APSA~CP

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Letter from the President

Area Studies and the Discipline

Robert H. Bates
Harvard University

A revolution has taken place in the relationship between area studies and the disciplines. It is symbolized most clearly, perhaps, in the decision by the Social Science Research Council to abolish area committees. But the SSRC, to a great extent, follows the preferences of the foundations. The foundations, for their part, often re-echo the opinion of the academy, And within the academy, the consensus has formed that area studies has failed to generate scientific knowledge.

Background

Many see area specialists as having defected from the social sciences into the camp of the humanists. Their commitment to the study of history, languages, and culture, as well as their engagement with interpretivist approaches to scholarship, signal this defection. This perception is shared by many political scientists. Rare is the political science department wherein those who study Europe, South Asia, Africa, etc. do not reside within area studies programs. Rare too is the department wherein the area specialists fail to constitute a center of resistance to new trends in the discipline. They tend to lag behind others in terms of their knowledge of statistics, their commitment to theory, and their familiarity with mathematical approaches to the study of politics. They often oppose the appointments of those who have trained in such areas but who may be deficient in language skills. They raise principled objections to innovations in political science, while lacking the training fully to understand them.

Area studies centers possess clients besides the social sciences, including departments in the humanities and "lay" groups within the student body and community. Multiple constituencies create the opportunity for wily directors to become independent of departmental chairs. The result is the application of criteria other than disciplinary standards to appointments, promotions, and course offerings. The tensions between area studies and the discipline therefore grow.

It would be naive to attribute the decline of area studies to political forces emanating from within the university, however. Budgetary retrenchment has focused on programs created to fight the cold war; international studies stand amongst them. As the grounds for international competition shifts from military to economic competition, corporate America has focused on primary and secondary schools, which are weak by international standards, rather than universities, which are not. The end of the cold war has bred widespread insularity. The battles on university campuses thus may account for why political and economic elites have been able to cripple area programs, rather than why they should want to do so. Greater forces, outside the university, generate the reasons for that.

How to Respond

I have long regarded area programs as a problem for political science. I have opposed their resistance to the search for theory and to the use of rigorous methods for evaluating arguments. Nonetheless, I do not welcome their demise. For I do not regard area studies as an intellectual rival; rather, I regard it as a necessary complement to

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the social sciences. Social scientists will be the weaker, the weaker our colleagues in area studies are.

One of my predecessors argued for a reconstruction of the relationship between area specialists and the discipline, arguing that area specialists would play the role that language specialists play vis-a-vis linguists; it is the linguists, of course, who would occupy the position of the scientist in that relationship. A second saw area specialists as fulfilling the role of, say, historians; indeed, he expected them to migrate to history departments, where they would record the data from which political inferences would be drawn by social scientists residing in political science departments. I, however, favor neither separation nor segregation. Rather, I favor mutual infusion. For the weakening of area studies is taking place just when our discipline is becoming equipped to handle area knowledge in rigorous ways.

Cultures are distinguished by their distinctive institutions. One of the major innovations in our discipline has been the creation of the tools with which to analyze institutions. Developed for the study of politics in the United States, these tools are increasingly being applied elsewhere. Cultures are marked by distinctive histories and understandings; these lead to distinctive ways of reacting to events and of interacting with others. The theory of games of imperfect information, newly ascendant in political science, can be used to explore the way in which history shapes political beliefs and the ways in which subjective beliefs affect choices and behavior.

Area specialists tend to focus on the distinctive and unique; this tendency has long frustrated social scientists, who search for broader regularities. But recent efforts to create analytic narratives, based on games in extended form, promise progress in finding rigorous ways to study unique events. This work suggests ways in which theory can impart greater leverage to empirical methods. Even in the absence of large N's, we therefore can find ways of explaining events that may be unique or rare, in ways that are subject to falsification.

