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Letter from the President

Theory in Comparative Politics?

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Comparative politics is a field in search of theory. It should be in search of a framework or orientation.

In my first job, I was the sole comparativist in a small faculty, all of whose members focused on the United States. In striking contrast with my field, I discovered, theirs possessed a consensus as to what constituted meaningful research or an important research finding. The reason was simple: they had democratic theory, while we did not.

Given their shared vision of politics, the Americanists could engage in normal science. It became meaningful, for example, to seek more precise measurement of, say, incumbency advantage: more decimal places and reduced errors obviously mattered. For everyone would recognize the implications of the results for the responsiveness of political leaders to those they governed.

The Americanists' shared orientation toward politics – their commitment to the democratic paradigm – also informed their response to theory and method. The importance of elections made investment in research into public opinion a "no brainer," as our youngsters might say; survey research offered an obvious means to mount scientific investigations into the behavior of citizens in democracies. Americanists more rapidly recognized the significance of formal theory as well; they were better positioned than the rest of us to appreciate the political significance of seemingly abstruse theorems regarding cyclicity in social choice and the possibility of equilibria under majority rule.

In comparative politics, we have lacked a similar intellectual framework. Indeed, to us, the Americanists, with their shared vision, appeared provincial: they acted as if democratic politics were the only form of politics. For our part, our efforts to compensate for a lack of similar structure made us at times appear ridiculous. Misperceiving the foundations for the Americanists' ability to build a cumulative research tradition, we sought to build ours on thin air. Thus our wholesale conversion to "systems theory," "structural-functionalism," the study of "the state," and – dare I say it – rational choice. But theory and method must be brought to bear on questions, issues, and problems; the research tradition becomes a cumulative research

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tradition when those questions, issues, and problems are closely related, because they have been chosen within a coherent and encompassing framework.

In the middle income countries of the world, democratic theory may provide that framework; it may fulfill a role in comparative politics similar to the role it played in the study of American politics. There are indeed rapidly mounting signs of it doing so, with the comparative study of electoral systems taking on new life in the field and survey research being employed in new and powerful ways in formerly communist societies.

Viewing development as the political economy of growth may provide criteria of relevance that transform research into the developing areas into a cumulative research program. The accumulation of capital, the process of investment, the search for the political foundations for economic growth: investigated within the structure offered by growth theory – classical, Marxian, neo-classical, or modern – these themes yield research programs that offer mutual commentary and criticism. Within such a framework, "normal science" could grow.

Social theory offers a third intellectual framework. Social theory highlights the political significance of culture and the producers of culture: artists, priests, and intellectuals. It also highlights the role of rhetoric and symbolism. It provides a framework in which we can see the intentionality that underlies the construction of values, and the political purposes to which culture is put. Within this framework, we should be able to address not just the privileging of voices in the West but also contemporary appeals to religion, ethnicity, and identity throughout the world. While social theory may provide the framework, it will no doubt be others who provide the theory and methodology. Social theory is relatively barren when it comes to showing how its claims can be systematically evaluated. Indeed, it may well be the practitioners of forms of science that social theorists themselves reject who show how and why the insights of the social theorists are correct. Game theorists, for example, will surely be un-welcome bed fellows; but it is they who have begun to formulate testable models of the strategic emission of meaningful gestures.

Students of comparative politics are given to fads; we often seek from methodology answers that it cannot give. We are too often given to scientific posturing. We seek to become scientific by acting like scientists, rather than by using the tools of science to pursue intellectual issues. Methodology cannot provide guidance to interesting and important questions. Nor can it provide a framework supportive of debate, cumulation, or self-correction. For guidance in these matters, we must turn to intellectual traditions. I have pointed to three. The contributors to this issue will point to others.

The *Newsletter* is published twice yearly.

Deadline for Winter issue: December 15
Deadline for Summer issue: June 15

News & Notes

The Nominating Committee, consisting of Valerie Bunce (Chair), Barry Ames, and Jeff Herbst, chose Susan Shirk, of the University of California at San Diego, and Ronald Herring, of Cornell University, as new members of the Executive Committee.

In consultation with the Executive Committee, Robert Bates appointed a new nominating committee, consisting of Karen Remmer of the University of New Mexico (Chair), Geoffrey Garrett (Stanford and the University of Pennsylvania), Torben Iversen (Harvard), Susan Stokes (Chicago), and Kiren Chaudhry (Berkeley). The committee will nominate two new members of the Executive Committee and select the President to succeed David Collier.

The Luebbert Award committee was this year chaired by James Alt and included Ruth Collier and Barry Weingast. The recipients of the award for the best book were Stephan Haggard and Robert Kaufman for *The Political Economy of Democratic Transitions* (Princeton: Princeton University Press, 1995).

The runners up for the book award were Barbara Geddes for *Politician's Dilemma: Building State Capacity in Latin America* (Berkeley: University of California Press, 1994) and Sidney Tarrow, for *Power in Movement: Social Movements, Collective Action, and Politics* (New York: Cambridge University Press, 1994).

(News & Notes continues on page 17.)

In response to spirited discussions at the Business Meeting, the President of the Section has sent the following letter to Elinor Ostrom, President of the American Political Science Association:

Dear President Ostrom:

As you are aware, the allocation of panels remains a contentious issue in the Association. The governing criterion has long been attendance at previous panels offered by the subfield.

Recent records indicate that membership in the Comparative Politics Section is increasing, and that the subfield remains the largest within the Association. As a consequence, we feel highly constrained by the Program, in which we are allocated many fewer panels than our membership can productively fill. Indeed, we are able to fulfill but 8% of our requests to offer panels.

The result is that, in our field, our convention, that is supposed to be inclusive, is in fact more exclusive than our professional journal.

This argument was vigorously advanced by the membership in the Section's business committee, who urged that the problem be brought to the attention of the Association. I would be most grateful if you could take it up with the Program Committee, perhaps in consultation with Karen Remmer, who served as the Program Chair for the Section at this year's meeting.

Robert H. Bates

The *Newsletter* will be available on the Internet for viewing and downloading.

Beginning March 1, visit us at http://shelley.sscnet.ucla.edu/apsacp/index.html.

Send technical inquiries to the Assistant Editor.

Letter from the Editor

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It has been five years since the *APSA-CP Newsletter's* editor has communicated directly and formally with readers, except when my predecessor, Ron Rogowski, did so three years ago in his dual capacity of Section President and *Newsletter* Editor. Given recent changes in policy and practices, now seemed a timely moment to do so again.

To recount for our newer members: this *Newsletter* was founded, initially as an annual publication, with the establishment of the Organized Section in Comparative Politics in 1990. The *Newsletter* moved from the University of Washington to UCLA in 1993, at the same time becoming a semiannual publication. Funded in part by members' dues, production of the *Newsletter* depends crucially on a local subsidy. We are currently in the first of a three-year commitment of funding on the part of UCLA's College of Letters and Science, for which we are highly grateful. For the prior three years, we received funds from UCLA's International and Overseas Programs, which we also gratefully acknowledge. UCLA's funding covers the costs of hiring an Assistant Editor to produce the *Newsletter*; the portion of Section dues that reverts to the Section pays for printing and mailing.

We currently have 1,550 members, making the Organized Section in Comparative Politics twice as large as the APSA's second largest organized section. Not only are we an extremely large Section, we are intellectually highly diverse. With the exception of the *American Political Science Review*, this *Newsletter* is perhaps the most widely read publication among students of comparative politics in the English-speaking world. As the practice of comparative politics becomes increasingly fragmented and intellectually balkanized, the need for a central forum for debate has become that much greater. The main goal of the *APSA-CP* is to contribute to the intellectual cohesiveness of the comparative field. By this I don't mean that we expect to impose an artificial degree of intellectual uniformity on our disparate readers, but only that the *Newsletter* aims to serve as a site for debate of the central issues animating our corner of the discipline. At the same time, we also seek to inform our readers about trends and developments in scholarship, to provide a place for leading figures in the comparative field to speak and to engage issues (as well as each other), and to alert our readers to scholarly resources of which they otherwise might remain unaware.

Under the earlier editorial leadership of my colleague, Ron Rogowski, policy was adopted to focus each issue of the *Newsletter* on a single central topic. Ron's goal was to make the *Newsletter* a place for substantive debate, not merely a site for professional announcements. I have followed his lead. My own aim has become that of focusing each issue on some very general, perhaps even meta-theoretical, topic. There are many places for us to present the results of our research, but far fewer where we can step back and consider how our own research speaks to the comparative field as a whole, how it intersects with other kinds of research or other approaches within the comparative field, and the trends and developments characterizing our subfield more generally. The *Newsletter* aims to be the place where we can reflect on the state of comparative politics. Of course, there's no substitute for the real thing, and in my own view, doing comparative politics is in the end more interesting and certainly more challenging than thinking and talking about what we do, but the occasional self-conscious foray onto meta-theoretical territory is nonetheless a useful detour. In particular, I hope that some self-reflection on the part of practitioners is useful to younger scholars who may still be looking to define their place in the field and the discipline. Suggestions for theme topics for future issues are always welcome.

Let me now turn to matters of practical concern to readers, and provide some information regarding policies and procedures. First, the APSA maintains our membership lists and we receive our mailing labels directly from the Association for each issue. If you move, your change of address will be handled through the Association as part of your more general change of address as a member. There is thus no reason to inform the *Newsletter* directly of any change of address. Similarly, when you join the Section, there may be some lag in getting you on the rolls, but again, you should communicate any problems directly to the APSA. At the APSA, Sheilah Mann (smann@apsa.com) and Jun Yin (jyin@apsa.com) are the persons responsible for Organized Sections.

Second, in an effort to impose a consistency of tone to the *Newsletter*, I have enacted a five-footnote policy. Contributors, in other words, are limited to no more than five footnotes in any article. The *Newsletter* is a professional but not a scholarly publication. We report on research trends but we do not report a great deal of original research itself. Hence, it seemed appropriate to limit footnotes. In instances where the limitation is unrealistic, we will provide information allowing readers to communicate directly with the author in order to obtain references. Underlying this policy, of course, have been serious space limitations.

Third, we have had serious space limitations, which, as the current issue testifies, have now been substantially relaxed. Until now, with Section dues as they are, printing and mailing costs allowed us to publish issues generally of 16 and occasionally of 20 pages. Our new Assistant Editor, David Yamanishi, has used his background in the graphics industry to achieve a major reduction in production costs. Not only is this issue of the *Newsletter* offset rather than photocopied, but we have consolidated printing, labelling and shipping from one location rather than dividing the work between our local copy shop and the UCLA mail room. This has two potential benefits. First, we hope it will decrease the technical errors in production that we have encountered (frequent misprints, extremely slow mailing) and that have justifiably so annoyed readers. Second, it has so dramatically reduced our costs that we have been able to double the size of the *Newsletter*, increasing it from 16 to 32 pages. The only potentially negative side-effect is that we are now printing on lower weight paper for foreign subscribers, in order to keep the extremely high costs of foreign postage under control. If this results in damaged *Newsletter*s arriving overseas, we definitely want to know.

Our new enlarged format should transform the *Newsletter* into an even more serious and substantive forum for debate. With 32 pages per issue, we can be much more flexible in the page limitations we establish for individual contributors. In addition, we can substantially increase the range we cover in various debates, requesting many more persons to contribute. Otherwise, however, while we welcome brief announcements of matters of interest to our readers, I cannot promise that we will have adequate space to print them. I consider the thematic articles and the book reviews more important, and we fit announcements in only as we can. We are especially unlikely to have room for announcements that contain already well-publicized information.

Fourth, we welcome unsolicited research reports on the construction of databases or archives accessible and useful to others. If you are assembling a data set that will shortly become publicly available, a summary description would be welcome. Research and data reports may be submitted at any time directly to the Editor or Assistant Editor, preferably by email, using whatever electronic format to which you are accustomed. If we have to postpone publication because of reasons of space, we will let you know.

Fifth, our policy for book reviews has evolved over time, and is now entering a new phase. Our book reviews are written by graduate students, who select the books to review themselves, according to their own interests. The great advantage of this policy is that students pick books that they find especially helpful or inspirational in their research. When the *Newsletter* was first moved to UCLA, largely as a matter of convenience we solicited reviews only from our own graduate students. Gradually, we began soliciting them from students working with officers of the Organized Section teaching at other institutions, so now our book reviews are drawn from universities across the country (although a disproportionate number probably still come from UCLA graduate students). We are now a stable enough body that I have decided to open the book review section to graduate students generally. Please encourage your students to submit proposals. In the first instance, this should consist simply of an inquiry regarding a particular title, which should be sent to myself and the Assistant Editor (preferably by email). We will let the student know whether someone is already reviewing the book proposed, whether we think we will have space in the next issue and when to get the review in. When the review arrives (again, we urge electronic submission), we may require the author to edit it, either for length or for other considerations. (We will handle minor copy-editing ourselves.) In extreme cases, we may reject a review which is somehow wildly inappropriate, but I certainly hope this will occur only in extraordinary circumstances. We suggest that book reviews run 1,000 to 1,500 words in length.

Increasingly, I receive books from presses with requests to review them in the *Newsletter*. Since students select their own books to review, there is no mechanism for handling unsolicited review materials. If you publish a book, you would be better advised to look for a graduate student to review it (I am sure it goes without saying that the student should be at an institution other than your own!) than to have a copy sent to me for review purposes. Likewise, a student who wishes to review a book should plan on procuring his or her own copy, since we lack the staff resources to solicit copies of books from publishers.

Finally, I encourage you to use whatever portions of the *Newsletter* are relevant in the classroom. The APSA has recently agreed to allow reproduction of articles in the *Newsletters* produced by Organized Sections for pedagogical use without requiring any fee. (The same is not the case for multiples greater than 10 of articles taken from either the *American Political Science Review* or *Political Science & Politics*.) Your graduate students are our future members. We hope you will share your *Newsletter* with them, and help professionalize them by encouraging them to submit book review proposals. And for our colleagues at institutions which do not grant graduate degrees, we hope the *Newsletter* will help keep you abreast of debates and publications in the field.

A last word to our out-going Assistant Editor, Terri Givens, who has done an outstanding job of laying out the *Newsletter*, getting it reproduced and mailed, and generally staying on top of the administrative details over the past few years. Under Terri's care, the layout of the *Newsletter* was improved, making it easier and more pleasant to read. She has also carried a huge administrative burden invisible to the reader but extremely visible to the Editor. No faculty member could take on a *Newsletter* like this without a large amount of assistance, and Terri has done a superb job. While I am sorry to see her go, she will have more time to devote to her own research without the responsibilities of the *Newsletter*. And I am pleased to welcome our new Assistant Editor, David Yamanishi, in her place.

