The Future in Comparative Politics

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THE PRESENT CHALLENGE

If not in journals, then certainly in the corridors, those engaged in the comparative study of politics express a sense of crisis. Central to our concerns is an apparent loss of diversity.

The phrase "globalization" best captures the sense of the crisis. As used by students of culture, the term refers to the spread of consumer tastes - be it for McDonald hamburgers, for Levi jeans, or for Coca Cola - and of powerful production technologies, in particular electronic forms of mass communication. The homogenization of preferences and technologies, commentators contend, results in a convergence of life styles, threatening the extinction of distinctive languages, values, and communal identities.

In politics, the term globalization tends refers to the spread of democracy and the market economy. Especially since the collapse of communism, democracy appears to have triumphed world-wide, with chief executives being chosen by ballot and having to share power with representative assemblies and with political parties having to compete for power by campaigning for majorities at the polls. Since the fall of communism, central planning has given way to the market economy; command and control has given way to market competition; and public ownership of the means of production has given way to private ownership. Political economies throughout the globe have come more closely to resemble each other.

As a result of globalization, those of us in comparative politics who were motivated by the global clash of rival ideologies sense a loss of vocation. The debates between radicals and conservatives and Marxists and liberals long animated our scholarship; the collapse of these polarities has disoriented us and enervated the field. Others, formerly impelled by scientific curiosity, fear that less variation has been left to explain. The differences that persist appear more the subject of engineering than of science, or of public administration than of political science, involving the implementation of economic "best practices" by bureaucrats or the organization of political procedures by legislative assemblies or electoral commissions.

A second response is fear. Formerly political systems were so different that specialization represented an unambiguously best response. Now barriers to entry have declined: political practices in one area resemble those in another, and one needs not be, say, a Zambian specialist to analyze the impact of electoral rules or party strategies during Zambia's recent elections. The origins of the fear do not derive just from the leveling of the scholarly playing field, however; they also arise from a recognition of competitive disadvantage. Those who possess a training appropriate to the study of advanced market economies now confidently extend their range of inquiry to include Shanghai, Warsaw, and Moscow. Those who possess skills in electoral research can now contribute to the study of mass behavior in Russia and those who practice game theory can study cabinet formation in Argentina. As market economies and democratic institutions spread throughout the globe, the technologies that have been perfected for research into the advanced industrial democracies can be applied everywhere. Those of us in comparative politics fear a loss of value in our profession.

Thus the nature of the challenges: apparent homogenization, loss of variation, and loss of a scholarly advantage. How should we best respond?

REDEFINING THE PROBLEM

Comparative politics is distinctive as a subfield in political science in that it is defined in terms of its method. Recognizing that methodology lies close to the core of the field encourages us to re-define the challenge: to see it not as a threat but rather as an opportunity. Insofar as globalization promotes homogenization, it may in fact enhance the prospects for comparative political research.

We make comparisons in large part because we cannot conduct experiments. Lacking control over "causal" variables, we instead attempt to control the selection of cases, matching them so as to capture the impact of explanatory variables while controlling for the impact of others that may be of less theoretical interest. In order to employ case selection as a means of causal inference, however, we need to make a key assumption: that the cases are, in fact, homogeneous, in the sense that the expected value of the dependent variable will be the same for each case, when the explanatory variable takes on a particular value. Viewed in this light, the growing homogeneity of the political systems about the globe can be seen as facilitating comparative inquiry and therefore strengthening, rather than weakening, our field.

To illustrate, consider the feasibility of conducting a study of, say, environmental politics in the formerly socialist countries of Eastern Europe.[1] One hypothesis might be that differences in the desire to market products in the economies of the West might produce differences in firm's responses to pressures from environmentalists. To test this hypothesis, a student might select a series of firms from a single industry, but from nations with different costs of marketing to the European Community. She might select a sample of chemical plants from nations located adjacent or distant from the common market, for example, and a control case from within Western Europe. It is highly useful to that student that the countries now possess relatively similar political systems; the similarity will enhance her capacity to attribute the choice of "green" technologies to economic factors.