The use of such methods requires precisely the kinds of data gathered by ethnographers, historians, and students of culture. It requires knowledge of sequence, perceptions, beliefs, expectations, and understandings. The tools cannot be applied in the absence of *verstehen.*

Somehow, in the United States, we have come to pose the "quantitative" against the "qualitative." This tendency is not universal; it is the result of factors particular to our culture. In recent years, I have been working in Colombia. There I have encountered an elite culture in which someone could be both an economist and a poet, Not being "hard-wired," the division between "the scientific" and "the humanistic" can be transcended. The issue is not whether to use the left side of the brain rather than the right. It is, rather, how to employ both. The blend will produce explanations of patterns, "facts," and phenomena, long known by the area specialists to be important and true. It will isolate the mechanisms that make cultural forces count in political life. It will help to account for the power of forces that we know shape human behavior, in ways that we have hitherto been able to describe but not to explain. It will lead to scientific progress.

The debate between area studies and the social sciences has been cast in confrontational terms. By pitting area specialists against social scientists, it has reanimated divisions that go back at least to the "two cultures" controversy initiated by C. P. Snow. C. P. Snow, it will be recalled, was alarmed by the division. So too should we be, especially as the separation between area studies and the social sciences takes place just when our discipline is becoming equipped to handle area knowledge in a rigorous fashion. The reluctance to invest in area studies represents a loss to the social sciences, as well as to the academy.

NEWSLETTER STAFF:

NEH SUMMER SEMINAR

"New Departures in the Comparative Study of Revolution"
Cornell University
June 15 - August 6, 1996
Sidney Tarrow, Director

Applications are invited for an eight-week seminar on new and renewed approaches to the comparative study of revolution. Until the 1990s, studies of revolution were dominated by the comparative/structural approach, which derived its generalizations from a relatively small number of "great" revolutions. Focusing on societies with large agrarian populations and centralized bureaucratic states, these studies underplayed both the political process of revolutions and the cultural themes and concrete actors surrounding them. Since then, a host of new approaches emphasizing culture, gender, social movements and the political process have appeared, and a number of new revolutions and archives on older ones have led researchers to seek new models and interpretations. This summer seminar will focus on three main goals: first; expanding the population of cases from the classical "great" revolutions; second, applying a political process model drawn from social movement research to revolutionary periods; and, third, examining the cultural aspects of revolution, including the role of gender, ethnicity and religion.

For further information, write or E - mail :

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As a service to the comparative community, the *Newsletter* would like to feature occasional review articles of undergraduate textbooks in different areas of comparative politics. Experienced instructors interested in writing such a review should contact the editor.

Luebbert Committee Seeks Nominations

The Luebbert Committee (James Alt, Ruth Berins Collier, Barry Weingast) solicit nominations for the annual best book and best article award in the field of comparative politics. While there are no restrictions on eligibility of authors or subjects, the Committee wishes to make known their strong preference for articles and books which are explicitly comparative, that is, which consider more than one case. Nominations are welcomed for books or articles published in 1994 or 1995. Please forward nominations to James Alt by email at jalt@latte.harvard.edu, or by ordinary mail to Department of Government, Littauer M-27, Harvard University, Cambridge, MA 02 13 8.

ESRC Data Archive

The Economic and Social Research Council (ESRC) publishes the *ESRC Data Archive Bulletin* in January, May and September each year. It contains news about the Data Archive at the University of Essex, other data archives and data organisations world-wide, and information of interest to users of computers for social and historical research.