Notes from the Annual Meetings

The article below is the first in what we hope will be a continuing series linking the Newsletter *and Section panels at the APSA meetings in a more explicit fashion. Weingast's reflections are drawn from the paper of the same title presented on a panel on "Formal Theory and Comparative Politics" at the 1996 APSA meeting.*

Formal Theory and Comparative Politics

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A remarkable event occurred two summers ago when Tim Fedderson and Roger Myerson ran a conference at Northwestern University on the formal theory of political institutions. This was a large conference, involving over seventy people. Although the official emphasis of the conference was on institutions, a large portion of the participants spoke about their work in comparative politics. Each participant had previously understood that his or her work included a comparative dimension. What few had understood was that nearly all of us were working in comparative politics. In short, comparative politics has become the program among formal theorists.

This seems to suggest that formal theory has a bright future in comparative politics. Other signs point in the same direction. Formal theory has long had a presence in comparative politics, and many comparative scholars have come to draw on elements of rational choice. Importantly, within every area and topic of comparative politics, I have found comparativists willing to engage in a dialogue with formal theorists.

Nonetheless, formal theory's future in comparative remains uncertain. All is not sweetness and light. At the abstract level, the reigning way of understanding the interaction of formal theory and traditional approaches is captured by two metaphors, Gabriel Almond's separate tables and Thomas Kuhn's competing paradigms. These metaphors suggest that formal theory and traditional methods are incompatible and competing approaches. They point scholars toward confrontation and the need to choose sides.

Extremists of both stripes reinforce the view suggested by these metaphors. Some rational choice proponents seem to believe that "we're science, you are not, and that we do it you don't." And many traditional scholars appear to reject the possibility of a social science of comparative politics. These extreme, "get lost" attitudes are hardly designed to elicit sympathetic cooperation.

A New Metaphor

I believe that the metaphors of competing paradigms and separate tables are not useful ways to think about the interaction of formal theory and comparative politics. I propose instead another metaphor, that formal and traditional approaches are complementary rather than competing paradigms. Each approach has what economists call a "comparative advantage," something it does well relative to the other. Each has something to bring to the same table, thus providing the basis for cooperation rather than competition and controversy.

A potentially useful model for cooperation between formal and traditional approaches reflects the way in which cooperation emerged between formal theorists and those studying the political culture and behavior in Congress in the 1970's and 80's. Comparativists are likely to be skeptical that anything from American politics might be useful for comparative politics. Nonetheless, as formal approaches emerged in American politics, the seeds of the same conflict were present. Traditional scholars dominated the study of Congress. Formal methods were greeted with considerable skepticism, in part because they abstracted from the details that proved central to the traditionalists' approach.

Traditional scholars, emphasizing political behavior and culture, focused on careful observation of congressional practice, such as congressional norms. They provided detailed descriptions of the norms as embedded in particular contexts and reflecting particular meanings for individuals. Formal theorists tended to take these findings as given, seeking to provide explanations for them; for example, by providing a model of why a particular norm could emerge as an equilibrium in a game among legislators.

In this way, the two approaches interacted positively; each provided insights for the other. Both studied the same subject, but from different perspectives. And scholars from both camps came to learn from and enjoy each other's work. Thus, the two approaches proved not to be competing in Kuhn's sense, but complementary ones emphasizing different aspects of the same subject.

The same type of complementarity appears possible in comparative politics. Consider, for example, the burgeoning literature on ethnic politics. One obvious lesson from the literature is the incredible variety of ways in which ethnic conflict arises. Formal theory is unlikely to predict the specific circumstances of particular conflicts; traditional approaches have a comparative advantage in the study of particular ethnic conflicts.

Formal theory's potential contribution resides in the ability to answer questions to which traditional methods are less suited. For example, how do we explain the common pattern across different ethnic conflicts of long periods of peace, punctuated by periods of intense ethnic violence? The promise of formal accounts of ethnic conflict is not in explaining details and meanings, but in understanding general mechanisms that apply to contexts across time and space.

The reaction to Jim Fearon's paper, "Commitment Problems and the Spread of Inter-Ethnic Conflict," illustrates the potential cooperation and open-mindedness among both kinds of scholars. Although this paper has not become "the way" to understand ethnic conflict, traditional scholars have been willing to take the insights of this approach seriously in a range of contexts. And Fearon could not have produced his idea without first embedding himself in the rich, traditional literature.

Cooperation between formal theorists and traditional comparativists is not inevitable, however. A central limit to the acceptance of formal approaches in comparative is found in formal theory's traditional focus on the institutions of representative government in developed societies. By emphasizing legislatures, elections, and voting typically studied in stable democracies with stable institutions formal theory's traditional focus ignores a vast set of important political phenomena associated with less developed societies. To mention a few: cycles of authoritarianism and democracy; problems of failed democracy; absence of the rule of law, stable property rights, and political rights; dramatic instances of discontinuous political change, as reflected by coups and revolutions; and finally problems of ethnic strife.

To many formal theorists, phenomena of this sort represent the attraction of comparative politics: a vast frontier of phenomena to be modeled. Of course, there have long been rational choice efforts in these areas (such as Bates and Popkin). In the last few years, formal theorists have begun to study a much broader range of comparative questions. Yet it is fair to say that formal theory's promise to provide a systematic approach to a range of comparative phenomena remains unproven. Formal theory's traditional emphasis leads many comparativists to be skeptical about whether techniques developed to study highly institutionalized societies will yield equally high payoffs for phenomena central to less developed societies. Until formal theory begins to provide serious answers to phenomena outside the developed societies, rational skepticism will remain. Put another way, the proof is in the pudding, and formal theory has only begun to provide it.

Strengths and Weakness of Formal Theory

I end this article by raising another concern about formal theory's role in comparative politics. Consider two of the impressive programs of formal research in comparative politics, Adam Przeworski's study of democratic stability and Michael Laver and Kenneth Shepsle's study of government formation in the European democracies.

These programs are emblematic of recent formal work in comparative politics. Both programs are rightly regarded as showcase applications of cross- national, formal methods. Both combine new theory with systematic empirical analysis. The texture of phenomena informed these scholars' theory building. Finally, the striking empirical power of Przeworski's and Laver and Shepsle's models demonstrates the value and insight of formal theory.

Inevitably, these programs' success invites the question: will abstract methods become the way to study comparative politics, pushing aside other approaches? I raise this question because I believe that the success of formal methods in comparative depends on how we answer it. Here, too, the literature on Congress provides some hints. Just as traditional congressional scholars care to learn about formal methods that debate, for example, whether members of Congress maximize votes or the probability of reelection, so too did many formal theorists start to immerse themselves in the close observation of Congress. The same pattern of complementarity and cross-fertilization is possible in comparative. The analytical power of formal methods will be greater if they become integrated with traditional approaches.

By way of summary, I have argued that formal methods and traditional approaches should be viewed as complementary approaches to the same subject rather than competing ones. They each have their place in comparative politics. Nonetheless, the future of formal theory remains uncertain. I have suggested that formal theory's long term place in comparative politics depends on two factors: first, how and whether it moves beyond the institutions of representative democracy to provide new insights into the problems of less developed societies; second, the degree to which formal theory becomes integrated with existing comparative approaches.

I'm optimistic about formal theory's future. I believe that formal theory will provide novel and enlightening approaches to most problems in comparative politics, that its theoretical perspective will help integrate a series of disparate sub-literatures, and finally that it will suggest a major reorganization of the field. Formal theory, as I've argued, holds the potential of fitting alongside traditional approaches. Whether it does so or whether it continues to be seen as alien by many comparativists, I cannot predict. I can predict, however, that confrontation rather than cooperation is more likely if most comparativists and formal theorists continue to treat their approaches as separate tables and competing paradigms.

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Book Previews

The focus of the current issue is on four forthcoming books which we believe will often be used in introductory graduate seminars in comparative politics. Two – those by Geddes and King – are methodological in orientation, one – the volume edited by Lichbach and Zuckerman – tries to give a sense of the scope and types of work currently done within the field, and one – by Przeworski et al. – considers a fundamental substantive problem within comparative politics. We intend these book previews to alert readers to some of the most exciting, innovative, and important work

Comparative Politics: Rationality, Culture and Structure

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Comparative Politics: Rationality, Culture, and Structure

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Theory advances through explicit discussions of strengths and weaknesses, detailing areas of success and failure that establish the ability of analyses to advance knowledge in a discipline of knowledge. Theory is a collective enterprise. While scholarship is the work of single scholars, knowledge accumulates as groups of scholars accept and reject claims about the world. The extraordinary range and importance of the topics examined in comparative politics, the powerful and competing research schools, and the scholarly pedigree invite periodic assessments. In the early 1960's, Harry Eckstein and David Apter edited a collection of essays that established the field's questions. In 1970, Robert Holt and John Turner gathered together a set of contributions that raised the level of theoretical sophistication. Both volumes guided research in comparative politics and influenced the contests among the research schools. As the authors of the essays in this volume examine the research schools in comparative politics, they assess knowledge and advance theory, seeking to direct research in the coming years.

We have organized this volume around the themes of theory and research schools in comparative politics. Because so much analysis in comparative politics is guided by the expectations, assumptions, methods, and principles of rational choice theory, culturalist analyses, and structuralist approaches, assessments of the state of theory and prospects for advancing theory need to focus on these research schools. What explains the imperialist expansion of these schools and the disappearance of other approaches? As Lichbach's and Zuckerman's essays in this volume demonstrate, these schools share an ontological and epistemological symmetry. They offer – indeed force – choices along the same dimensions. Furthermore, at a more fundamental level, the themes of the research schools rest at the heart of the human sciences. Reason, rules, and relations are unique to social theory. Focusing on these themes sets research in the social sciences apart from the physical sciences, providing a fundamental basis on which to theorize about political phenomena. Rationalist, culturalist, and structuralist theories are embedded in strong research communities, scholarly traditions, and analytical languages. As they dominate comparative politics, they provide the locus for assessments of theory in this area of knowledge.

Because research in comparative politics centers around distinctive topics, we have selected four themes to examine the interplay between theory and the three schools: the analysis of mass politics, especially regarding electoral behavior, and social movements and revolutions; political economy; and state-society relations. Why did we choose these topics? Taken together, they encompass much of the research done in comparative politics. Each displays a history of sophisticated theoretical and empirical work that stretches over several decades. The comparative study of voting behavior begins in the inter-war years. Because most people who engage in political activities do so only at the ballot box, this research examines the political behavior of the largest set of people; here the study of politics moves its focus away from politicians and bureaucrats, government agencies and political parties; and the abstractions of state and society. The systematic analysis of social movements and revolutions descends directly from Marx and Weber. It also links to studies of regime transformations and the bases of stable democracies. Beginning with Keynes' theories, the analysis of the political economies of advanced industrial societies has become the focus of the largest part of research on the political institutions and public policies of established democracies. As comparativists study state-society relations that follow a path first marked by Marx, Weber, Mosca, Michels, and Pareto. As they study the formation of states, they blend abstract theorization and detailed empirical studies. In addition, each displays other vital characteristics. Examining the successes of the research schools with regard to each of these topics also casts light on the utility of various analytic techniques: electoral analyses typically use quantitative techniques to study survey results and work on state-society relations includes the results of qualitative studies, while both research on social movements and revolutions and political economy vary in the use of quantitative and qualitative modes of analysis. Finally, these research themes also elucidate the relations between theories developed in comparative politics and those that characterize related fields in political science and the other social sciences, such as the utility of hypotheses devised to explain electoral behavior and social movements in the United States and methods and arguments drawn from economists in the study of political economy and from anthropologists for the analysis of state-society relations. As we analyze these research topics, we examine central issues of theory in comparative politics.

We have divided the essays into three units. The first, containing the chapters written by Margaret Levi, Ira Katznelson, and Marc Howard Ross, offers briefs for each of the research schools. The essays summarize each school's core principles, noting variations within the approach and presenting recent work that points to new combinations. The next unit contains the chapters written by Samuel H. Barnes on mass politics, Doug McAdam, Sidney Tarrow, and Charles Tilly on social movements and revolution, Peter Hall on the political economy of established democracies, and Joel S. Migdal on state-society relations in newly formed states. The concluding unit contains essays by Mark I. Lichbach and Alan S. Zuckerman, returning the focus to the theme of advancing theory in comparative politics.

A Model, A Method and a Map: Rational Choice in Comparative and Historical Analysis

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Empirical rational choice in comparative analysis is in its relative infancy. It is still in the process of making the transition from analytics to analytic narrative. Even so, it has become one of the leading paradigms in the field and produced some major and influential work on a range of subjects, places, and periods. Although the divide between rationalists and many of those in this volume may be shrinking as rationalists become more concerned with context and non-rationalists recognize collective action problems, voting cycles, and other insights from rational choice, the divide remains nonetheless. Structuralists and rationalists are in the same conversation but persist in very different views of the origins and affects of institutions and preferences. Many rationalists are taking on board the concerns of culturalists with providing a more complete account of preferences and strategies, but they continue to disagree on the uses of those accounts. What divides rationalists from culturalists and structuralists is not method in the sense of mathematics versus statistics, field work, observation, and archival research; there are many rationalists who rely on precisely these tools. What divides them is method in the sense of how to construct theory, organize research findings, and address the issues of falsifiability and plausibility.

Rational choice will continue to have its serious detractors in comparative politics. It simplifies the world and human psychology more than suits the tastes of many comparativists, especially those committed to area studies or interpretivist explanations. Its positivist ethic may be unpalatable to postmodernists and others. The very commitment of rationalists to scientific progress by means of fact-finding, testability, and partial universalism will remain repugnant to some critics and an impossible goal to others. Rationalists must continue to refine and clarify their models so as to increase their explanatory power, and they must find more satisfying means for arbitrating among competing accounts. These are the tasks incumbent on all comparative social scientists committed to explanation.

Structure and Configuration in Comparative Politics

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At just the moment *Comparative Politics* was founded in 1968 as a new journal devoted to the reorientation of the subdiscipline in a scientific, behavioral direction from its older, more country-by-country institutional qualities, another scholarly tendency – structural, macroanalytical, configurative, historical, and institutional – had begun to convene a research program that soon transformed the potential scope, ambition, and content of comparative politics. Turning the study of post-feudal modernity away from the realms of description and metaphysics, the treatments of immense historical change by scholars including Perry Anderson, Reinhard Bendix, Shmuel Eisenstadt, Samuel Huntington, Barrington Moore, Stein Rokkan, Theda Skocpol, Charles (with Louise and Richard) Tilly, and Immanuel Wallerstein, for all their differences, broadly came to share a common mode of inquiry combining ontological and methodological commitments geared to the specification of macro-foundations for human action.