Recognizing that increased homogeneity offers methodological advantages encourages us to look beyond the use of such controlled comparisons and small-N case studies and to the greater use of statistical methods. When we do so we realize that we stand at the threshold of important new research.

POLITICAL ECONOMY

One reason for this belief is the existence of a stock of high quality economic data for nearly all of the world's nations, some in time series that extend back to the 1950s. Many of these data have been generated as a by-product of the reporting requirements imposed by the United Nations and its agencies; some have been generated by the international financial institutions, such as the World Bank and the International Monetary Fund. Researchers at Wharton, the NBER, and international financial institutions have invested time, money, and skill in upgrading the reliability and validity of these data.[2] Because of the existence of these data, a bit of political information collected now is vastly more valuable that same bit if collected in the 1950s, when the economic data were scarce and of lower quality. One implication is that we should be thoughtfully and systematically collecting comparable sets of political data; another is that we may wish to revisit previous efforts at cross-national data collection, to thoughtfully re-appraise those data sets and to ponder the value of up-dating them.

Given that much of these data are economic, research efforts devoted to them should primarily impact upon the field of political economy. It is not surprising that it is *political economists* such as Alesina, Rodrik, and Londregan who have most productively responded to the opportunities they offer.[3] More surprising is that we in political science have been so slow to follow the lead of such scholars. Economists engaged in cross-national research often rely on uninformative measures of politics, such as the Gastil index. If political scientists do in fact possess a relative advantage in the analysis of politics, then they surely could provide better measures. But with notable exceptions — e.g. Przeworski and Limongi (Przeworski and Limongi 1997) — few of us have attempted to do so.

The collection and analysis of cross-national data will enable us to deepen our understanding of the *political* determinants of *economic* performance. North and Weingast (North and Weingast 1989) and Firmin-Sellers (Firmin-Sellers 1995) have advanced persuasive arguments about the impact of political institutions on capital markets. The economic data exist to test them; the political data do not. The collection and analysis of such data would also enable us to explain the economic determinants of politics. Bates and Lien, for example, offer a model of the impact of capital markets on parliamentary governments (Bates and Lien 1985); joined with existing economic data, the collection of the right kind of political data would enable a test of their arguments. These two instances offer but a small sample of the possibilities for political/economic research that such data provide.

DEVELOPMENTAL POLITICS

The impact would be as great in another field: the study of developmental politics. Even for the developing nations, many of these economic data now exist in times series that extend back thirty years or more. By collecting the relevant political measures, it should be possible to explore the dynamics of political systems. By way of illustration, consider an argument based upon the impact of institutions on growth and development. The dynamic element is capital formation; the strategic problem is time consistency, wherein period by period optimization yields a difference between preferences *ex-ante* and *ex-post*. Forming expectations rationally, investors may doubt a government's promise to refrain from expropriation, should the lender invest. The lender realizes that once she has committed resources to the investment, the borrower could then benefit by breaking his earlier promise not to expropriate them; *ex-ante*, the promises were valuable, but *ex-post*, the borrower does better abandoning them. Recognizing the force of these incentives, the lender will not lend, thus making both the lender and the borrower worse off.

The new institutionalists argue that the borrower, behaving rationally, will then search for means to constrain herself and to do so both visibly and credibly, so that the lender will realize that the borrower will keep her promise. Such arguments have been used to explain the capacity of "weak states" to gain greater access to capital, and at lower rates, than "absolutist" ones. They have also been employed to account for the preference of governments to borrow from politically organized consortia rather than from competitive capital markets. The arguments rely on backward induction; the reasoning is sequential. The arguments can therefore best be evaluated by analyzing data sets that contain repeated observations of a given set of cases, gathered over time.

Consider another example: the new growth theory, and its promise for the development sub-field. The models of growth contain state variables — such as stocks of capital, both human and physical and of infrastructure — and control variables, such as policy choices by governments. The models cry out for political amplification. If policy choices and government expenditures on schooling and infrastructure can be rendered endogenous, and if stocks of private capital adjust to the levels of political risk, then there exists the potential for a *political* theory of *economic* growth. And, even more important, there exists the potential for developing and testing such a theory in the laboratory, as it were of cross-national data sets.