New acquisitions in the latest issue of the bulletin include two data sets from Professor Cheryl Schonhardt-Bailey (London School of Economics): *British Parliamentary Divisions on Repeal of the Corn Laws, Including M.P. Party Affiliation and Constituency Characteristics, 1832-1846* (032 76); and *Distributions of Individuals by Type of Occupation in 54 Cities in Britain (03277).* These data will also be available from ICPSR. To contact the ESRC:

ESRC Data Archive
University of Essex
Colchester CO4 3SQ
United Kingdom
Tel: 44 (0) 1206 872001
Fax: 44 (0) 1206 872003
E-mail: archive@essex.ac.uk
URL: http://dawww.essex.ac.uk

News and Notes

The Peder Sather Symposium

March 2 l-22, 1996
University of California, Berkeley
Clark Kerr Campus

On a biennial basis, the Center for Western European Studies at the University of California, Berkeley sponsors an interdisciplinary, two-day conference on issues of immediate concern to Scandinavia and the United States. This year's Peder Sather Symposium will be entitled, "Challenges to Labor: Integration, Employment and Bargaining in Scandinavia and the United States." The two-day symposium brings scholars and policy-makers from across the globe to the Berkeley campus. The event will yield a series of working papers that present, in-depth, the issues discussed during the sessions.

The initiative and funding for the first Peder Sather Symposium in 1991 came from the Norwegian government to promote the understanding of current issues relating to Norway. Since then, the sponsorship of the Symposium has been expanded to include the participation of the Swedish government.

Among the speakers we expect to attend this year's symposium are such outstanding scholars and policy-makers as Robert Reich, U.S. Secretary of Labor; Erik Orskaug, State Secretary of Norway; Dr. Stein Kuhnle; Prof. Richard Hyman; Prof. Douglas Hibbs; Dr. Odd Aukrust, former Head of the Norwegian Statistical Bureau; and Prof. Robert Flanagan.

For more information on the Peder Sather Symposium, please contact Linda Cathryn Everstz-Eriksson at the Center for Western European Studies, University of California, Berkeley: phone: 5 1 0/ 642-5 157. fax 510/643-5996 or e-mail: leverstz@garnet.berkeley.edu.

Research Network for Comparative Research on Europe (RENCORE)

Following a successful meeting of a working group at the European Sociological Association's Budapest Conference in September 1995, it has been resolved to establish under the auspices of the ESA a network on methods of comparative research on Europe. The aim of the network is to encourage and enhance comparative empirical research of individual, national and institutional level data from the states of Western, Central and Eastern Europe.

This aim will be met through the following objectives:

1. The support and promotion of cross-national European research, both quantitative and qualitative.
2. The development of comparable indicators for comparative research.
3. The enhancement of information exchange between those who create and use cross-national data sets.
4. The refinement of methods for the analysis of data obtained from a number of European countries.

The network will act as a forum and channel for discussion and communication between those involved in cross-national European research, either as data collectors or as data analysts. The activities of the network will include:

a. The organisation of meetings and workshops on topics related to comparative empirical research. These will be held about once a year. The first workshop will be on the subject of "Asking questions across Europe" and will be concerned with formulating survey questions to yield answers which permit cross-national comparisons. It is proposed to hold this first meeting in October 1996. The second meeting will be held at the 1997 European Sociological Association's conference (venue yet to be decided).

b. The establishment of an E-mail discussion list and WWW page. The E-mail list will enable discussions between those in the network, and the WWW page will describe the network members, their interests and their research activities.

c. The organisation of scientific visits between members of the network. A directory of opportunities for research visits (e.g., for sabbaticals, post-doctoral studies, exchanges and short visits) will be compiled. The directory will also list external sources of funding to support visits.

Administration

The network will be administered by a small Executive Committee who will stand for election every two years (at the ESA meeting). The network will be established by a small ad-hoc Committee consisting of Loek Halman (Netherlands), Peter Mohler (Germany), and Nigel Gilbert (UK). Membership in the network will initially be free, although a subscription may be levied once the network is well established. Potential members should write (or e-mail) their application for membership to Nigel Gilbert, at the address below, explaining their involvement in comparative European research and listing relevant publications. Applications should include a full mailing address, and E-mail address, and telephone and fax numbers. All those who are engaged in European comparative research, wherever they may be working are welcome to apply for membership in the Network.