These scholars developed a probabilistic approach to structure, wagering that the most significant processes shaping human identities, interests, and interactions are such large-scale features of modernity as capitalist development, market rationality, state-building, secularization, political and scientific revolution, and the acceleration of instruments for the communication and diffusion of ideas. 'Society' in this orientation is replaced by the structured concatenation of processes. These, while not determining of behavior in any strict sense, establish in specific times and places a calculus of cognitive and behavioral probabilities by creating situational orders in which individuals think, interact, and choose. Persons, in this view, are embedded agents operating within relational structural fields which distinguish the possible from the impossible, the likely from the less likely.

In the 1990's, neither the original project of *Comparative Politics* nor the structural microanalysis of the 1960's and 1970's are giving powerful direction to comparative political studies. Rather, as the 1995 *World Politics* symposium on "The Role of Theory in Comparative Politics" signified, much of the drive in the subfield now belongs to rational choice scholarship and to various postmodern currents, leading some of the participants, including Peter Evans and Theda Skocpol, to worry whether the traditional core of comparative politics risks being overwhelmed.

In truth, however, most current work still resides in the space between the poles of microeconomics and signification, but much, alas, is rudderless and lacking in self-confidence. My paper for the Lichbach-Zuckerman volume is geared to ask how the macroanalytical tradition might again come to play a defining role in comparative politics, less as an alternative to other currents than as a way of working capable of engaging and incorporating them confidently.

Put differently, I am searching for a program and way of working more ambitious than the trajectory of the lineage of work growing out of the grand macroanalytical scholarship of the 1960's and 1970's that has produced the new historical institutionalism of the 1970's and 1980's which mainly has focused on comparative interest representation, public policy, and political economy. Viewed against the work of their predecessors, such scholars as Peter Hall, Ellen Immergut, Sven Steinmo, and Paul Pierson who have produced superb studies have shortened their time horizons, contracted their regime questions, and narrowed the range of considered options. This smaller-scale historical institutionalism has proved stronger as a skeptical response to other disciplinary trends than as a sharply-etched project of the kind the earlier and more adventurous macroanalytical scholarship appeared to propel. The result has been something of a loss to the *élan* and potential of comparative politics, especially at the core of the enterprise. In part this contraction is the result of a certain lack of theoretical and methodological self-consciousness at a time when Marxism, which in fact did much of the intellectual work for macroanalysis, has been called into question by global events and by challenges to its essentialism, functionalism, and teleology.

In discussing how structural macroanalysis could reclaim its leadership position in comparative politics, the paper surveys the macroanalytical heyday, focusing primarily on Moore and Skocpol, and elaborates on five key issues central to the revival of macroanalysis: the qualities of comparison, the relationship of history to analytical social science; structural theory after Marxism; the special status of the state; and the question of behavioral and strategic microfoundations. This endeavor is sustained, in part, by fresh scrutiny of three pivotal texts: Alexis de Tocqueville's *Democracy in America*, John Stuart Mill's *A System of Logic* and Max Weber's *The Methodology of the Social Sciences*; and by a perspective on institutions linking their configuration and design to the formation and existence of political agents who possess particular clusters of preferences, identities, and interests. Some of the best work along these lines has been appearing in studies of the United States under the rubric of American Political Development (APD): a genre of work that has begun to recover the dimensions of invention and surprise that attracted many of us to political science and to comparative politics two and three decades ago.

Culture and Identity in Comparative Political Analysis

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Two distinct, but not unrelated, features of culture are relevant to comparative politics. First, culture is a system of meaning which people use to manage their daily worlds, large and small; second, culture is the basis of social and political identity which affects how people line up and how they act on a wide range of matters. The effects of culture on collective action and political life are generally indirect, and to fully appreciate the role of culture in political life, it is necessary to inquire into how the impact of culture interacts with interests and institutions.

Culture is not a concept with which most comparativists are comfortable. For many, culture complicates issues of evidence, transforming hopes of rigorous analysis into "just so" accounts which fail to meet widely held notions of scientific explanation. Culture violates canons of methodological individualism while raising serious unit of analysis problems for which there are no easy answers. Culture to many, neo-Marxists and non-Marxists alike, seems like an epiphenomenon offering a discourse for political mobilization and demand-making while masking more serious differences dividing groups and individuals. Finally, employing the concept of culture puts political scientists into a series of controversies over which proponents of cultural analysis in anthropology themselves are deeply divided. Each of these objections is addressed in this article, and while I do not argue that they are unimportant, I do not view them as sufficiently damaging to warrant throwing the baby out with the bath water.

Cultural analysis of politics takes seriously the postmodern critique of behavioral political analysis and seeks to offer contextually rich intersubjective accounts of politics which emphasize how political actors understand social and political action. In cultural analyses, for example, interests are contextually and intersubjectively defined and the strategies used to pursue them are understood to be context dependent. I argue that this view of culture can be compatible with rigorous comparison (while not denying its complexities). At the core of cultural analysis is the concept of interpretation. The interpretations of particular political significance are built from the accounts of groups and individuals striving to make sense of their social and political worlds and interpretation refers both to the shared intersubjective meanings of actors and to the explicit efforts of social science observers to understand and to present these meanings to others. Shared interpretations of actors – world views– are important in any cultural analysis and offer an important methodological tool, along with an examination of rituals and symbols, for examining both systems of meaning and the structure and intensity of political identity.

This article first discusses five contributions which the concept of culture defined as a system of meaning and identity makes to comparative political analysis: culture frames the context in which politics occurs, culture links individual and collective identities, culture defines the boundaries between groups and organizes actions within and between them, culture provides a framework for interpreting the actions and motives of others, and culture provides resources for political organization and motivation. I then examine five central themes in cultural analyses of politics: culture and personality studies, the civic culture tradition, culture and political process (an approach which originated in anthropology), political ritual, and culture and political violence. Third, I identify five critiques of cultural studies of politics: unit of analysis issues, the problem of within-culture variation, the difficulty of distinguishing culture from social or political organization, the static nature of culture in explaining political change, and the need to identify underlying mechanisms which suggest "how culture works." The fourth section examines the role of interpretation in cultural analysis as an effort to link the contextually rich political details found in particular political settings (be they small communities or countries) to general domains of political life such as authority, community, and conflict. I discuss the concept of psychocultural interpretations, and their methodological relevance in the comparative study of culture and politics for understanding processes such as ethnic and national identity construction. I conclude that culture is a too-often ignored as a domain of political life and that cultural analyses can enrich how we conceptualize areas such as political economy, social movements, and political institutions in a number of useful ways, often complementing the insights derived from interest and institutional approaches.

Electoral Behavior and Comparative Politics

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The study of electoral behavior has progressed in an *ad hoc* manner, largely unconcerned with the grander theoretical issues of political science as a discipline or with the great "isms" of the twentieth century. Most research has been country oriented; comparative politics as a subdiscipline has not greatly influenced this field and has not been greatly influenced by it. Electoral studies have formed a theoretical "island," developing a data rich research tradition that has been influenced by many conceptual and methodological trends without being monopolized by any single one. This chapter focuses on the survey tradition and evaluates culturalist, institutionalist, and rational choice contributions to its study.

A review of studies of partisan choice suggests that the bases of electoral choice have varied according to space and time. An isomorphism exists between theories of partisan choice and theories of changing patterns of the mobilization of mass publics in democracies. Mobilization has shifted from being based on *social* cleavages such as class, religion, ethnicity, and the like; to *political* mobilization based on attachments to specific political objects; and – increasingly today – to *cognitive* mobilization reflecting individual decisions based on knowledge of issues, perceptions of interest, and "preferences," including values (see Dalton, 1984).

Such an interpretation assumes that in the earlier period attachment to groups defined by societal cleavages and institutions, of which unions and churches were the most significant, was the basis of partisan identity. In the second, it was attachment to particular political parties and movements – which may have originated in the older cleavage structures but then acquired independent identities and loyalties. Advanced democracies today, with their highly educated populations, access to information through the media and elsewhere, and declining involvement in associational life, are moving toward cognitive mobilization.

Cognitive mobilization involves increasing individual processing of information, calculation of interests, and perhaps even the individual "construction" of political identities that were ascribed at birth in earlier times. It reflects emerging trends in advanced societies toward demassification, higher education, access to information, privatization, rational egoism, and, especially, the dominant role of television.

There is strong empirical evidence for the progression described above, but research findings are not conclusive. Some components, such as a decline in the importance of many cleavages, the rise in educational levels, and the role of the media, are noncontroversial. The assumed decline in involvement in organizations and attachments to parties seems generally valid but not for all countries (see especially Kaase, Newton, and Scarbrough, 1995).

Early academic studies in the United States focused on sociological variables such as occupation and group memberships; investigators were surprised that these variables proved not to be very important. However, here is little doubt that social cleavages were important determinants of the vote in Europe. Religion, class, and – in countries such as Spain and Belgium – region all fit into the category of social partisanship. While Marxist and leftist parties made extensive efforts at political education of their members – and also sought to create large membership organizations – many voters possessed little sense of a uniquely *political* identity. The vote was largely an expression of religious, class, and regional identities.

The weakness of social partisanship in the United States led researchers at the University of Michigan in the 1950's, in a widely influential series of electoral studies and publications, to develop the concept of partisan identification. Underlying the concept is a learning model (Converse, 1969) that has proved useful in explaining the relationship between age and participation, the strength of partisan attachments, volatility in voting patterns, perceived legitimacy of institutions, and other matters. Partisanship, like other aspects of culture, is largely learned through socialization. However, the learning model has flaws. Early socialization may prove especially inadequate in periods of change, in which the past is a poor guide to the present. The disintegration of major parties in established party systems in recent years is likewise damaging. The learning model does not build in intensity, strong affect, charismatic leadership, and similar factors that can accelerate or block change.

There is no argument among scholars as to whether institutions make a difference. The question is "how much?" It has been demonstrated in numerous studies that institutional differences lead to differences in behavior – when all else is held constant. But it also appears true that institutions are greatly affected by the cultures within which they operate as well as by the internal cultures that they generate over time. The question of the importance of institutions gives rise to additional questions concerning the question of what constitutes an institution – how much is structure and how much is culture – for similar structures function quite differently in different political systems.

Rational choice is at present the chief contender as a general theory of politics. However, while it has contributed to the study of electoral behavior in the United States as well as in other individual countries, its contributions to cross-national research are largely nonexistent. The empirical nature of electoral research has not meshed well with the data-less analysis and model building favored by some rational choice scholars. Few have invested heavily in the costly and time-consuming task of generating cross-national data. The elegance and parsimony of formal theory are quite promising for dealing with complexities of many countries and many variables. But the work has not yet been carried out on a substantial scale.

This chapter argues that, rather than all-purpose grand theories, it is islands of theory, theories of the middle range, which have emerged largely inductively, that have thus far proved most useful in the cross-national study of mass behavior, including electoral behavior.

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Towards an Integrated Perspective on Social Movements and Revolution

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The protest cycles of the West in the 1960's stimulated renewals of thinking about contentious politics in both western Europe and the United States in the rationalist, structuralist, and culturalist traditions. Not only that: there was also a separate development of models for social movements, revolutions, and other forms of contention. This led to a flourishing of research on contentious politics but had the consequence of leading to declining communication and synthesis between traditions and across fields. Sociologists wrote of "movements," political scientists of "protests" and "violence" and few scholars attempted to connect various forms of contention to one another or to routine politics. To make matters worse, much of the work on contention in non-Western, nondemocratic countries was being done in isolation from Western models.

In the last decade or so, some of these lacunae began to be filled and bridges constructed across traditions, areas and different types of contention. Close examination of work on social movements and revolutions reveals numerous opportunities for analogy and synthesis across phenomena and theoretical traditions. The paper begins with an outline of the main lines of the structuralist, rationalist and culturalist traditions and of the potential interfaces among them. It then distinguishes opportunities for synthesis with respect to analysis of (a) conditions generating contention, (b) mobilizing structures, and (c) framing processes. A political-process account of the American civil rights movement illustrates the promise and problems of synthesis among rationalist, structuralist, and culturalist models. A closing session alludes to the analogies among movements, cycles of protest and revolutions.

The Role of Interests, Institutions and Ideas in the Comparative Political Economy of the Industrialized Nations

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In keeping with the themes of the volume, this essay compares approaches within the literature on comparative political economy that emphasize, respectively, interests, institutions and ideas. In broad terms, these approaches correspond to three questions that have long animated the field. Whose interests are served by a given set of economic arrangements? How do institutions affect the operation of the economy? How do specific conceptions of the economy and its effects arise?

Interest-based approaches to the economy come in two dominant variants. The first emphasizes the way in which changes in the international economy shift the material interests of producer groups so as to form new coalitions behind particular economic policies. The second emphasizes the way in which broader electoral pressures lead politicians to pursue particular kinds of policies, so as to generate theories of the political business cycle, retrospective voting, and the impact of coalition government. These are powerful approaches that speak directly to the intuition that, if a pattern of policy is to be sustained, it must advance the interests of broad segments of society and of the politicians who implement it. However, most such analyses are highly sensitive to the economic theories used to specify the material interests of the relevant actors and some are relatively insensitive to the collective action problems associated with coalition formation.

Institution-oriented approaches emphasize how the institutional structures that organize the political economy affect the character of economic policy and performance. Initially rooted in the literature on neo-corporatism, such approaches now emphasize the way in which a variety of institutional features, ranging from the character of wage bargaining or the organization of the financial system to the independence of the central bank, interact to generate nationally distinctive economic outcomes. Such analyses provide us with a way of understanding why common economic challenges may not produce convergent responses across all nations or regions and, in recent years, they have begun to restore the firm to a central place in the analysis of the political economy. However, as more interaction effects are observed, it becomes increasingly difficult to put nations into neat categories and those who take this approach face increasing pressure to devise general theories of institutional determination, including theories capable of explaining the resilience or mutability of institutions.

Idea-oriented approaches stress the role of ideas in the determination of economic policy and come in several variants. One privileges interest-based explanations but incorporates ideas to complete the causal chain, for instance, as focal points specifying one from among several competing equilibria. Another emphasizes the way in which ideas about appropriate policy in the relevant communities of experts acquire influence over policy and become institutionalized in standard operating procedures. A third goes farther to explain policy in terms of the cultural outlooks characteristic of particular nations. All of these approaches remind us that the economy is not visible to the naked eye and can be modelled in multiple ways. However, it proves especially difficult to disentangle the effects of 'ideas' from those of other variables and to explain how ideas can be persuasive in themselves or at least partially independent of the power of their proponents.

