. The model of behaviour requires self-interested, goal-oriented actors who choose between a finite set of alternatives based on a calculation of the costs and benefits.

To the extent that we can compare them anyway, there is an overlap between the two. Ethnographic methods can be used to probe the data for whatever relationships the researcher is interested in – the relationship between an independent variable and an outcome, or the sequence of mechanisms leading from the independent variable to the outcome. Ethnography might yield macro-correlations or mechanisms that are consistent with a rational choice model as well as macro-correlations or mechanisms that are not. Ethnography requires only that the macro-correlations or mechanisms are inductively generated in a particular way – by studying people in their cultural settings. A rational choice approach, on the other hand, is concerned explicitly with mechanisms that assume or reveal self-interested, calculated actions. These mechanisms may be deductively or inductively identified. Indeed, although it is common to associate rational choice theory with purely deductive thought, many of the most important rational choice arguments in comparative politics are derived from and embedded in particular empirical contexts -- Down's *Economic Theory of Democracy* (1957), for instance, is informed especially by the dynamics of the two-party democracy in the United States, and Riker's theory of federalism (Riker 1964) rests on assumptions informed by the particular path of American federalism. The area of overlap between the two approaches, thus, lies in the area of inductively developed mechanisms about rational action. This overlap is represented in the chart below:

Rational Choice and Ethnography



Each approach has its comparative advantages, allowing us to model certain types of mechanisms, at certain levels of analysis better than others. The problem dictates the choice of approaches or the combination of them. This is by now a standard pious statement in the methodological wars in comparative politics, usually used to argue that there is no one right way for

how all research should be done. That's true – but it also means that for particular problems, there is one right way – or a small set of right ways. Problems do dictate choices of methods, and if we do not adopt certain methodological approaches, we cannot solve certain problems.

Let me illustrate again through Laitin's work. In his first book, *Politics, Language and Thought* (Laitin 1977), Laitin argued that the choice of a single state language is not neutral – it creates (or maintains) an elite, composed of those who have access to education in the state language, and a subordinate class (or classes) of those who do not. Once we accept that the choice of official language puts some people in a position of disadvantage and others in a position of disadvantage, it is natural to ask how expectations about an outcome affect the outcome itself by affecting the strategy of political actors in the present. If English speaking Somalis know that the choice of Italian rather than English will affect their job prospects in the future, then might this not affect the language that is actually adopted by informing their strategy in the present?

Rational choice approaches are the principal family of approaches which have been used to theorize about problems of this nature. Thus, if a scholar wants to model the relationship between expectations and outcomes, she must turn to rational choice methods. She may very well discover other approaches that are eventually superior – but this is the logical place to start. Not surprisingly, then, Laitin has tried to integrate rational choice into his study of language outcomes in every book on this subject since the publication of *Politics, Language and Thought*. He has used ethnography – but also history, survey research and experimental methods -- to map the preference structures of individuals and collective actors – and then rational choice analysis to predict actions given this preference structure. Other research problems may well suggest different approaches to identifying mechanisms.

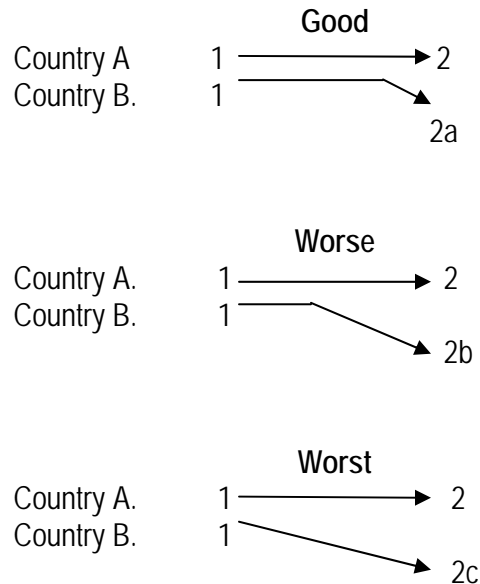
In recent work, Laitin proposes that this particular combination of approaches – formal theory, used mainly to mean rational choice theory, and narrative analysis, which I take to mean mainly ethnography and historiography – should become part of a standard, “tripartite” method in comparative politics, along with statistical methods (Laitin 2003a, Laitin 2003b). Given the context of the methodological debates in comparative politics, this appears to be a broad approach, suggesting, as he has demonstrated in his work, that both methods can be used in a complementary way and one need not be chosen at the expense of another. But if we think outside that context, this is an unreasonably narrow statement. Ethnography and rational choice are not an exhaustive set of approaches which can be used to identify mechanisms. Depending on what kinds of questions the researcher is interested in, almost any technique can be used. Those that are being used in political science so far include survey research, experimental research in the laboratory or in the field, agent-based modeling, among others – and there are many other approaches that may be imported from other disciplines, including neurobiology, psychology, psychoanalysis, even literary analysis! Why restrict ourselves to only these two?

6. How should we evaluate the quality of a mechanism based approach?

The quality of a mechanism-based approach can be measured by the length of the chain of micro-correlations uncovered in one country that is replicated in a new country or countries. The longer the chain of micro-correlations in the new country that is illuminated by a previous study, the narrower the field of inquiry becomes in that country, and the more precise the remaining questions she needs to answer in order to generate a point prediction for that country.

Consider the scenarios represented below. In each case, the study of Country A is an initial within-country study which produces a chain of mechanisms, represented by an arrow, linking variable 1 to some outcome. We would like to generalize on the basis of this study to country B. Note that in all three scenarios, the same independent variable produces different outcomes in country A and country B. But the three scenarios differ in the extent to which the chain of mechanisms triggered by variable 1 in Country B runs parallel to the chain of mechanisms triggered by variable 1 in Country A.