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The Replication Debate: Introduction

Robert H. Bates
Harvard University

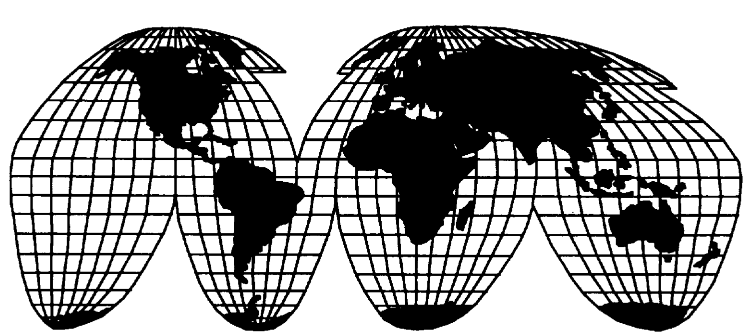
Gary King, a noted methodologist, has contributed to a variety of subfields in our discipline. Among them, of course, is comparative politics, where his famous *Designing Social Inquiry,*[1] coauthored with Sidney Verba and Robert Keohane, represents a major contribution. In a series of public presentations — some in print, others at meetings — King has been advocating the norm of replication in political research. Many of us have been surprised by the controversy his proposal has sparked in our corner of the discipline. It has been particularly disturbing to those of us committed to rendering comparative politics a social scientific enterprise. The controversy, however, should inform, rather than alarm. For it illuminates the refractory properties of comparative political research which render elementary scientific methods difficult to employ.

With the backing of David Laitin, my predecessor, King delivered his proposal to an open meeting of the section on comparative politics. His suggestion initially appeared uncontroversial. We are political scientists after all; and surely anyone committed to the scientific method would support the norm of replication in research! But then a reaction began to build, not based on principle, by and large, but on practicality. The debate began at the section meetings. It now spreads to the pages of this Newsletter, where some of the leading social scientists in our field underscore the conflicts and trade-offs that underlie the attainment of King's objectives.

When entering the field of comparative politics, I was struck by the disparity between its pretensions and achievements as a social science. I remain dismayed by the magnitude of that disparity. When a neophyte, and therefore arrogant, I blamed the shortfall on the inadequacies of my predecessors. But I now recognize that much of the difficulty originates in the materials we work with and the tasks we must perform in our efforts to generate knowledge. For reasons outlined in these contributions, the scientific study of comparative politics remains difficult to achieve. The fate of King's norm illuminates much about our field and our ability to contribute to the social sciences.

This debate will continue. After listening to the reactions that these contributions should inspire, we will have a chance once again to discuss them. They will be on the agenda of the fall meeting. I look forward to those discussions.

[1] Gary King, Robert Keohane, and Sidney Verba, *Designing Social Inquiry* (Princeton: Princeton University Press, 1994).



Read My Footnotes

Ian S. Lustick
University of Pennsylvania

It doesn't take a very close reading of the advice of the replication advocates to realize that their concerns are first and foremost with quantitative research. Despite Gary King's own efforts to argue that the inferential and procedural norms governing quantitative and qualitative research are identical' and his overall view that high-quality qualitative and quantitative studies are of equal professional merit, his articles on replication, and those of Miriam Golden and others, tend to treat qualitative research as a residual category'. Repeatedly readers are given the impression that these advocates of replication consider qualitative researchers to be substantially behind their quantitatively-oriented colleagues in their awareness of the need for replication and in the development of practices that could make it possible.

As they say in one political arena that I study most closely (Israel), *"Hahefech hu hanachon!"* "The opposite is the case!" Qualitative political scientists should definitely applaud their quantitatively-oriented colleagues for trying to follow through on the comparative advantage of quantitative methodologies--the opportunity to assess systematically the degree of corroboration available for hypotheses by aggregating and evaluating countable observations. But when qualitatively-oriented researchers are advised to invest heavily in such things as transcribing and scanning field notes, interview notes, or notes on archival and secondary source literature in order to deposit this material in centrally accessible archives, we must reject such advice as monumentally wasteful; as discriminatory; as reflecting only cursory consideration of the character of qualitative research; and as based on a fundamentally disputable premise about the intellectual returns to different kinds of scholarly activity in political science.