Few scholars emphasize interests, institutions or ideas to the exclusion of the others; and there is considerable potential for integration among these perspectives. However, the field also faces a more basic methodological divide between those who view political economy as an effort to apply the methods of economics to politics and those who emphasize the way in which noneconomic factors, associated with politics or culture, influence the course of events that the former attempt to explain in largely functional or rational terms.

Proponents of institution-oriented approaches find themselves in the middle of such debates and, perhaps as a consequence, some of the most interesting developments in the field are taking place at the interface between this and the other two approaches. At one interface, the "new economics of organization" and "endogenous growth theory" have opened up new ways of reconciling institutional approaches with traditional economic theory. At the other, a series of debates about the character of "social capital" and the "new institutionalism" in organization theory provide sites for fruitful dialogue with those who stress the importance of culture to economic outcomes.

Contemporary economic developments are bringing the field back to some of the core issues that inspired its founders, including, most notably, questions about the relationship between the state and the market, given that each provides a different kind of mechanism with the potential to advance the "public interest". However, there are still at least three significant gaps in the literature.

Paradoxically, the field as a whole has paid little attention to the problem of explaining the distributive inequities that national patterns of policy and performance can generate. It has not yet tied the study of economic policy to the questions about representation, which have long been central to political science. And none of the three approaches surveyed here have yet captured very well the contribution that political conflict and debate can make to the construction of interests. These issues provide research frontiers on which recent work has only now begun.

Studying the State

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Political science has most often treated states as firmly bounded entities. Within the state's boundaries, citizens have also been seen in clear, formal terms. Both realist and liberal approaches in international relations, for example, have assumed the notion of states exercising sovereignty within their existing borders. And, in comparative politics, the notion of universal citizenship has been key in terms of states' claims to legitimacy as representative of fairly homogeneous populations (expressed as the nation or society).

This paper will re-examine the conceptions of states' fixed boundaries and of a single society, represented by the state, within those borders. Western European unification, mass transnational migration, state disintegration in eastern Europe and Africa, spiralling international capital flows and more impel political scientists to view state boundaries, control, and sovereignty in different terms. Varying classifications of citizens – either formally, as in Indonesia, or informally, as in Germany – also demand re-thinking the relationship of the state to society and of social groups to each other. Some preliminary work – e.g., Yoav Peled's article in the *APSR*, "Ethnic Democracy and the Legal Construction of Citizenship," and Yasemin Soysal's *Limits of Citzenship* – begins the process of assaying the mix of state, ethnicity, and citizenship. Now, political science needs to make state-formation and society-formation, as well as the relationship between them, a central part of its agenda.

Much of our existing thinking stems from earlier conceptions of politics as the clash of established interest groups within a given territory. Such thinking gives rise to methodologies and approaches, such as rational choice or standard structuralist theories, which assume fixed preferences from which political scientists can deduce action.

The fluidity of state and social boundaries that we are witnessing at the end of the twentieth century must lead us to question our old approaches. We must now begin to treat states and social groups not as established, but as variable. How and why do they form and re-form? Politics cannot simply be understood in terms of well-defined structures with given preferences, as rational choice or structuralism would have it. We must work towards an understanding of politics given the indeterminacy of the boundaries of states and social forces.

What is the Current State of Theory in Comparative Politics?

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Compared to twenty-five years ago, self-conscious theoretical reflection finds almost no home within our field. We do not take our theories nor our theorists seriously. Evidence to support this harsh judgment comes from our leading journal's recent symposium on "the role of theory in comparative politics" (*World Politics*, October 1995). The participants minimized the value of deductive, *a priori* theorizing of the sort that is done within strongly defined research communities. Moreover, in spite of the fact that the symposium included widely acknowledged experts in specific research traditions, apparently no one viewed, for example, today's rationalist/culturalist divide as theoretically interesting, exciting, and productive. Structural or institutional analysis was not even recognized as a theoretically distinctive enterprise but rather was thought of as a part of the field's "messy center." Most participants feared that the field might return to the sort of Marxist/functionalist debate that characterized it in the 1950's and 1960's. Consequently, method – prediction, comparison, counterfactuals, history, quantitative and qualitative data, explanation, interpretation, causation, and generalization – was on everyone's minds. The "nomothetic" vs. "ideographic" divide was what really animated discussion. The consensus was that most comparativists are part of the consensus: today's comparativists practice "theoretically informed empirical political analysis" and adopt "diverse conceptual lenses" (2) and "eclectic combinations" (5). They are interested in "questions" and "empirical puzzles" (10). Hence, "comparative politics is very much a problem-driven field of study" and comparativists are mostly interested in solving "real-world puzzles" (46).

The flaw of this pragmatist, means-oriented heaven was perceptively recognized by the symposium's organizer. He concludes that "if the problem orientation of the field tends to relegate the role of theory mainly to that of a tool of empirical research, the quest for causal generalizations, by contrast, moves its role to the forefront" (47). Similarly, the conclusion from a methods symposium on comparative (small-*n*) studies in another leading journal (*American Political Science Review*, June 1995) may be stated here as paraphrase of Kant: good theory without good research design is empty; good research design without good theory is blind (454). As Rogowski's (1995) important essay makes clear, one cannot begin inquiry with "evidence" derived from and used to test "theory"; one must begin with theoretically-embedded observations. The inevitable conclusion is that one will eventually reflect on the nature of that theory – which leads one to questions broadly defined as "social theory" or "philosophy of social science."

World Politics's symposium did not contribute to the cause of theory in comparative politics because its picture of theory in our field as dominated by a "messy center" is inaccurate and self-defeating. My essay seeks to refute that perspective and advance theory in comparative politics in three ways.

First, I recognize that three research traditions – the rationalist, the culturalist, and the structuralist – are active in contemporary comparative politics. Section 2 thus begins the analysis with three exemplary comparativists. Each thinks of himself or herself as a member of a strong research community. Robert Bates (1989) argues that he is a rationalist, James Scott (1985) identifies with the culturalists, and Theda Skocpol (1979) places herself within the structuralist school. While each recognizes the value of synthesis and the cross-fertilization of ideas, each is principally concerned with advancing a particular intellectual tradition and theoretical agenda that transcends comparative politics.

Second, I set the dialogue among the schools within the historical context of the development of social theory. Section 3 thus attempts to understand the three research communities by tracing them back to Talcott Parsons's (1937) effort to systematize the classic social theorists and thereby integrate social theory. I have modified his approach to take account of the structure-action problem of reconciling individuals and collectivities. I call this modified approach the socially embedded unit act. Using this meta-framework to provide insight into the individual frameworks, I explore the differences among the three traditions with respect to core assumptions, explanatory strategies, and historical developments.

Finally, I set the dialogue within the framework of the historical situation confronting the contemporary world. Section 4 thus seeks an underlying unity in rationalist, culturalist, and structuralist thought by delving even further back to Max Weber's master problem of a century ago. Weber studied the dialectic of modernity in world historical and comparative perspective: how reason and irrationality manifest themselves at individual and societal levels with great normative and empirical significance. The dialectic is important to contemporary politics in the West. Due to the West's influence on the globe, the dialectic is equally important to the entire world community of nations.

Section 5 summarizes my theme about the problem situation of contemporary comparative politics: there are fundamental difficulties with a field that consists only of a "messy center" and basic virtues with a field that embraces creative confrontations, which can include well-defined syntheses in particular research domains among strongly defined research communities. Comparativists should explore the rationalist-culturalist-structuralist debate and thereby appreciate the different structure-action combinations of interests, identities, and institutions that guide inquiry. Even self-described "problem-oriented" comparativists – those who think of themselves as part of a "messy center" – should be aware of the competing research traditions that have historically been a part of our field. We cannot remain theoretically challenged – a field of theoretical Philistines – and actually solve substantive problems. Contemporary comparative politics therefore will be greatly enriched by a dialogue among the traditions, especially one that is informed by self-conscious reflection about the enduring issues of social theory. Comparative politics needs strong and yet mutually sympathetic intellectual communities: believers who raise questions and nonbelievers who appreciate answers.

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Reformulating Scien-

tific Understanding and Advancing Theory in Comparative Politics

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Explanations based on covering laws and causal accounts have long defined the set of acceptable forms of scientific understanding in comparative politics. The principles of the research schools that guide analysis in comparative politics reflect this expectation. Rational choice theory draws heavily on the epistemology of logical positivism, centering explanations around covering laws. Structuralist analyses combine nomological and causal explanations. At the heart of the divisions among culturalists is a fundamental disagreement over the nature of explanation. While some interpretivists seek causal accounts and others apply nomological principles, many seek only to understand an actor's goals, eschewing the need for theory or causal mechanisms, moving their scholarship outside the realm of scientific explanations. In comparative politics, as in political science and all disciplines that claim to be science, covering law and causal explanations have long stood as the standards for scientific understanding.

The widespread appreciation of these standards to the contrary notwithstanding, there is reason to expand the models of scientific understanding used in comparative politics. Each of the standard forms of explanations has deficiencies. Some of these are long-standing, appearing in the work of philosophers of science. Others derive from recent advances in various sciences, that highlight the limited conception of reality inherent in the standard forms of scientific understanding. Causal and nomological explanations apply only to simple and determinate patterns, and they assume that reality rests on smaller, more basic phenomena. They envision a world composed of linear relationships among variables; parity in the size of cause and effect; recurrent, determined and closed-ended patterns; and the fundamental insignificance of chance happenings. Analyses of complex dynamic systems, however, call attention to the limitations of this ontology. Nomological and causal explanations are not useful for the analysis of a world of nonlinear relationships among phenomena; no necessary parity between size and effect; sensitive dependence on initial conditions; the possibility of change at any point in time; open-ended processes; and the presence of chance as a substantive part of processes and their explanations – all of which are characteristics of an ontology associated with the chaos theory and other modes of analyzing complex dynamical systems. Covering law and causal explanations apply to a relatively limited set of phenomena.

Although the precise utility of chaos and complexity theory in comparative politics still remains to be demonstrated, there are obvious parallels between political phenomena and the world described in these new fields of study. Consider the following summary statements about recent research:

1. Political and social structures persistently display fluid and complex patterns. Ethnic, social class, and political diversity characterize European and North American societies over the past century.
2. Political attitudes are complex, variable, and probabilistic. Hence, explanatory questions need to examine the probabilities that accompany phenomena.
3. Nonlinearities characterize interactions among citizens and the people around them. The influence of social contexts and discussion networks is not simple and straightforward but complex and interactive.
4. Formative characteristics of political organizations and decisions constrain subsequent processes and events.
5. Political protest, revolutions, and cabinet crises display unstable patterns, making their emergence unpredictable.
6. More generally, chance factors influence social and political patterns. Chance is neither "statistical noise," nor "unmeasured variables." Chance is an inherent part of the political world.

In comparative politics, research explores a political world that is not encompassed by the simple ontology of causal and nomological explanations.

Reformulating the epistemology and ontology of comparative politics affects the research schools. Rationalists need to reduce the domain of what they study or expand their theoretical principles. Classic rational choice theory applies to limited types of political phenomena: the simpler the process, the more defined the rules and the meanings of winning and losing, the more likely are persons to use the cognitive processes of instrumental rationality, and the more likely are rationalist principles to be useful. The more complex the circumstances, the more likely are people to draw on other sorts of knowledge and reasoning, in which instrumental calculation plays a limited role. In turn, structural analysis needs to jettison its realist assumptions. State, ethnicity, social class, political cleavage and other concepts are not natural types. They do not signify cohesive sets of persons; aggregate patterns are not easily predicted, and they are certainly not determined. This mode of analysis needs to combine several distinct principles: (a) the persistent effect of formative patterns and decisions; (b) the expectation of unpredictable events that have major consequences; (c) the divergence of like systems over time; (d) aggregate characteristics that may not be reduced to the decisions of individuals; and (e) the inherently probabilistic effect of structured relationships on individuals. Substantial changes in the principles of structural analysis follow from changing the understanding of the underlying political reality.

There is reason to alter the standards of scientific understanding used in comparative politics. General theoretical propositions may include law-like propositions, not necessarily laws. They may apply to specific sets of cases; they need not display an unlimited scope. The effort to establish covering principles stands in the way of other kinds of explanations. It directs research to examine the accuracy, reliability, and the domain of general laws. It directs attention away from the analysis of more tractable problems. It is impossible to establish general laws and causal mechanisms with absolute certainty. All explanations require assessments of their relative certainty. All benefit from tests that eliminate nuisance factors and assess the power of rival plausible hypotheses. Note as well that all explanations require theories; in their absence, the selection of explanatory variables is arbitrary.

Comparativists need to combine general claims and particular details. Formal models in comparative politics require bridges that link the abstract mathematical claims to the explanation of particular cases and sets of cases. Statistical models should not be bound by the assumptions inherent in linear models. "Why questions" need to include the probability of the emergence of particular events, not only their absence or presence. Analysis may include process models that respond to "How questions," moving the analytic focus away from questions about emergence and cross-national variation. Chance should find a central place in political analysis. The complexities of the political world need to be incorporated into the theories of comparative politics.

As these changes occur, fresh theoretical combinations emerge. Rationalists, culturalists, and structuralists examine rationality as one element in a system of meaning. Scholars from all three schools examine the relationship between individual decisions, social contexts, and institutions. Blending theoretical positions risks obliterating distinctions without necessarily leading to theoretical gains. Rigorous adherence to the standards of explanation in comparative politics enables theory to advance, distinguishing the path of scholarship from the swamp of the messy center.

News & Notes

(News & Notes began on page 3.)

The committee also conferred the award upon George Tsebelis for his article, "Decision Making in Political Systems: Veto Players in Presidentialism, Parliamentarism, Multicameralism, and Multipartism," *British Journal of Political Science* 25, 3 (July 1995): 289-325.

The runners up for the article award were Jonas Pontusson for "Explaining the Decline of European Social Democracy: The Role of Structural Economic Change" in *World Politics* (v. 47, no. 4 (July 1995): pp. 495-533), Sylvia Maxfield for "Financial Incentives and Central Bank Authority in Industrializing Nations" in *World Politics* (v. 46, no. 4 (July 1994): pp. 556-589), and Michael Bratton and Nicolas van de Walle for "Neopatrimonial Regimes and Political Transitions in Africa" in *World Politics* (v. 46, no. 4 (July 1994): pp. 453-489).

Next year, the selection of awards will be divided among three committees. Two will confer prizes in the name of Greggory Luebbert; one for the best book and the other for the best article published in the field of comparative politics in the last two years. The committees will consist of:

Luebbert Book Committee: Sam Popkin of UCSD (Chair), Jeff Frieden (Harvard University), and D. Michael Shafer (Rutgers).