Research into growth was rekindled in large part by the systematic accumulation of economic data in the post-war period. The possibility of analyzing the impact of the relations of production on the forces of production, or of political institutions upon the performance of the economy, underscores once again the importance of complementing these economic data with theoretically relevant political measures. This effort could help to liberate development studies from its bondage in comparative politics, enabling us to focus on the dynamic aspects of politics and the political significance of time.

QUALITATIVE RESEARCH

Once before, we had an outpouring of cross-national, statistical studies, much of which proved disappointing to students of comparative politics. The tradition was largely abandoned in favor of context rich, in depth, qualitative research. The two approaches have thus been treated as substitutes. They should have been treated as complements. The blending of the two research traditions stands, I feel, as a second major component of the research agenda made possible by the increased "homogenization" of politics.

To illustrate: The African Economic Research Consortium is launching a study of Africa's development experience over the first fifty years of independence, i.e. more or less over the fifty years following the end of the Second World War. The research will be undertaken by a thirty-person team of researchers, each member of which will address the economic and political histories of a single nation.[4] The problem each author will face is one each of us faces when embarking upon a case study:

while it is relatively straight forward to *describe* the experience of a given nation, what are we actually to learn from its analysis? One way of responding to this question is to determine what, if anything, is distinctive about the case. Did the nation grow more rapidly, or less, than other African nations? Was it politically more, or less, stable? A deeper question would be: did the nation grow more or less rapidly than could have been expected, given its characteristics: its initial income, its stock of human capital, or its natural resource endowments and so on? And was it politically more or less stable than could have been expected, given its social and economic make up and the nature of its political history? Responding to the first kind of question requires knowing the location of the particular case in the broader distribution of cases. Responding to the second requires a model of the processes that generate that distribution and therefore of the relationship between the properties of nations and their political and economic behavior. Locating the outcomes of the case within those distributions highlights its distinctive properties; the nature of the puzzle that it poses; and what, therefore, we can learn from its study. Cross-national, large-N research therefore provides a frame within which to lodge the study of particular cases, and helps to highlight their most informative properties.

The argument thus far suggests that the two research methods can best be combined in a form of deviant case analysis. But say a case systematically conforms to the regression line; say that given its attributes, the nation unimaginatively and relentlessly adheres to expectations. Such an outcome would still not call into question the value of an in depth, qualitative investigation. For in the vast majority of cases, our models yield measures of statistical relationships, not of causal effects; even when inspired by structural models, many are in fact estimated in reduced form. To locate and observe the mechanisms that generate the relationships that we observe and measure, we need to trace out the causal path that links, say, income distribution to political stability or ethnic diversity to violence. We need to move from the analysis of association to causation. We need to locate, describe, and understand the mechanisms that link right hand side variables to their effects. We therefore must combine qualitative investigations with large-N research. Our predecessors abandoned the one for the other; in the future, I hope, we will do what we now can do: that is, to treat the methodologies as complements rather than substitutes.

In the blending of the two research traditions, an important contribution could be made by someone who is willing to arbitrate between the two kinds of scholarship and to act as a translator of their language and preconceptions. This contribution could most effectively be made by someone with sufficient proficiency in statistics that she need not make a fetish out of mathematics and sufficient intellectual curiosity that she is willing to pay close attention to the challenges posed by those immersed in the tradition of qualitative research. I illustrate by referring, once again, to Africa, where researchers encounter numerous opportunities to investigate informative comparisons and therefore encounter as well the difficulties of drawing meaningful inferences from them.

In the 1980s, for example, scholars attentively traced the performances of Tanzania and Kenya in East Africa and Ghana and the Ivory Coast in West. Each pair pit the "conservative," i.e. market-oriented development strategy of one nation against the " radical," i.e. socialist strategy of another. In response to growing evidence of the advantages of market-based policies, the advocates of the socialist alternative challenged our ability to extract lessons from such comparisons. In East Africa, some rejected the very possibility of meaningful comparisons between Tanzania and Kenya, stressing their contrasting colonial histories. Others pointed to factors other than policy choices that would account for the growing disparity between them: differentials in foreign assistance or the uneven impact of drought or price movements. In West Africa, others, such as Samir Amin, rejected the value of focusing on the *contemporary* "horse race" between the Ivory Coast and Ghana, arguing that the more informative comparison would be between Ghana in the 1940s and the Ivory Coast today (Amin 1974). Ghana, he argued, projects the future of the Ivory Coast; so dependent are both on external forces that the only difference between the two lies in their time of entry into global markets.