If replication is, as Gary King stipulates, the reproduction of reported inferences from the data from which they were putatively inferred, then the heart and soul of replication for qualitative researchers is the footnote. Footnotes that specify the exact location in books, articles, archives, or even personal files, where quotations can be checked, data documented, characterizations compared to that which is characterized, etc., are the primary, and very effective, way that qualitative researchers have always made their work open to "replication." King suggests that scholarly C.V.'s should list archives into which replication data have been deposited[3]. Goodness, does this mean I should list on my C.V. that my books and articles include footnotes to books and journals located in libraries? The fact is, of course, that the absence of footnotes has long and properly been a *prima facie* basis for the rejection of any manuscript submitted to a scholarly press or any paper submitted to a scholarly journal.

There are at least three delicious ironies about the apparent invisibility of footnotes as a sophisticated and effective form of replication (invisible, that is, to most of those in *PS* and in this *Newsletter* who have been promoting the adoption of replication standards by qualitative researchers). First, these writers themselves use footnotes in their own articles to refer to exact places where the opinions they cite can be checked against their claims. Second, a "replication footnote" (i.e., a technique for making replication of results possible) is exactly one of their most important recommendations. Third, trends in journals to adopt a mode of citation associated with the natural (quantitatively-oriented) sciences, requiring only parenthetical references to works listed in a bibliography rather than discursive commentary and exact page citations, encourage abuse of sources and discourage replication by those who might otherwise be inclined to check claims about what sources say against what those sources actually say.

Gary King's claim about the most important task performed by political science, qua science, is that it "produce(s) much reliable knowledge about the political world."[4] If this is what one believes, then investing more

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The Replication Standard in Extreme Circumstances: Field Research in Comparative Politics'

Melanie Manion
University of Rochester

The collection of qualitative data through field research in comparative politics surely differs from quantitative data collection, especially in other subfields of political science. To what extent are those differences relevant to the elaboration of common standards of methodological self-consciousness, methodological transparency, and data sharing? This commentary joins the recent debate on the "replication standard" by considering the applicability of the standard to field research in comparative politics.

My main point is that field research in comparative politics distinguishes itself in ways that mostly produce not essentially different versions – but only more extreme versions – of precisely the sorts of issues aired in the debate, which focused nearly exclusively on quantitative research. My own field research experience is on mainland China, a research context that frames those issues in particularly extreme ways. If the case for common standards can be made with the circumstances of field research in Chinese politics as the vantage point, it can probably be sustained for any empirical research in political science.

Characteristic Features of Field Research in Comparative Politics

Reliable data from which valid inferences can reasonably be made are the scarcest resource in comparative politics. The paucity of such data, rather than any shortcomings in theoretical or analytical competence among comparativists, is the most serious weakness of the subfield. For qualitative data, the scarcity is due to the high costs of data collection through field research conducted in a foreign country. Typically, data collection consumes much more time and effort than data analysis. Researchers travel long distances and conduct their research in a foreign language. Field research can entail physical hardship and perhaps physical risk too. In no country is the environment for research on politics, whether among political elites or ordinary citizens, as open as in the United States. Finally, the researcher usually conducts her field research alone, rather than as part of a collaborative effort or as director of a team of interviewers.

Reliable data on Chinese politics are particularly scarce. Field research requires an unusually large investment, not only for the obvious reasons that the language is difficult and the country distant. Chinese officials often view with suspicion the collection of political data by Americans, who are usually funded by the American government. Cooperation in private interviews can be politically risky to ordinary citizens, with the result that researchers rarely interview without some self-selection. Ultimately, success is in no small degree an arbitrary outcome: the climate for research on politics can deteriorate suddenly, as a result of shifts in elite politics. Such unpredictable shifts can effectively put an end to data collection, with the possible exception of archival research.