Luebbert Article Committee: Gary Cox of UCSD (Chair), Susan Whiting (University of Washington), and Arun Agrawal (Yale).

A third committee will deliver the Sage Award, endowed by Sage Publications, for the best paper presented at the annual convention. The committee will be chaired by Desmond King (Oxford), and will include Allan Kornberg (Duke) and Peter Gourevitch (UCSD).

Nancy Bermeo (Princeton) is in charge of organizing panels for the Annual Meetings of 1997. In a new departure, the Executive Committee has recommended that she work with Miriam Golden, editor of the *Newsletter*, to orchestrate debates over central issues in the field, the debates to originate in the *Newsletter* and to culminate in round tables or panels at the Convention.

The first appears in this issue; the June issue will focus on rational choice and political culture.

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Paradigms and Sandcastles: Research Design in Comparative Politics

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Forthcoming, University of Michigan Press

The last twenty years have been unkind to students of politics in countries outside the North Atlantic core. At precisely the moment when the shift to authoritarianism had "been fully explained by a variety of converging approaches and [was] therefore understood in its majestic inevitability and perhaps even permanence" (Hirschman 1979, 98), democratization swept through large numbers of countries. In a second equally unexpected development, numerous governments began to abandon state interventionist economic policies in favor of greater market orientation. On top of everything else, the Soviet empire collapsed. Though scholars have greeted many of these events with delight, they did not predict them and, even today, could if they wished explain more persuasively why these events should not have taken place than why they have.

Confronted by these compelling and exciting events in the world, scholars quickly turned their attention to trying to understand them. One of the first fruits of these investigations was the recognition that few of the theories dear to the hearts of comparativists offered much leverage for explaining recent events. The inability to explain, much less foresee, these transitions has made us more conscious than ever of the paltry accumulation of theoretical knowledge in the comparative field. Imaginative theories and sweeping paradigms arise to the accompaniment of excitement and fanfare, but they disappear with equal frequency and rapidity, leaving scarcely a trace. Why?

I begin the book with the argument that our limited success in accumulating theoretical knowledge derives largely from methodological and research design norms in the comparative field. Because of these norms, we have failed to test rigorously the arguments we advance and have accepted the arguments of others without demanding that they be supported by strong evidence. The book aims to show in a compelling manner why standard methodological practices in the comparative field should be improved. It does this by demonstrating the consequences of some of the methodological pitfalls most characteristic of large parts of the subfield.

The best argument that methodological issues need to be taken seriously is the demonstration that inappropriate methodological choices lead to wrong substantive conclusions. Much of the book is taken up with retests using more appropriate research designs of arguments in important and respected work with which most comparativists are familiar. By using, where possible, graphical demonstrations of the logical flaws involved in the original research designs, and by drawing examples of what can go wrong from well-known and respected literature, I make clear the undesirable consequences of methodological practices usually considered acceptable by comparativists.

All the studies discussed in the book are intelligent, plausible, insightful, and possibly correct in their knowledge claims. All have been advanced by highly respected social scientists. The effort here is not to discredit arguments or belittle authors – who are, after all, working within accepted conventions – but to demonstrate the deficiencies of the conventions themselves. These conventions affect not only authors but readers of comparative politics. Authors, including some of those discussed below, are frequently aware of the tentativeness of the evidence supporting their arguments and indicate their awareness in the caveats they attach to them. Readers, however, tend to ignore the caveats and give greater to weight to unsystematic evidence than it deserves. Many studies in which authors have carefully hedged their explanatory claims are discussed in seminars, cited in literature reviews, and summarized in qualifying exams as though the tentative arguments advanced were actually supported by solid evidence. One of the purposes of the book is to decrease the credulity of readers.

Some comparativists may find the book controversial, since it criticizes practices widely accepted and self-consciously defended within the subfield. I believe, however, that methodologists will see it as an accessible exposition of well-known ideas. All but one of the methodological ideas presented simply show the implications for non-quantitative work of ideas about the logic of research design that have been carefully worked out and accepted without reservation among those who do quantitative work. I expect the book to be useful to social scientists with little training in statistics and to be assigned in introductory graduate classes on research design and methods.

Chapter I: Rise and Decline of Paradigms in Comparative Development

This section discusses the rise and fall of theories of development from World War II to the present, making the case that the scholars who advanced these theories failed to make full use of available evidence. As a result, later researchers rapidly discovered that these theories could not account for events and processes in the world. In consequence, the theories, and eventually the paradigms in which the theories were embedded, were swept away like sandcastles.

Though all theories may be "born in ideological sin rather than scientific virtue," to use Dick Sklar's words, the reason for the failure to accumulate theoretical knowledge in the field of comparative development is not ideological bias *per se*, but that the methodological norms of the subfield do not compensate for individual scholars' biases. In principle, the systematic testing of hypotheses and replication of studies conducted by other scholars should gradually eliminate the biases introduced into analyses by individual scholars. Norms in the comparative politics field, however, legitimize research designs that preclude even minimally effective tests of hypotheses and set standards of evidence that are too low to overcome ideological bias.

In the book, I deal with several characteristic features of research design and the use of evidence to support knowledge claims that have wide acceptance in the comparative field. The specific practices discussed at length include:

- Selection on the dependent variable
- The comparative case study method in complex, path dependent explanations
- Fluid definitions of key variables
- The attempt to explain large complex outcomes such as democratization and revolution

In chapters dealing with each of these, I lay out the logic underlying what the researcher does when he or she use the practice I criticize. I use extended examples drawn from well-known authors and schools of thought to illustrate each methodological problem.

Chapter 2: Selection Bias

One of the most durable conventions in comparative politics is the selection of cases for study on the dependent variable. That is, if we want to understand something, for example, revolution, we select one or more occurrences and subject them to scrutiny to see if we can identify antecedent events as causes. This chapter demonstrates the consequences of this research strategy by retesting arguments made in Theda Skocpol's *States and Social Revolutions* and in the literature on successful economic performance in the Asian NIC's, using evidence drawn from samples of cases which, unlike those used in the original arguments, are not correlated with the outcome on the dependent variable. In each retest, an unbiased sample yields conclusions at odds with those originally advanced.

Chapter 3: Case Studies and Path Dependence

The comparative case study method, though useful in some kinds of studies, precludes serious tests of theories when paired with the complex, path dependent arguments often made by comparativists. Path dependent arguments begin with an initial causal hypothesis about an outcome at time one, followed by a series of intervening causal hypotheses to explain a sequence of later outcomes. At each intervening point, new independent variables enter the argument to explain different outcomes, and whichever outcome occurs limits what may happen in the future.

Each link in a path dependent argument could in principle be tested rigorously by identifying the universe of cases in which a particular hypothesis should hold (that is, cases having the initial conditions specified in the earlier stages of the argument), randomly choosing a sample from the universe if it is large, and examining the outcomes in those cases to see if they are consistent with the hypothesis. Reliance on comparative historical case studies of a limited number of countries as the primary source of evidence, however, eliminates the possibility of this kind of testing procedure. Instead of testing each hypothesis on the universe of cases to which it, though perhaps no other hypothesis in the sequence should apply, a small number of cases are selected at the beginning and followed in detail throughout the relevant historical period. Consequently, all hypotheses are tested on a few, often three, cases. By the end of the analysis, the number of independent variables almost always exceeds the number of cases.

To demonstrate the possibility of finding cases that fit the initial conditions required to test later stages of path dependent arguments, I retest Lipset and Rokkan's "freezing" hypothesis on a set of Latin American countries that, though not included among the cases they examined, meet the initial conditions set out at the relevant stage in their argument. As in the selection bias examples, the retest fails to confirm the original argument.

Chapter 4: Fluid Definitions of Variables

No one would argue against the need to define concepts and variables precisely. Nevertheless, the meaning of key causal variables often seems to vary as an author moves from one element of the argument to another, case to case, or time period to time period. The concepts most subject to this kind of slippage are those with no clear empirical referents, such as state autonomy, threat, power, and insulation. I demonstrate the effect of loose variable definition by choosing a few key variables from important studies, operationalizing each to reflect different definitions available within a single work by a single author, conducting tests of the argument advanced using different operationalizations, and showing the differences in conclusions that result.

Chapter 5: "Big Structures, Large Processes, and Huge Comparisons"

In this chapter I argue that we can more fruitfully focus research on underlying processes than on final outcomes when trying to understand major events in history. The point of this argument is not that there is anything methodologically wrong with trying to explain major events, but that our ability to do so is severely limited by the interaction of two problems: the large role of non-systematic factors in determining any particular outcome; and the very primitive techniques we rely on, which give us no way of determining when we have identified systematic factors that explain some but not all of the variation in outcomes. Perhaps the best example of this problem can be found in contemporary efforts to explain democratization. Most are frustratingly atheoretical, in part because it can be shown that virtually any cause identified as important in some set of cases was not important in one or more other cases, and in part because analysts who have observed the process closely know that serendipitous factors such as deaths of key leaders, personality traits, and natural disasters have influenced outcomes. In my view, these are insoluble problems as long as we choose major events as dependent variables.

In choosing such complex dependent variables, we inadvertently paint ourselves into a methodological corner in which inductive research strategies prevail, and the implicit model of explanation always turns out to be an enormous kitchen sink regression. While simple deductive explanations of these outcomes never seem to be confirmed by evidence, the long lists of hypotheses generated by inductive research strategies cannot be tested because variables always end up outnumbering observations.

At its best, this approach to the study of big questions is analogous to that of medical researchers who try to understand the onset of cancer by amassing data on all the dietary, hereditary, and environmental factors that can be imagined to increase the likelihood of the disease. This is useful research. It results in the accumulation of factual knowledge and leads to some inductive generalizations. It does not, however, lead to an understanding of the process through which cancer develops. For that, researchers must step back from the complex outcome, the diseased person, and focus instead on basic mechanisms, for example, the nature of cells and the genetic mechanisms that regulate cell division. They must concentrate on the units within which the process occurs, the cell and the chromosome, rather than on the overall outcome that results: the diseased organism.

In a similar manner, I think students of comparative politics need to seek to understand underlying processes rather than "explaining" complex outcomes. To do that, we need to focus on the fundamental unit of politics, in most cases the individual. We need to break up the traditional big questions into more precisely defined questions that are more theoretically accessible. This would involve a redefinition of the questions of interest so that the construction and testing of theories becomes possible.

To demonstrate this approach in practice, in this chapter I examine the process underlying one component of regime transition, the breakdown of authoritarian regimes. I use simple game theory to model the relationship between dictators and their supporters and rivals in three types of authoritarian regime, showing the effects of different authoritarian regime types on the probability and manner of regime dissolution. I then test a few implications drawn from the models, using evidence from a data set that includes all authoritarian regimes in existence at any time between 1946 and 1990.

The purposes of this exercise are twofold. First, it is a demonstration of what I mean by a focus on process rather than outcome. Using game theory is a way to emphasize the incentives facing decision makers and the logic of the situation they face. It simplifies reality in a useful way, allowing the analyst to concentrate on particular interactions without being distracted by all the other things that are simultaneously occurring in the world. The models do not seek to explain regime transition; rather, they aim to explain a process that contributes to regime transition.

Second, this example illustrates the research strategy of testing the implications of an argument rather than the argument itself. Many arguments cannot be tested, for either practical or logical reasons. Nevertheless, we want to have some assurance that they are not false. Often some of the implications of an argument can be tested, even though the argument as a whole cannot. I seek to persuade readers that a wide-ranging search for testable implications should be a part of every conscious research strategy.

Chapter 6: Contending Approaches to Theory Building

This chapter considers what approaches to explanation are most likely to result in the accumulation of theoretical knowledge. It discusses the characteristics that explanations need to have in order to be useful in the social enterprise of knowledge building, and assesses several of the approaches now advocated by comparativists in light of these needs. A review essay on the uses and limitations of the rational choice approach illustrates the argument. The point here is not to advocate further imperialism by rational choice but rather the creation of other, possibly more realistic, approaches that have most of the characteristics that have given rational choice its fruitfulness, stamina, and reach.

Conclusion

A new theory is like a river in spring. Rushing down from the high ground, it cuts a narrow channel through the wilderness of complexity. When it encounters factual obstacles too large to sweep along, it should be diverted into a new, equally rapid and narrow course. In the comparative field, however, old theories, modified by many collisions with inconvenient facts, are like rivers that have reached the delta after crossing a broad plain. They dissipate into numerous small channels meandering through swamp until merging gradually and imperceptibly into the sea of thick description. We have now reached the swamp stage on multiple fronts. This is not, in my view, a temporary state of affairs. Theoretical inadequacy will continue to plague us as long as we remain loyal to the time-honored methodological traditions of comparative politics.

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A Solution to the Ecological Inference Problem: Reconstructing Individual Behavior from Aggregate Data

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In Press, Princeton University Press, 1997

My special thanks to Miriam Golden for encouraging me to write this article.

For several years prior to publication, I ran a "virtual seminar" on the subject of this book with faculty and students from other universities. The arrangement was that participants would email me while reading the manuscript with anything they found unclear, unhelpful, or untrue, and in addition to revising the manuscript I tried to return the favor with immediate explanations via email of anything that was holding them up. As the book is now in production, the seminar has ended, but I would still be happy to respond to related inquiries.

Introduction

This article summarizes a book manuscript that gives a solution to the ecological inference problem, a method of inferring individual behavior from aggregate data that works in practice. Below, I describe the problem in somewhat more detail, give an example of how the method works, and discuss how the solution can be of use in comparative politics research. To save space, I do not describe the details of the statistical procedures introduced. (The title of this article is the tentative title of the book. Until publication by Princeton University Press in April 1997, the complete current draft of the book is available from my home page on the World Wide Web, http://GKing.Harvard.Edu; also available there is an easy-to-use computer program that implements all the statistical and graphical methods introduced.)

Ecological inference, as traditionally defined, is the process of using aggregate (i.e., "ecological") data to infer discrete individual-level relationships of interest when individual-level data are not available[1]. The best existing methods usually lead to very inaccurate conclusions about the empirical world; indeed, frequently, they give impossible answers, such as -20% of Israeli Labor Party voters remaining loyal between the last two elections. The ecological inference *problem* is to develop a method that gives accurate answers in practice. Ecological inferences are required in political science research when individual-level surveys are unavailable (e.g., local or comparative electoral politics), unreliable (racial politics), insufficient (political geography), or infeasible (political history). They are also required in numerous areas of public policy (e.g., for applying the Voting Rights Act) and other academic disciplines ranging from epidemiology and marketing to sociology and quantitative history. The ecological inference problem has been among the longest standing, most actively pursued, and consequential in all of quantitative social science. It was originally raised over 75 years ago as the first statistical problem in the nascent discipline of political science (Ogburn and Goltra, 1919) and has held back research agendas in most empirical subfields of the discipline ever since.