Each of these objections points to potential sources of error in drawing inferences from data. Each of these arguments finds its parallel in debates among methodologists concerning the proper specification of statistical models. Building upon this recognition represents an important next step, I feel, in bridging the gap between the qualitative and quantitative research traditions.

FROM METHOD TO THEORY

Thus far I have argued that comparative politics is a field that is based upon method; that the presumed homogenization of politics, globally, and the existence of large-scale collections of data, highlight the utility of returning to large-N, cross-national research; and that the sub-fields of political economy and development are best positioned to secure rapid advances from their redirection. I have also emphasized that we need to blend cross-national statistical research with in depth case studies, arguing that the two are not substitutes but rather complements, and should be pursued in tandem.

My emphasis, thus far, has been on the use of methods. But what of questions of theory? My response is that the question is misplaced, and in two important ways.

Historically, many of the debates in comparative politics have focused on the choice of theory: structural-functionalism versus Marxism, dependency theory versus modernization theory, systems versus conflict theory, and so on. For all their vigor, these debates have often proved unproductive. The reason, I feel, is that practitioners too often lost sight of the role of theory, which is to explain. Debates over theory should be based on assessments of the theory's capacity to account for outcomes. In this sense, questions of theory can not and should not be separated from questions of method.

A sign of the "over supply" of theory in comparative politics is that its productivity remains so low. One implication is that we should re-allocate our time and energy away from the generation of theory. Another is that we should make more productive use of the theories we do possess, which means attempting to use them to explain the world about us. The separation of the discussion of theory from the discussion of research methodology blinds researchers to a second basic truth: that in the actual process of research, the distinction blurs between deductive and inductive methods. A good theory, in practice, provides a powerful engine of empirical discovery.

ANALYTIC NARRATIVES

To illustrate, consider the situation confronted by those who wish or need to construct a detailed case study, but who also aspire to "do" social science. Progress toward both objectives can be made by substituting, in effect, theoretical constructs for independent observations. A tightly reasoned model, in which logical necessity reduces the number of independent parameters, can be fruitfully brought to bear upon complex data. And it can turn into an engine of discovery. The method is one outlined by Avner Greif, Margaret Levi, Jean-Laurent Rosenthal, Barry Weingast and myself in a book, entitled *Analytic Narratives,* recently published by Princeton University Press (Bates et al. 1998). In ways that many will recognize, it resembles the method of process tracing advocated by Alexander George (George and Bennett 1998).

Such an approach starts with a problem: Why, for example, in county i or in situation j did event or outcome Y occur? By soaking and poking, the analyst discerns the process that generates Y; alternatively, recognizing the parallels between the situation and classes of phenomena that are well understood — e.g. collective action problems; models of imperfect competition; models of entry deterrence and contestability, for example — the analyst can apply a model to the case that has already been developed elsewhere. A good model yields testable implications; and the better the model, the richer and more abundant the implications it yields. A very good model, then, is one that is highly susceptible to failure. That means that a good model is not only deductively rigorous and rich, but also empirically informative. *For it is precisely when a model fails that we acquire new insights.* When the model fails, we then recognize that there is something about the case that we do not understand. We can then return to the data to locate variables that have been omitted from the model and forces whose impact was not captured in the initial theory. We can use the failure to learn.