Implications for the Replication Standard

The characteristic features noted above have two key implications for the applicability of the replication standard to field research in comparative politics. They pertain to the controversial requirement that data be publicly available (King 1995) as well as the unexceptionable basic requirement of methodological transparency implied by the standard.

On data sharing, field research in comparative politics raises in more acute form the different perspectives debated on the relative importance of advancing knowledge through verification and respecting proprietary rights to collected data. One argument against data sharing is that there will be significantly less data collection in political science and more verification and secondary analysis of publicly available data (see Herrnson 1995). As data are the scarcest resource in comparative politics, the adoption of systematic disincentives to collect data are a serious threat to the advancement of knowledge. Whether or not more data sharing will indeed result in less data collection has not been established, however (see Stone 1995). Moreover, if qualitative field

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Comparative Politics and the Replication Controversy

Barry Ames
Washington University, St. Louis

The replication controversy begun in *PS* raises important questions about the scientific quality of our work as comparativists. Should we adopt, as Gary King suggests, mandatory deposit of replication data sets as a prerequisite for publication? King has put a lot of thought into the case, and his argument has provoked serious discussion in *PS*, but I think he is wrong. We ought to promote reciprocal, nonexploitative exchanges of quantitative data, and we do need higher standards in citations of dates, places and sources of funding for qualitative field work. But a universal replication standard, either for quantitative or qualitative data sets, is the wrong way to achieve these aims. Such a standard, moreover, would harm primary researchers, just the people we need to encourage.

How does the debate over replication affect comparativists? It is curious, really, that in a discussion of replication, an issue supposedly on the cutting edge of methodological controversy, comparativists have to define themselves so anachronistically, that is, as "students-of-politics-outside-the-U.S." For many purposes this definition is quite appropriate; here it misleads. The replication controversy, at least in terms of the debate in *PS* and in my own experience, really involves two issues: (1) the mix of qualitative vs. quantitative data marshalled in a particular piece of research, and (2) the persona1 investment of the researcher in the gathering of data, whether such data are quantitative or qualitative. The work of comparativists (in the "outside-the-US" sense) generally includes a higher component of qualitative evidence, even in essentially quantitative pieces, and comparativists expend much more effort in data collection than the typical Americanist. There are many exceptions, of course, but the exceptions mostly involve non-comparativists utilizing qualitative techniques or gathering new data. Rarely do comparativists, particularly those working in less developed countries, reanalyze data sets of the National Election Study type, and rarely do comparativists make arguments based purely on quantitative data or forma1 theory.

Before we can understand the relevance of these two dimensions, i.e., the qualitative-quantitative mix and the persona1 investment in data collection, we need to agree on the purpose of a replication standard. Gary King ultimately concurs with Paul Herrnson that verification (rerunning an analysis to duplicate the original results) is not an important form of scientific endeavor, though it might have pedagogical benefits. Rather, King says (1995, 494), duplicating past research is often a "necessary step to building on prior research." If an analysis is based on NES-type data, King's position is reasonable. Building on such research usually involves a mode1 manipulating the original data, but with new aggregations, measures, or techniques. The first researcher has invested nothing in gathering the data, and the data were originally collected with the expectation of wide dissemination.

Comparative research is different. The data sets of most comparativists are constructed, often at enormous persona1 cost, by the same scholar who ultimately publishes research based on the data. If a replication standard means that other scholars have access only to the variables and observations used in the published article, replicators will be able to do little more than verify in the narrowest sense the original results. As King admits, this is not worth much. For "building on prior research" to have the same meaning as in the NES example, the entire data set would have to be made available, precisely because comparative data sets are so distinct from each other. Any scholar, as a result, would have the right to mine data gathered by primary researchers without incurring anything close to equivalent costs. Such a standard would discourage the collection of original data. It would also encourage researchers to hold their research for book publication and avoid publishing preliminary results in journals. Both effects would be serious roadblocks in the progress of comparative politics.