The method introduced gives accurate estimates as well as reliable assessments of the uncertainty of all inferences. It is robust to numerous data problems such as high levels of aggregation bias. Because the ecological inference problem is caused by the lack of individual-level information, all methods of ecological inference, including those introduced in the book, will always entail some risk. These risks are minimized in the approach taken by new models that incorporate far more available information, intuitive graphics and diagnostics to evaluate when assumptions need to be modified, and easy methods of modifying the assumptions. To verify that the method works in practice, I use a variety of new data sets for which the true, individual-level answer is known. This makes possible over 16,000 comparisons between estimates of individual-level relationships from aggregate data and the known individual-level answer. (This compares to 49 such comparisons in the history of research on this subject.) The method works in practice.

Example

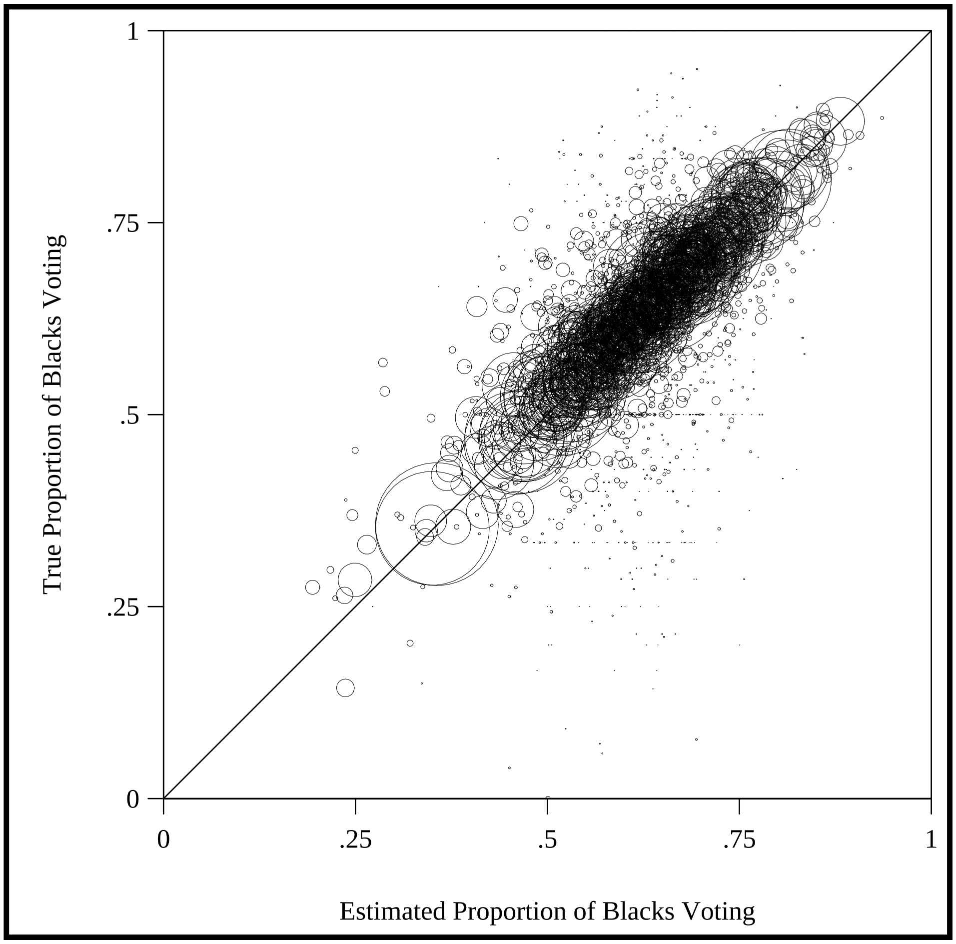
The goal of ecological inference in this example is to fill in the unobserved cell entries given only the observed aggregate percentages from the column and row totals in this table (from Louisiana in 1990) and the corresponding tables from each of Louisiana's 3,262 precincts (i.e., without survey data). For example, the upper left cell entry is the (normally unobserved) percent of blacks

Given these marginal percentages from Louisiana in 1990, what are the cell entries?

	Vote	No Vote	
Black	?	?	26.6%
White	?	?	73.4%
	68.5%	31.5%	

who voted in 1990. Since the cell entries in this case are known from public records, I can report that the true value of this cell is 64%. The estimate from the method reported in the book, based only on aggregate data, is only a fraction of a percentage point under this mark. In this example, like numerous others reported in the book, the method gives accurate statewide estimates, which has been the goal of past research.

But the solution to the ecological inference problem turns out to provide much more interesting information than accurate statewide estimates. It also provides, in these data for example, accurate estimates of the fraction of blacks who vote (and fraction of whites who vote) for each of Louisiana's 3,262 electoral precincts. For example, the following figure compares *estimates* from aggregate data of the percent of blacks who vote to the *true* percent of blacks who vote in all Louisiana precincts (with each precinct represented by a circle sized proportional to its black population). The center of almost all the circles falls on or near the diagonal line, indicating that the estimated percent of blacks voting is very close to the true individual-level percentage. This figure is not merely a plot of the observed values of a variable by the fitted values of the same variable used during the estimation procedure: it is instead a much harder test because the true fractions of blacks voting (the vertical dimension in the figure) were not used during the estimation procedure.



Here's to you, Mr. Robinson

Nearly half a century ago, a single article by William Robinson (1950) diverted the stream of academic research. By popularizing "the ecological fallacy," greatly clarifying the ecological inference problem, and (appropriately) convincing generations of scholars that aggregate data should never be used (with methods then available) to infer information about individuals, he fundamentally changed the nature of social science research. Vibrant fields of scholarship that relied on aggregate data withered. Once-important traditions of political geography in France, Germany, and the U.S. largely collapsed. The scholars who continue to work in some of these fields – such as those attempting to explain who voted for the Nazi Party, or which social groups support each political party in the newly emerging democracies – do so because of the lack of an alternative to ecological data, but they toil under a cloud of great suspicion.

Research based on aggregate data was succeeded at mid-century with the then emerging methodology of survey research. Surveys have not only taught us a great deal; they may well represent the single most important methodological contribution of the social sciences of this era. Because better data beat more sophisticated statistics every time, the ecological inference problem does not arise if accurate survey data are available. (Although in almost no cases is existing survey data sufficient to obviate ecological inferences at local geographic levels.) Yet, an exclusive focus on available surveys has had some unavoidable costs. Surveys necessarily force analysts to study recent periods, and thus to miss long-term trends and some large-scale patterns. Moreover, when we choose our subject to study based on available survey data, we study random collections of isolated individuals from essentially unknown geographic locations. We thus necessarily lack the ability to gather knowledge about local communities, contextual effects, or geographic patterns. Creative combinations of quantitative and qualitative research are also much more difficult when the identity of and rich qualitative information about individual respondents cannot be revealed to readers. Indeed, in most cases, respondents' identities are not even known to the data analyst. If "all politics is local," political science is missing much of politics.

In contrast, aggregate data analysis can almost always be based on extensive, detailed, and local, geographic patterns; it can extend over very long time frames; and it can be supplemented with qualitative information at any level of richness or detail. In fact, with a working method of ecological inference, there is little reason for any division between quantitative and qualitative approaches to a problem. Systematic quantitative analyses can be easily joined with richer qualitative data at the local geographic level and any study will be improved as a result. Going into the field and visiting villages and communities from which data are available becomes a central task once again. In fact, if some limited survey data are available, it too can be used with the method proposed to improve ecological inferences.

Moreover, the quantity and quality of unanalyzed aggregate data sets waiting around for enterprising political scientists to take notice is staggering. In the U.S. alone, there are political data on dozens of electoral offices and 3000+ census variables available from each of 190,000 electoral precincts. Most of these data have been untouched by social scientific hands. The situation is not much different in most other parts of the world. When the Berlin Wall fell, scholars of Eastern Europe and the former Soviet Union found that behind it all these years was an ocean of data, and those standing nearby were inundated when the flood began. With these and other massive unanalyzed aggregate data sets pouring in from all over the world, and a solution to the ecological inference problem, much opportunity remains.

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[1] The name 'ecological data'' dates at least to the late 1800's. It stems from the word ecology, which is the science of the interrelationship of living things and their environments. Statistical measures taken at the level of the environment, such as summaries of geographic areas or other aggregate units, are known as ecological data. Ecological inference is the process of drawing inferences from these ecological data about the individuals within the aggregates or areas.

News & Notes

(News & Notes continued from page 17)

At the end of 1996 *Current Sociology* published a special issue entitled "Political Sociology at the Crossroads", edited by Baruch Kimmerling. The volume includes:

- Erik Allardt - "Is There a Scandinavian Political Sociology Today?"
- Birgitta Nedelmann - "Between National Socialism and Real Socialism: Political Sociology in the Federal Republic of Germany"
- Christopher Rootes - "Political Sociology in Britain: Survey of the Literature and the Profession"
- Victor Voronkov and Elena Zdravomyslova - "Becoming Political Sociology in Russia and Russian Transformation"
- Hieronim Kubiak - "Hopes, Illusions and Deceptions: Half a Century of Political Sociology in Poland"
- Dipankar Gupta - "Engaging with Events: The Specifics of Political Sociology in India"
- Habibul Haque Khondker - "Sociology of Political Sociology in Southeast Asia and the Problem of Democracy"
- Elisa P. Reis - "Political Sociology in Brazil: Making Sense of History"
- Anthony M. Orum - "Almost a Half Century of Political Sociology: Trend in the United States"
- Baruch Kimmerling - "Changing Meanings and Boundaries of 'Political': Some Conclusions"

Book Reviewers Welcome

Doctoral students at any institution are welcome to submit proposals for book reviews. To do so, contact the editor and/or assistant editor with the name of the book you wish to review and a short (approximately 200 word) discussion of the book's importance and why you wish to review it. Books must have been published no more than two years previously. We will let you know if the title is still available for review, as well as the deadline for the next issue of the *Newsletter*. Reviewers are responsible for procuring their own copies of books. Reviews should run 1,000 to 1,500 words, must be submitted electronically, and must be in English.

Democracy and Development: Political Regimes and Economic Performance, 1950-1990

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Forthcoming, Cambridge University Press

Introduction

The central question is whether democracy in the political realm fosters or hinders economic development.

We first dichotomize political regimes as democracies and dictatorships and ask: under which regime are economies more likely to develop? Which is more likely to generate miracles and which disasters? Which is more likely to assure that the country does not go backwards, that its development is sustained? Which is more apt to exploit advantageous conditions and which is more adept at coping with adversities? Then we delve deeper, distinguishing different types of democracies and dictatorships, and posing a new set of questions: can the observed differences in performance be explained by the particular institutional arrangements that differentiate democratic systems? Can we predict which democracies will develop and which stagnate? Can we tell *ex ante* whether a dictator will turn out to be a statesman or a crook? Are there some observable features of dictatorships that allow us to predict their performance?

Yet even if our question concerns the impact of political regimes on economic development, for methodological reasons we must first examine whether economic development affects the rise and fall of political regimes. The problem faced in any analysis of an impact of institutions, policies, or programs on performance is that if some factors, observed or not by the researchers, affect both the choice of institutions and the performance, then inferences based on the observed sample are invalid. A bias may emerge if very poor countries tend to have dictatorial regimes and to stagnate (exogenous selection on observables), or if democracies are less likely than dictatorships to survive economic crises (endogenous selection on observables), or if countries endowed with enlightened leaders tend to have democratic regimes and to develop (selection on unobservables). Under such conditions, we must study the relation between institutions and performance as simultaneous, using models appropriate to the mechanisms of regime selection. Indeed, one impetus for this book is that the numerous previous studies of the relation between political regimes and economic performance all fail to consider selection bias.

This methodology underlies the organization of the book. Even though our primary question concerns the impact of democracy on development, in order to study this question we must first learn how countries happen to have particular regimes – the impact of development on democracy. Thus, part one is devoted to the analysis of the impact of development on the rise and fall of political regimes, while part two examines the impact of regimes on development.

The book is written at three levels: for those who are interested only in what the world is like, for those who want to know why we think that it is this way, and for those who wish to delve into methods of analysis. More important findings are summarized in an extensive preview that opens the book. This part is self-contained and it includes the conclusions. The main body of the book presents the arguments and the statistical analyses upon which these conclusions are based. Finally, technical materials are presented in the appendices to the particular chapters and to the book as a whole.

The analysis is based on 135 countries (we exclude six countries which derive more than one-half of their revenues from oil) observed between 1950 or the year of independence or the first year for which the data are available ("entry year") and 1990 or the last year for which data are available ("exit year"). Our unit of analysis is a particular country during a particular year, for a total of 4,318 country-years, of which economic data are typically available for 4,126 observations.

Part One: Development and Democracy

Obviously, our first task is to define democracy and dictatorship, distinguish various types of both, and classify the observed regimes into these categories. We treat as democracies regimes which hold elections in which the opposition has some chance to win and to assume office. Our treatment of dictatorships is basically residual: all regimes that fail to be qualified as democracies are considered to be dictatorships or, a term we use interchangeably, authoritarian regimes. Note that if different dictatorships succeed one another, we treat them as a continuous spell of authoritarianism.

Whatever are the peculiarities of our rules, the resulting classification differs little from alternative approaches. Hence, there is no reason to think that our results are idiosyncratic to the particular classification of regimes.

We also distinguish three types of democracies: parliamentary, mixed (semi-presidential), and presidential – as well as different types of dictatorships: mainly institutionalized ones, which we call "bureaucracies," and the non-institutionalized "autocracies."

Once the regimes have been classified, we need to understand how they come about and die. Using a dynamic probit model, we discover that democracy is more likely to survive in wealthier countries, while dictatorships endure in poor countries, are less stable in countries with middle income levels, and somewhat more stable again in wealthier countries. Perhaps the most startling discovery is that no democracy ever fell, during the period under our scrutiny or ever before, in a country with a *per capita* income higher than that of Argentina in 1976. In turn, the level of development alone does not predict a single transition to democracy. Hence, while Lipset was correct to argue that democracies are more stable in the more affluent countries, the central hypothesis of modernization theory – that development under dictatorship breeds democracy – is false. The observed cross-sectional pattern – the fact that democracies are much more frequent in wealthy countries – is thus the result of the process in which dictatorships die almost at random, but if they happen to die in a wealthier country, democracy is almost certain to survive forever. These findings offer evidence that regime selection depends on the level of economic development.