In *Analytic Narratives,* each of us applies this method to a particular case study. When our models fail, we then proceed to dig deeper in the facts of that case. Given the initial commitment to theory, the search for new information proceeds in an organized fashion. Attention is focused locally, i.e. on the model itself or upon its empirical penumbra. Does the model fail because it makes assumptions that do not accord with the situation to which it is being applied? Does it assume plurality voting in electoral systems which are actually PR? If its assumptions are not flawed, might its failure not then result from its omission of important variables? Does it treat as closed, for example, a polity which is in fact open, and therefore fail to appreciate the domestic importance of external actors - a mistake made by both Avner Greif and myself in our initial efforts? Compelled by the failure of the model, we thus return to the data and engage in a focused re-assessment of the empirical record.

Note that in bringing theory to bear on case materials, we do not "theorize" the case; we do not take theory as an end in itself. Rather, we use theory to explain and we use it methodically, i.e. in a way that enables testing. The approach enables us to use models to gain leverage over case materials and to render deduction an empirical method. It is a method that helps us to span the gap between case studies and scientific investigation.

There are, of course, many kinds of theory. I prefer rational choice theory, precisely because it proves so useful in the analysis of *qualitative* data. Rational choice models are actor-centric. In strategic form, game theoretic models offer narratives, depicting actions and responses, sequences and paths of play, and the unfolding of understandings and realizations over time. They focus on individuals who are making choices in situations in which they have control over some alternatives — their choice sets — but not over others; the others fall in the constraint set and capture the impact of structures. Game theoretic models also capture the fact of inter-dependence and thus the strategic nature of politics. They highlight the significance of sequence and thus the impact of history. They suggest — and indeed model — the significance of structures; choice and constraint represent the two blades of the scissors of explanation offered by rational-choice reasoning. When analyzed, such models (may) yield testable predictions. These predictions (may) take the form of statements about behavior and outcomes that will result in equilibrium.[5] Because of these characteristics, game theory offers, I believe, a framework for systematically investigating qualitative data.

The value of this form of modeling becomes most apparent, perhaps, at the next step in the analysis: at the point at which the model fits the data. Rather than constituting the point of resolution, this stage represents a point of deep uncertainty and crisis. For, at this point, what have we in fact achieved? Is it not the mere mathematization of insights that can best be expressed in words? Merely translating the account from a language that is accessible to one that, being argued in mathematics, is abstruse does not really constitute scientific progress. When the model cannot be rejected on the basis of the empirical materials, how, then, are we to know if it is true? At such a point, is it not simply a tautology?

Thus the crisis: these questions pose serious challenges to this form of reasoning, and stand at the core of the kinds of skepticism most articulately expressed, perhaps, by Green and Shapiro. What renders "analytic narratives" important and persuasive is that the method offers a possible rejoinder, and in so doing, provides a possible bridge spanning qualitative and quantitative research. For the models begin by being actor-centric: they represent actors making decisions in strategic settings. But once their equilibria have been established, they can then be transformed into statements about relationships between variables. By exploring the comparative statics of the equilibrium, we can derive testable propositions about the relationships between the endogenous and exogenous variables, capturing how perturbations in the exogenous variables should affect the value of the variables that are determined by the choices made in equilibrium.

At this point, the game theoretic model has shed its narrative structure. It is no longer a statement about actors and decisions, but rather a source of propositions about relationships between variables. These propositions flow from the logic of the model; they offer testable implications of it — statements that must be true, if the model itself is accurate. The propositions, moreover, are general propositions: they can, and should, be tested outside of the data that originally gave rise to them. In this way, then, game theoretic analyses offer the promise of moving from qualitative case studies and narratives, on the one hand, to large-N explorations of relationships between variables on the other.

It should be noted and stressed that the methodology of *Analytic Narratives* possesses a major point of vulnerability. When, as is often the case with game theoretic models, the models possess multiple equilibria, then their equilibrium conditions must be highly robust for the method to provide a bridge between qualitative and statistical investigations. For when there are multiple equilibria, and when the equilibrium conditions are not robust, then a change in the value of an exogenous variable yields a change in the equilibrium set. Robustness analysis offers one way out of this impasse. So too might a reformulation of expectations; rather than looking for continuous relationships between variables, we should determine the existence or non-existence of outcomes. The former lends itself to the estimation of parameters linking variables; the latter to the study of the occurrence of particular events.