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Replication in the Democracy & Local Governance Research Program

Henry Teune
University of Pennsylvania

Replication of research is an "in principle" criterion of scientific knowledge. The procedures for approximating it, of course, are soft and matters of judgment of others. It can be achieved by providing "research protocols," such that someone else can do the same research and "in principle" come up with identical findings. Differences between the "doer" and the "replicator" should be easily accounted for. If not, then rule of "triangulation," a third try, kicks in.

Strict replication of the analyses of data should ferret out error but cannot help much with bad design or bad data. The more general criteria of "reliability" and "validity" for making knowledge robust dominate concerns with replication of data made available by researchers. The classic, once much cited paper in political science, by D.T. Campbell and D. W. Fiske, "Convergent and Discriminant Validation by the Multitrait-Multimethod Matrix" (Psychological Bulletin, 56 (1959), 81- 105) provides principles that should inform the conduct of empirical research. It has done so in the Democracy and Local Governance research program.

A sketch of the dimensions and processes of the Democracy and Local Governance research program will be presented along with some steps taken to assure its credibility. It is currently funded in part by the National Science Foundation (Grant no. SBR94-23801). It received support from the U.S. Institute of Peace and a variety of other foundations, universities, and institutions for various countries and parts of the research. (Contact author for more complete list.)

Democracy and Local Governance: The Problems

This research now includes interviews (about l-2 hours, face-to-face in most of the countries) of about 12,000 local political leaders (mayors, deputy mayors, and major administrators; local council people; and political party leaders, if identifiable) in about 800 localities ranging in population size from 25,000 to 250,000 (adjusted upward or downward, depending on the level and kind of urbanization in a country) between the spring of 1991 and continuing today. Data for 20 countries are in an international file; they are or soon will be for a second point in time (1995) for eight former communist countries; and are being collected or organized now in two countries. Several other countries are in the process of starting the research in 1996. Countries are the starting points; localities within a certain population range are the universe for the sample; political leaders within them are targeted for interviews.

Our objective is to examine the relationship between democratic values of local political leaders, the "globalization" of localities, and the local political orientation of leaders. The data do not contradict that relationship for local leaders, no matter what country they are from (all leaders pooled in analysis): those who are more democratic in their values see their localities as more involved with "foreign" trade, immigrants, workers, television, pollution; and they are more oriented to local problems and solutions. These relationships, however, are complicated by the contexts of countries, regions, localities, time, as well as individual differences among leaders.

We have been especially sensitive about making the data and the statements based on them credible. We moved quickly into former communist countries in 199 1, where this kind of research was unfamiliar, the collaborators and their associates variously motivated under conditions of uncertainty, and where telling the truth or saying what one believed was not among the habits of political leaders, or perceived so by western social scientists.

The main initiators, however, had experience dating from the mid-1960s in the International Studies of Values in Politics. The basic sample frame was used, and shown to be exceptionally efficient for getting a national sample; most of the value scale items and many other questions had been used over the years and had proven robust (and have served well in the 1990s) and can be used for cross-time comparisons in some countries; and we were confident that we

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(Lustick, continued from page 6)
resources in refining and checking accumulated claims makes some sense. But if one believes, at least about-the kinds of problems one is interested in, that theoretical advances deliver more scientific bang for the buck (as must be the case, in different parts of any scientific discipline, at any given time), then it is just bad scientific judgment to, as it were, and to take an extreme example, improve the measurement of the lead one is pouring into the gold-making machine. My point here is not that we lack theories that tell us about the political world, but that advice to shift resources toward replication and away from theory building and paradigm testing must not be given generally, but must be based on analysis of the particular problems one is trying to solve about the political world, and the satisfactoriness of the theories one has presently at hand.