This analysis is then replicated with regard to the impact of the rates of economic growth. We find that democracies are more vulnerable to economic crises than dictatorships. Moreover, all this instability is limited to poor democracies, which are extremely brittle when they face economic crises. Hence, we find evidence for endogenous selection of regimes, conditional on economic growth.

The same analysis is then conducted with regard to different types of democracies and dictatorships. The central finding is that parliamentary democracies are much more stable than presidential ones. We wonder why so many new democracies choose presidential regimes, and find some evidence that they constitute a legacy of military dictatorships. We argue that the choice of presidentialism is due to the threat posed by the military.

We conclude this part by summarizing the observed patterns of regime selection and then speculate about mechanisms of selection which we are unable to observe: in particular, the quality of the political leadership. The models of selection summarized above are used to generate a statistical instrument for the probability of particular regimes, and this instrument is then used in the analysis of the impact of regimes on development.

Part Two: Democracy and Development

This part has a similar structure. We begin with a conceptual discussion of the dependent variable – development – and relate it to our measures of economic performance: the growth of income and of consumption, the share of physical investment in gross income, the growth of the labor force and of human capital. Then we present a general outline of the historical patterns of growth and in their light examine the economic models of growth. Our purpose at this stage is to construct a robust specification of the growth model, a specification to be used in assessing the impact of regimes. Since we are concerned that the variables used to explain growth should not be themselves endogenous to regimes, we end with a specification that is minimalist but which does include the standard panoply of production-function variables.

Using a selection-augmented model, we then proceed to examine the impact of democracy and dictatorship on economic performance. We learn that, contrary to the most cherished beliefs, these regimes invest at an almost identical rate. Indeed, in very poor countries the share of investment is higher under democracy than under dictatorship. In turn, to our surprise, we discover that the rate of growth of labor force (and of population) is much higher under dictatorship. Finally, and most importantly, we find that the selection-corrected average rates of growth of *per capita* income and of *per capita* consumption are not different under the two regimes. This finding is robust under various specifications and different estimators. It survives controls for the quality of the economic data and experiments with a different classification of regimes.

Yet to say that regimes do not differ in their average performance is not to say they do not matter. While we find that the allocative efficiency of investment is about the same under the two regimes, the elasticity of output with regard to labor, which under competitive conditions equals the labor share, is much higher in democracies. Hence, dictatorships use more labor, get less out of it, and pay it less: their growth is more labor-intensive than that of democracies.

A comparison of the performance of parliamentary and presidential democracies generates overwhelming evidence in favor of the former. We attribute this difference to the mechanisms of political accountability characteristic of these two types of democracy. We discover that the great majority of presidents who peacefully left office did so because of impending term limits, while among those who ran for re-election few were defeated. We argue that presidential regimes give an excessive advantage to incumbents – who are simultaneously heads of government, of state, and typically of the armed forces – and then counteract this advantage by constitutional rules limiting re-election, thus depriving the voters of the electoral mechanism to control chief executives.

Since we are still analyzing the impact of the types of dictatorships, it is too early to report these results. Our main question is whether the fact that a dictatorship is institutionalized – namely, that is formally organized and rules by laws – affects economic performance. Thus far our main surprise is that the survival of dictators in power is sensitive to economic performance. We argue that elections do matter under dictatorships; even though they are not a mechanism for selecting rulers, once elections are held, dictators are concerned that they demonstrate a show of strength for the regime. They care whether the turnout is 90 or 95 percent, since they fear to be overthrown if they do not show themselves to be fully in control. Whether this mechanism of accountability has consequences for economic performance, we do not yet know.

We close the analysis of the impact of regimes on development by building and testing a model in which this impact is mediated by the size of the government, specifically of the public productive expenditures. Drawing on economic models of the optimal size of government, we argue that their actual size should be too small under autocracy, too large under bureaucracy, and closer to optimal under democracy, and that the size of government should have an impact on the rates of growth. The statistical analysis is still in the process.

Conclusion

The hypothesis that emerged in the late 1950's and dominated U.S. foreign policy over two decades was that we face a trade-off between democracy and development. Dictatorships were needed to generate development. Yet the future was not so bleak for democracy, since dictatorships would self-destruct as the result of their own success. According to the dominant canon of the time, democracy would naturally emerge after a society had undergone necessary economic and social transformations.

Since in this view dictatorships generate development while development leads to democracy, the best way to democracy was seen as a circuitous one. The policy prescriptions that resulted from this mode of thinking rationalized supporting dictatorships, at least those that were "capable of change," that is, anti-communist ones.

Communism is now dead, and the idea that it ever represented the future appears ludicrous, albeit in the omniscient retrospect. Yet doubts remain. For many, Pinochet's Chile is the paradigm of successful economic reforms; the economic success of authoritarian China is the model for Russia. Even if democratic ideals nourish political forces from Argentina to Mongolia, the allure of a "strong government," "insulated from pressures," guided by technical rationality, capable of imposing order and discipline, continues to seduce. Whether in the case of the Tienanmen Square massacre or the *autogolpe* of the Peruvian President Alberto Fujimori, international financial institutions, as well as governments of developed countries, are still willing to close their eyes at violations of democratic, and even human, rights on behalf of the purported economic effectiveness of dictatorships. Thus, the question of the relative economic merits of political regimes continues to evoke political as well as intellectual passions.

Our results should dispel such doubts. True, the "tigers" tend to be dictatorships: the fastest growing country in the world in the 1950's, at least if we are to believe its own statistics, was Romania; the economic miracle of the early 1970's was the military-ruled Brazil; the heroes of the 1980's were the dictatorships of Singapore, South Korea, and Taiwan; in the 1990's, it is China. But are dictatorships the tigers? The list of economic disasters generated by authoritarianism is long and tragic. Even the economic collapse of communism pales in comparison with the destruction caused by dictatorships in many African and Latin American countries and the squandering of resources in the Middle East. Hence, we must compare the average, not the best, practice.

There is just no evidence that, on the average, dictatorships are better at generating growth than democracies. Moreover, when dictatorships generate development, they tend to do it by exploiting cheap labor. Once different types of democracy are distinguished, it becomes clear that parliamentary democracies are better at generating development than other political regimes, including presidential democracies but also different types of dictatorships.

Neither is there evidence that economic development under dictatorships breeds democracy. In those few countries that did develop under dictatorship, the regime survived well into levels of development under which other countries have long enjoyed stable democracies. The level of economic development just fails to predict transitions to democracy, while *ex post* explanations of development-driven transitions entail a logical fallacy.

Hence, to summarize these results in a positive tone, we end arguing that to get democracy we should support democracy, not dictatorship. And to the extent to which political regimes matter for economic development, it is clear that parliamentary democracies dominate all the alternatives.

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Data, Archiving and the Real World of Comparative Scholarship

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After six years of field research and writing, my new book manuscript *Identities in Formation: The Russian-Speaking Population in the Near Abroad* is being prepared for publication at Cornell University Press. This project has crystallized for me some of the issues concerning the replication norm debate as it affects the comparative politics community. As an instigator of that debate on these pages, I sought to promote a norm that was both reflective of the practical realities of our subdiscipline and the requirements of a scientific community. In this discussion, I shall review some of the practical realities, in order better to specify a reasonable norm.

I had written, in my role as president of this section, a commentary in support of the development of a strong norm on the archiving of data among comparativists in order to promote replication as a standard part of our scientific repertoire. Some of our most honored members (for example, Ian Lustick, Sidney Tarrow and Robert Putnam), in reaction, raised important points against a norm that called for the archiving of virtually all field data. They were concerned, for example, about the confidentiality of sources, and how this might be violated unwittingly by replicators. They were concerned as well for the rights of the collector of the data to the initial interpretation of it. Since, in comparative politics, the initial interpretation, if done right, may take a long time, the collector of the data can easily lose out to "quick and dirty" interpretations, whose authors preemptively exploit the data set. In response, at the general meeting of the section, I sought a compromise that would *inter alia* allow for the exception of any data set that could compromise any individual, and for the postponement of the dissemination of the data until the collector of the data had a reasonable time to publish his/her results.

At that very time, I began to face the realities of the data gold mine that I (in collaboration with Jerry Hough) collected in the former Soviet Union. The book manuscript, *Identities in Formation*, examines the political and cultural reactions of the Russian and Russian-speaking settlers that were "beached" outside of Russia proper when the Soviet Union receded. The empirical work covers four republics: Estonia, Latvia, Ukraine, and Kazakhstan. The book is data rich, and the data come from a wide variety of sources. To be specific, the book reports on:

1. Large-*n* surveys administered to Russians and Titulars (titular meaning the nationality group after which the republic was named) in all four republics. About 150 questions were asked and there were from 1,100 to 2,300 respondents, depending on the population size of the republic (and the costs of the survey).
2. The "matched-guise" experiment, well-known in socio-linguistics as an unobtrusive measure of language status. In this experiment, respondents listen to a tape recording of several voices reading the same text. Half the voices speak Russian; the other half speak the titular language. Respondents are not aware that each person speaking has read the passage in both languages, and thereby has two "guises". Respondents rank each voice on a number of attributes, and the researcher can compare scores on both guises of a single individual speaker. The matched-guise experiment was performed in several schools in each of the four republics.
3. A content analysis of newspaper articles dealing with the nationality issue in all four republics, written both in Russia and in the republics. In this analysis, the variety of terms used to identify the beached diaspora were coded on a variety of dimensions. There were from 73 to 88 articles coded from each republic, with nearly 2,000 identity terms coded in the total sample.
4. Ethnographic field notes collected in all four republics. I put together a research team in which each member spent months in the field, living with and interviewing families of both Russian and Titular nationality. The team of four (Vello Pettai and I shared Estonia; Pettai also did Latvia; Dominique Arel did Ukraine; Bhavna Dave did Kazakhstan) coordinated on a set of participant observation techniques, and extended interviews to write family biographies. These data gave me a sense of the micro elements of the Russian-speakers as they adjusted to their new roles as minorities in the new republics.
5. Official documents, newspapers, electronic news sources, and secondary sources. These materials help provide political narrative to the events that occurred in these republics, and in the world.

Of all these data, only the large-*n* surveys have been properly archived at ISR, housed at the University of Michigan, Ann Arbor. In preparing these data for archiving, I was sobered by a factor raised not fully elaborated by Lustick. This factor is time and expense. The four surveys were written originally in Russian, and local teams translated the survey into the titular language. But the surveys had organic existences in each of the republics. We found that precisely similar wording had different meanings in different republics, and to keep the meaning of the question the same, we had to change the wording. Also, with slightly different political situations, we couldn't ask the same questions in each republic. Allow me to give a simple example, partly simplified for purposes of illustration. We wanted to know whether respondents were sympathetic or not to the use of the titular language as a medium of instruction in the schools. But we wanted to make the question concrete. If the titular language was used as a medium of instruction in few schools, we were able to ask "do you think the titular language should be used as a medium of instruction in most schools?" But if it is already used as a medium of instruction in most schools, we changed the question to "Do you think it is a good thing that the titular language is the medium of instruction in most schools?" In sum, the surveys were not precisely the same.

Getting the four surveys translated properly into English, and in a way that all future researchers will immediately see the wording (or choice set) differences on each survey was a vast enterprise. Of course, I could have deposited the surveys only in Russian; but that would have constrained replication to those who read Russian or who could pay for translation. I felt this to be an unnecessary restriction for data that would have general appeal. But more than translation is involved. Since the survey teams were not always the same, in some cases different protocols were used in sampling. All of these sensitive issues were resolved as best I could and noted in a wide variety of files and notebooks. To get all this information clearly written, and for deposit into an archive, I employed two students for about five months in an exercise that had no use to me, but was essential for archiving if replication is to mean anything.

There were other tasks that had to be done to make the data set useful. I constructed several indices that played a crucial role in the specification of the variables for the book. As the project developed, and my research assistants performed tests to see if the elements of the indices were cross-pressuring one another. In some, they were. Each time I did re-codes for new specifications of indices, I had to revise the package I had prepared for the archive. Thus, the archived data were held up until the last revision was done, or else replicators would not have access to the key variables used in the book.

The NSF – which funded the surveys – demanded archiving these data. They were so important for future researchers that I had no qualms about spending the time and money to do it right. But the difference between orderliness of the data that is needed in order to write a book and orderliness that is needed for other scholars to understand the data set is enormous. NSF panels, Gary King tells me, have begun – rightly, in my judgment – to demand a proper budget for the fulfillment of that data archiving demands they set. If political scientists through their contacts helped make it a norm for all public and private granting agencies to make the same provisions, the quality of our scientific community would be enhanced.

The matched-guise and the content analysis data cry out for replication, but I have not (as yet) prepared the data for archiving. (NSF did not require the archiving of the matched-guise tests; the content analysis was supported by other sources). The likelihood, however, of replication for these extremely focused tests is quite small. The expected returns for archiving the data are therefore quite low. It follows, I think, that it would be proper to make known that any scholars who wish to replicate these data are welcome to contact the author, who will cooperate in all ways to aid the replicator understand the data set. This is comparable in biology and chemistry to inviting colleagues to one's lab in order to show how a new experimental technique is done. This kind of collegiality is far cheaper than would be fulfilling a bureaucratically-set norm requiring the public archiving of all data.

The ethnographic data are often quite sensitive. To "clean" them up would take a great amount of effort. Again, I would give access to any scholar who established his or her *bona fides* to those data, if they promised to assure confidentiality of sources. (Also, the asides by the field researchers telling their reactions are a great source of material; but it is questionable whether we should permit replicators to quote our semiprivate comments on the margins of our field notes into their publications. This makes me queasy).

Perhaps it could be argued (as Miriam Golden did in her article on replication in these pages, and more forcefully in a private communication in response to an earlier draft of this comment) that field researchers who run off into the field, do some *ad hoc* open-ended interviews, and don't enter their notes into computer files, are still living in a pre-scientific age. Once scholars have made the psychological leap into the replication age, she suggests, the whole research process will be affected, as every step in it will be open for scrutiny by those who follow the original researcher in the field. She recognizes that with this norm in place there will be increasing pressures on comparativists to assemble relatively larger, more standardized data sets. She further recognizes that this sort of rationalization of the data assembly process will work against the solitary researcher, and give us incentives to rely on data that are more easily quantified and that have built-in economies of scale. Over time, she concludes, this will change the nature of field research, and clearly downgrade traditional area studies work in the comparative field.