SUBSTANCE

In that comparative politics is defined by its method, I have argued, it stands as a distinctive field in political science.

But I have also argued that methods are not ends in themselves. It is therefore time to turn to substantive issues: phenomena into which we seek insight or for which we seek explanations.

In doing so, we can best begin by returning to the theme of globalization. Virtually all political economies have been incorporated into the global economy and most are becoming increasingly so. The result is a vast rise in the potential for exploring the diverse terms of incorporation. Take, for example, the various terms in which farmers are subject to forces from the international market or workers or others. Already there is a large qualitative literature on these themes, but clearly, with the spread of openness, it is time to raise the ante and to begin to systematically to assess and evaluate the insights gained from these explorations.

I offer an example from my recent study of the global coffee market. As suggested in the map, coffee production girds the globe. Between 1962 and 1989, the International Coffee Organization (ICO) — an international agency formed by the coffee producing nations, restricted the export of coffee so as to raise its price. It did so by assigning a quota to each country, i, the sum of which generated a total quantity of exports, Q*, that, given international demand, would generate a target price, P* (see Table 1). When farmers saw the new (and higher) price, P*, they would naturally seek to export more coffee. As this response would threaten to lower international prices, the members of the ICO devised ways of reducing exports.

Table 1 I.C.O.'s Constraints

Choose	: P*
Calculate	: $P^* = f'(Q^*)$
Quotas	: $(q_1, q_2, \ldots\ldots, q_n)$

As suggested in Table 2, different nations employed different policies to achieve this end; and each method suggests a different political story. Where farmers are dominant, governments allowed them to respond to the raised international price by producing more coffee, but then purchased the surplus and removed it from the market. The result was then a transfer of resources from the tax payers to the coffee exporters. Where governments can resist the influence of export agriculture, they placed a tax wedge between local and international prices. The result, economically, was a reduction in incentives to export coffee; politically, it is a transfer of resources *from* farmers *to* the state. Alternatively, governments can over-value the currency. Once again, the result is to secure a weakening of incentives for coffee producers and adherence to the constraints imposed by the export quota. Politically, however, the story is different; it represents a triumph of importers, generally urban industries, that can now use the export earnings secured by coffee producers to import greater amounts of foreign products,

The general point is that we possess abundant opportunities to explore diverse forms of adjustment to international markets, and to the shocks, such as the setting of quotas by the ICO, that they generate. The diverse responses to the common impulses generated in such markets generate variation that can be employed to extract insights into politics.[6]

Table 2 Modes of Adjustment

Problem	$P^* > P^o$
Solution	Farmers See
Government Purchase surplus	P^*
Government Taxes Farmer	$(1 - t)P^*$
Farmers Tax Selves	$(1 - t)P^*$
Overvalue Currency	$r P^*$

Even more stimulating is the potential offered by the spread of democratic forms of government. The study of elections and electoral systems; parties and party systems; legislatures and their relationship with the executive — each can now be carried out virtually world wide.[7] The diversity of these forms offers rich variations on the theme of democracy. It also offers the kind of variability that has been missing hitherto for students of democratic politics, something that has often rendered their research culture bound and parochial.[8]

There are of course numerous other issues to be pursued. Some deal with the breakdown of states; others with violence and new forms of warfare. We shall never want for challenging issues to pursue. But what is different now is the opportunity to pursue them by combining qualitative evidence and cross-national research designs.

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Notes:

[1] This portion draws on discussions with Ms. Liliana Botcheva, a doctoral student in the Department of Government, Harvard University.

[2] See (Bank 1998), (Barro and Lee 1993), (Summers and Heston 1988).

[3] See (Alesina and Rodrik 1994), (Rodrik 1991), (Londregan and Poole 1990).

[4] Or small group of nations.

[5] I emphasize "may" because analysis may suggest the non-existence of an equilibrium.

[6] This is of course the agenda of the so-called UCLA school of comparative politics. See Rogowski (Rogowski 1989) and Frieden (Frieden 1991).

[7] And this has been pioneered by the so-called UCSD school. See, for example, (Cowhey and McCubbins 1995).

[8] As has been characteristic of the study of American politics.