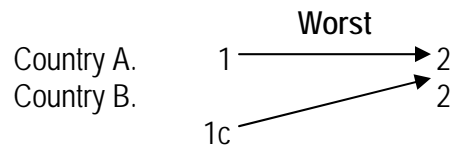
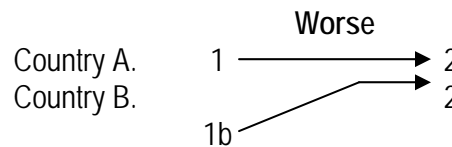
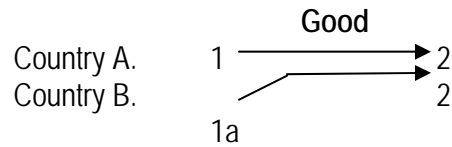
Same independent variable, different outcomes across countries



An outcome-based approach would judge all three scenarios to be of equally poor quality, since the same variable is not associated with the same outcome across both countries in any of the three scenarios. But a mechanism based approach would judge them differently. In the first column, the chain of mechanisms identified in Country A dramatically narrows the scope of inquiry in country B. In trying to explain the same outcome in Country B, we find that we can start at the same place as in country A and follow along a great distance before the paths diverge. This is an example of a good generalization. In the second column, the chain of mechanisms identified in Country B takes us a smaller part of the way towards explaining an outcome in country B. This is an example of a worse generalization. And in the third column, identifying the chain of mechanisms in Country A that produce the outcome in Country A tells us nothing about the mechanisms that produce the outcome in country B: the two paths diverge at the outset. This is the worst of the three generalizations.

We might also, in generalizing from one within-country study to another, move backwards from an outcome to a set of independent variables. Consider the scenarios below. In each scenario, the outcome is the same in Country A and Country B -- but the initial independent variable that produces the same outcome is different.

Different independent variable, same outcome across countries



According to an outcome based approach, there is little to choose between the three scenarios – since the same outcome is explained by a different variable in each country in each scenario, all three are equally bad. But the three scenarios are not equivalent according to a mechanism based approach. The first scenario shows a higher quality generalization about mechanisms than the second, since the chain of mechanisms from Country A that is replicated in Country B is longer. And the second scenario shows a higher quality generalization than the third, by the same logic.

In each case, what the mechanisms identified through other the study of one country can do in generalizing to other countries *is tell us where to look and what questions to ask – but not whether the answer is correct.* The test of whether each link in the chain of mechanisms that explain an outcome in a new country is the correct link should be a within-country test, since only within-country tests can control for all the variables in that context. Continuing with the example of Laitin's work, if we want to test for whether the mechanism of the private subversion of a public good identified in Somalia recurs in India, we cannot assume that finding the same mechanism in both countries is evidence that it is at work in the same way across countries. We would want to identify observable implications of the argument about the working of the mechanism and test these implications using variation across space or time in India. It is through this painstaking series of questions and tests that we can get to unique point predictions for individual countries. And if the study of a phenomenon in one country shows scholars who study other countries which questions to ask, and which tests to perform, in order to generate point predictions for those countries, then that should count as progress in comparative politics regardless of whether these studies, taken together, add up to some universal explanatory framework.

References

- Chandra, Kanchan (2004). *Why Ethnic Parties Succeed: Patronage and Ethnic Headcounts in India*. Cambridge University Press.
- Downs, Anthony (1957). *An Economic Theory of Democracy*. New York: Harper.
- Elster, Jon (1989). *Nuts and Bolts for the Social Sciences*. Cambridge: Cambridge University Press.
- Laitin, David. (2003a). "The Perestroika Challenge to Social Science." *Politics and Society* 31, 1: 163-94.
- Laitin, David. (2003b). "Comparative Politics: The State of the Subdiscipline." In Ira Katznelson and Helen V. Milner, eds. *Political Science: The State of the Discipline*. New York: Norton.
- Laitin, David (1998) *Identity in Formation: the Russian Speaking Populations in the Near Abroad*. Ithaca: Cornell University Press.
- Laitin, David (1992). *Language Repertoires and State Construction in Africa*. Cambridge: Cambridge University Press.
- Laitin, David (1989). "Language Policy and Political Strategy in India." *Policy Sciences* 22, 4: 415-36.
- Laitin, David (1986). *Hegemony and Culture*. Chicago: University of Chicago Press.
- Laitin, David (1977). *Politics, Language and Thought: The Somali Experience*. Chicago: University of Chicago Press.
- Lijphart, Arend (1977). *Democracy in Plural Societies*. New Haven: Yale University Press.
- Lijphart, Arend (1975). *The Politics of Accommodation: Pluralism and Democracy in the Netherlands*. Berkeley: University of California Press, 2nd edition.
- Posner, Daniel (2005). *Institutions and Ethnic Politics in Africa*. Cambridge: Cambridge University Press.
- Putnam, Robert (1993). *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton: Princeton University Press.
- Riker, William. *Federalism: Origin, Operation, Significance*. Boston: Little Brown 1964.
- Varshney, Ashutosh (2002). *Ethnic Conflict and Civic Life: Hindus and Muslims in India*. New Haven: Yale University Press.

Wilkinson, Steven (2004). *Votes and Violence: Electoral Competition and Ethnic Riots in India*. Cambridge: Cambridge University Press.