I have sympathy for Golden's analysis, but I am reluctant to accept it fully. If we systematically exclude the experiences many comparativists have had in the field from the enterprise called "science" – background materials picked up in places that we don't want the world knowing that we inhabited, with people who may one day be in jail – our research intuitions throughout our careers about what is really going on would be severely dulled. The advantages of keeping our two ears to the ground to catch undercurrents of political feeling – a point I emphasized in an essay I wrote in these pages on field work – would be lessened if we felt the breath of future researchers on our necks. The plausible response, that we keep two books – one cleansed, and one private notes to ourselves – would go against the spirit of the replication norm.

For small, specialized data sets such as in the matched guise and content analysis and for field notes as well, I cannot therefore recommend that researchers be required to archive their data. An alternative idea, brought to my attention by Gary King, is that instead of depositing data in some central archive, comparativists would only register the existence of the data. The description of the contents of data would be publicized and included in a data base available to all comparativists. If someone wishes access to those data, they would have to contact the researcher. That way, the comparativist who collected the data can clear confidentiality issues with people individually, and condition what they give to others on the research needs of those asking for access to the data. In recent years economic historians (as part of the Economic History Net Database Registry) have normalized this practice, and it might be a model for us.

Finally, there are the documents and secondary sources. As Lustick argued in his response to the proposed replication norm, comparativists use footnotes to aid replicators. For the secondary sources, documents, and electronic sources, I footnote sufficiently so that future researchers can check the degree to which my sources make claims that I attribute to them. I would, however, accept a rule that crucial documentation garnered in the field, but not accessible under normal conditions within the scientific community, ought to be scanned (if legally permitted), and put onto the net for use by the larger scholarly community.

In sum, the experience of writing Identity in Formation has given me a clearer idea of the type of differentiated replication norm that is appropriate for our sub-discipline. Certain kinds of data (such as large-*n* surveys) ought to be archived no matter what the cost. Of course, we must teach our funding sources that they need to fund this crucial aspect of scientific practice. Other kinds – more localized data sets – perhaps can be archived "in lab" open to other researchers, with an announcement of its nature in on-line disciplinary registries, but without needing the high up-front costs of explaining how to use the data for all potential replicators. Field notes may not be appropriate for archiving, even if they could be cheaply scanned, and it is justifiable to require restrictions to their access by other scholars. The existence of these data, however, should also be registered. Secondary sources and the like already archived in libraries require only footnotes; but it would be a useful norm requiring us to scan documents that are not accessible to other scholars.

Essential to the replication norm, and yet to be authorized by our section, is that all books and articles in comparative politics should have a replication footnote, in which the author specifies the conditions under which the data can be accessed by replicators. The justification of those choices should be assessed by peer reviewers just as they would do for the data analysis itself. Community norms as they evolve, presumably different in distinct subfields, rather than some predefined scientific standard, would then define the criteria for an acceptable replication footnote.

Book Reviews

Privatizing Russia

Maxim Boycko, Andrei Shleifer and Robert Vishny
MIT Press, Cambridge, 1995

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While numerous policy makers and scholars have explained the rather obvious economic rationale for privatizing state-owned enterprises (SOE's) in formerly communist countries, very few have successfully addressed the acute political problems associated with this process. Under what circumstances will managers, workers, and the general population embrace, or at least accept, a program of rapid privatization? In addition, what mechanisms are most effective in forcing politicians to relinquish control over SOE's? *Privatizing Russia* offers several important new insights into these questions, which have implications for the reform process not only in Russia and eastern Europe, but for a whole range of countries that are attempting to reform the state sector of their economies.

The book tells the story of Russian privatization, a process in which the authors were all key participants[1] . Their focus is on the voucher privatization program in Russia, begun in late 1992 and completed in the summer of 1994. They suggest that privatization succeeded because the primary objective of the program was to depoliticize economic activity - to sever the links between managers and politicians[2] .

The authors present a detailed explanation of how political control of economic activity in Russia (and the USSR) led to poorly defined property rights, which in turn led to poor economic performance. Politicians used political control of public enterprises to overemploy people, pay excess wages, locate factories in areas where it is inefficient to produce, etc. The first objective of privatization should be to free firms from politicians control; issues such as effective corporate governance should be dealt with later. Privatization severs the links between politicians and managers, thereby robbing politicians of control over firms. In order to accomplish this, control and cash flow rights must reside in the hands of enterprise managers and outside investors. For tactical reasons, other stakeholders in firms, namely the workers and local governments, must also retain some control rights.

They argue that a combination of corporatization (the allocation to firm managers of control rights that once belonged to politicians) and privatization (allocation of cash flow rights to outside investors) is the only politically viable strategy for creating an efficient ownership structure. Effective privatization strategies thus require the building of political coalitions that remove control rights from traditional (non-reformist) politicians. In Russia this coalition included insiders such as firms' managers and workers, and outsiders such as the general public and local governments.

The authors describe how the Russian privatization program fits this theoretical setup. Through a program of guaranteed insider rights (managers and workers were offered a controlling stake in their individual firms) and public provisions (privatization vouchers were distributed to all citizens for a nominal fee and control of much of the process was devolved to the local government level), reformist politicians were able to develop a winning coalition of managers, workers, local governments, and the general population. The privatization process is thus depicted as a political battle wherein reformist politicians try to establish a coalition that has enough political power to effectively challenge entrenched politicians.

Politicians receive tangible benefits from public ownership - they can hire political allies; elicit bribes; gain political support and votes from managers, workers and local community members; control certain prices for political ends; and so forth. Their argument for why entrenched politicians resist reform, and why firms should be *de-politicized* is thus clear. However, it appears that they are focusing primarily on national-level politicians. Would not non-reformist local politicians have the same incentives to retain control of SOE's? This local dimension of the story remains undeveloped, as local authorities are granted certain key control rights in the privatization process without mention of the possible negative ramifications.

Boycko, Shleifer, and Vishny are justly proud of the program they helped to conceive and implement. Privatization is, however, only one piece of a much larger economic reform puzzle. Private ownership of firms in no way ensures that decisions regarding critical issues such as government subsidies and investment and bankruptcy enforcement will be made in accordance with reformist economic logic. They do suggest that these and other issues such as overall restructuring of firms, corporate governance, and the legal environment are important, but do not go very far to integrate them into their story.

In the midst of a large and growing literature on economic reform in Russia and Eastern Europe, *Privatizing Russia* stands out as perhaps the best example of a melding of solid theory with descriptive empirical work. In addition to telling a fascinating story, the authors have made an important contribution to the relatively recent theoretical literature focusing on the interests of politicians in the process of enterprise reform. They have also gone a long way toward answering a number of key questions with important implications for both the practice and comparative study of economic reform.

[1] As advisors to the Russian government, they were active participants in both the development and implementation of the privatization program.

[2] This suggestion is derived from previous articles; in particular Schleifer and Vishny. 1994. "Politicians and Firms." *Quarterly Journal of Economics*, CIX: 995-1025.

Challenging the State: Crisis & Innovation in Latin America & Africa

Merilee S. Grindle
Cambridge University Press,
Cambridge, 1996

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Continuing the state-society analysis that has marked her earlier work, Merilee Grindle undertakes an ambitious and revealing cross national comparison of neoliberal market reforms and subsequent political ramifications in sixteen developing nations from Africa and Latin America, with detailed case studies of Mexico and Kenya. The literature on neoliberal reforms – economic prescriptions that drastically diminish the state's presence in the economy – typically examines the input side of the reforms, covering technical issues about the shape and scope of reform policy as well as the proper state role in building a coalition to support reform. Grindle, however, delves into the output side of the reform process to uncover how reforms affect the state's ability to establish and maintain internal and external security, raise revenue, and maintain dominance over alternative social organizations. Her discovery is twofold: first, long-standing relationships among the state, society, and the economy have been destroyed by reform; and "the medicine applied to correct economic imbalances often contributed to weakening the ability of the government to carry out and sustain needed reforms" (127).

During the 1980's, massive budget deficits, the necessity of attracting foreign investment, the increasingly transnational character of capitalism, and requirements of IMF and World Bank loan programs precipitated a move in the developing nations of Latin America and Africa from state-led development strategies to policies that placed greater emphasis on market forces to generate economic growth. Much like Samuel Huntington in *Political Order in Changing Societies*, Grindle assumes that strong institutions are necessary for determined policy implementation: "Regimes attempt to negotiate and impose formal and informal rules about how the state will relate to the economy and to society; durable and legitimate regimes have greater capacity to achieve these goals than do those that are less institutionalized"(4). To gauge state strength, Grindle employs a longitudinal study of four factors. The first, *institutional capacity*, deals with the state's power to assert primacy in setting authoritative rules governing economic and political interactions. A state with great *technical capacity* can manage effective macroeconomic policies. *Administrative capacity* allows a state to deliver basic physical and social infrastructure, a function essential to economic development and social welfare. The final factor, *political* capacity, involves the sufficiency and legitimacy of channels for societal demand making, representation, and conflict resolution. The case studies of Mexico and Kenya in particular provide a fascinating illustration of how these factors have waxed and waned. Overall, Grindle concludes that as these states have implemented neoliberal reforms, their strength has fallen, jeopardizing the long-term chances for successful development.

Why have reforms had this paradoxical effect? The answer involves the relationships that neoliberal reforms have severed. As Grindle recognizes, most Latin American and African regimes depended upon resource allocation to maintain a melange of relationships between the state and society that underpinned social stability and political support (155). Neoliberal reforms that diminish state control over economic resources have disrupted this *mélange* of relationships; thus, the four dimensions of state strength decline. Once this happens, state leaders find themselves in a situation similar to that described by Joel Migdal in *Strong Societies & Weak States* and Barbara Geddes in *Politician's Dilemma*: they must balance long-term economic reforms with the need for political stability. With state strength diminished, leaders have generally undermined their own reforms to please societal groups whose support is necessary for political stability, so the long-term chances for successful development are sacrificed for political expediency.

Despite this pessimistic assessment of the reform process, Grindle declares that all of this bodes well for more participatory politics in the largely authoritarian nations in Africa and Latin America. With falling institutional, administrative, and political capacity, states face a much more restive society that contests the basic rules of the game and demands economic redress. Such pressure from civil society often leads a state that now controls fewer economic resources to allow a political opening, essentially trading more popular control over the selection of local and national leaders and more popular input into policy for political stability. Indeed, such an opening could be the only path to successful development, for Grindle argues that fully capable states may only emerge where civil society is capable of contesting the state and its leaders, demanding greater accountability and responsiveness (194). So, though Grindle agrees with Huntington's assessment that economic development can undermine political stability, she posits an opposite remedy. More participatory political institutions, rather than institutions insulated from societal pressure as Huntington argues, are the key.

Grindle offers an innovative account of state strength, and her comparison of sixteen nations from Africa and Latin America is impressive and novel. However, there is one inconsistency in the argument. At times, Grindle asserts that strong political parties and well-institutionalized bureaucracies are needed to preserve state capacity and aid development (11). She also declares that powerful resources resulting from the structure of political institutions, such as presidentialism and centralized control in Mexico, assist development (189-93). While strong political parties and bureaucracies are consistent with the participatory politics that Grindle sees as an essential precondition for successful development, presidentialism and centralized control are not. This incongruence is puzzling in an otherwise excellent analysis.

Internationalization and Domestic Politics

Robert O. Keohane and Helen V. Milner, editors
Cambridge University Press
Cambridge, 1996

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In few issue-areas has the division between international relations and comparative politics been more apparent than in the study of political economy. While IPE specialists have examined trade, international capital flows and the like, comparativists have tended to focus on comparative national case studies of economic crisis and development. The former have generally shunned systematic domestic political analysis, while the latter frequently fail to address the international economic context.

Into this gap arrives the new edited volume by Keohane and Milner entitled *Internationalization and Domestic Politics*. Grounded in the assumption that the increasing integration of domestic and international markets are raising the domestic political costs of ignoring the international economy, the authors focus on the effects of internationalization on domestic politics. Generally speaking, they argue that this can take place in three ways: by reshaping domestic political coalitions and their policy preferences, by creating economic and political crises, and by limiting governments' capacity to manipulate macroeconomic policy. Furthermore, domestic policy responses to international economic pressures, while viewed as reactive, are linked with the partisan makeup of governments, the organization of labor and financial markets, and the nature of domestic political institutions.

The strongest feature of the volume are the three theoretical chapters by Keohane and Milner, Frieden and Rogowski, and Garrett and Lange respectively. The first provides a strong justification for analyzing the international economy and domestic politics jointly while forwarding a number of testable hypotheses regarding the international realm's impact on domestic politics. The chapter by Frieden and Rogowski begins with the assumption of economic pluralism in which international prices determine domestic political coalitions, but goes on to examine the domestic political implications of three competing theories rooted in the factor and sectoral make-up of domestic economies. Particularly successful, however, is the theoretical chapter by Garrett and Lange in which they explicitly account for the mechanisms through which internationalization impacts domestic policy outcomes by offering a model in which international economic impulses are filtered through domestic socio-economic and political institutions. Their theoretical model allows for domestic politics to influence the process linking the international and domestic, and thereby avoids the determinism of economic pluralism while introducing domestic agency as an important variable.

Somewhat less successful are the case

studies. With the exception of Garrett's chapter examining the relationship between state-labor institutions and budget deficits, the remaining chapters generally stray from the theoretical framework of the earlier chapters. Not systematically rooted in theories of either the international economy or domestic politics, the case studies tend to relay idiosyncratic accounts of the relationship between internationalization and domestic politics in the United States, Japan, the Soviet Union and China. Most interesting in these studies are the findings by Shirk and Evangelista that Soviet and Chinese institutions have been, and in the Chinese case are still able to, severely limit the influence of international economics on the shape of domestic political coalitions by blocking relative changes in international prices.

More significantly, Shirk and Evangelista's findings suggest a weakness in the internationalization hypothesis that informs the volume: namely, that the relationship between the international economy and domestic politics is unidirectional. Haggard and Maxfield note this in their chapter on LDC's in which they suggest that "the 'international' variables that are at the center of this project...are themselves partly a function of past government policy" (235). This suggests the need to further specify the domestic institutional factors that provide definition to the international/domestic nexus. Thus, while Keohane and Milner suggest that institutions matter, and Garrett and Lange identify a number of domestic factors such as regime type, the nature of representation, the number of veto points, and bureaucratic independence as relevant in differentiating government behavior across nations, these factors are never operationalized and remain theoretically marginal to the project as a whole.

With their volume, Keohane and Milner have made impressive progress in unifying the study of political economy. It remains for future research to provide systematic comparisons of the role of domestic political institutions in the relationship between governmental policy and the international economy, and thereby to integrate more comprehensive theories of political economy.

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