As do thousands of other political scientists, I have dozens of boxes of notes in my basement. These notes were the basis for four books. In the twenty-two years since I began taking those notes, I have had no more than five (enthusiastically responded to) requests to see documents or notations in my files. Even if I would take the advice that Gary King gives to quantitatively-oriented scholars, "to provide only the data actually used in the publication"[5] (which itself is extremely suspect from the point of view of the footnoting tradition of qualitative political science, not to say very likely to encourage false positives at the expense of not detecting false negatives), I can still not imagine that the time and energy involved in scanning, reproducing, and depositing those thousands of pages of notes would be as well spent as developing new ideas with theoretical potential, taking more notes on more materials, or participating in this vigorous, important, and enlightening debate.

[1]Gary King, Robert 0. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1993)

[2]I am, of course, not only responding here to Miriam Golden's gently offered advice in the Summer 1995 issue of the ***CP-Newsletter***, but to the raft of comments included as responses to Gary King's article "Replication, Replication" in the September 1995 issue of PS.

[3]Gary King, "Replication, Replication," PS, Vol. 28, no. 3 (September 1995) p. 447.

[4]Gary King, "A Revised Proposal, Proposal," *op. cit.*, p. 495.

[5]King, "Replication, Replication," *op. cit.*, p. 449.

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research and quantitative research differ in any important respect relevant to the replication standard, it is on the issue of verification. It seems to me that qualitative field research results are much less likely than quantitative results to be subjected to verification by others, even if both types of data are publicly available. Rewards for confirmation of quantitative or qualitative results using the same data set are trivial (see Portis and Bond 1995). When data are qualitative, rejection of results does not turn on matters of coding or statistical technique. Unless the field researcher has been obviously careless, the data she makes publicly available will probably not be used by others to reject (or confirm) her findings. They may be used in secondary analyses. More likely, to the extent that they are used by others at all, they will be combined with other sorts of data – for real replication perhaps, with quantitative data collected to test the results obtained through qualitative research. This benefits the individual field researcher, the comparative politics subfield, and the discipline as a whole.

Of course, because scarcity creates value, it is vital to recognize the proprietary rights of field researchers to data they have invested so much to obtain. That does not imply acknowledgment of a right to hoard data. Publication of research findings should carry with it a professional obligation to make publicly available the subset of data (i.e., relevant field notes and interview notes) on which the findings rest. With the incentive structure currently in place, however, it is not surprising that more political scientists do not fulfill that obligation unless required by granting organizations. The discipline does not adequately reward the collection of original data, except to the extent that it rewards published analysis of those data.

Data sharing is directly relevant to a more fundamental and less controversial requirement of the replication standard: methodological transparency. Unless the logic and practice underlying the process of data collection are plainly evident, no one but the researcher who collected the data can adequately evaluate the findings. No conclusions can be reached as to the status of those findings as a contribution to knowledge, even to purely descriptive and very particular knowledge. Unfortunately, field research violations of the basic requirement of methodological transparency implied by the replication standard appear to be routinely tolerated in comparative politics (see Golden 1995). It is particularly unfortunate as consumers of published work are often completely unfamiliar with the context in which the data were collected and, therefore, in need of even more information on which to base their evaluation.

Making available to others the data on which an analysis is based represents the highest form of methodological transparency. Obviously, field researchers must take steps to protect the confidentiality of sources when they have assured them of it. But I can think of few circumstances in which minor alterations to notes would not provide anonymity to specific interview subjects. In such cases, a general note about the nature of alterations could be provided with the archived data.

Methodological transparency is important for another reason too: it promotes methodological self-consciousness. The lack of transparency cited by Golden (1995) by no means necessarily implicates the comparative politics subfield in carelessness in the conduct of field research. But field research conducted in foreign countries is typically a solitary enterprise and often requires compromises and changes in original research designs in the course of data collection, to adjust for unexpected problems and take advantage of
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unexpected opportunities. That means it is particularly important that the field researcher make the effort to keep herself "on track." If the methodological choices cannot be rationalized to consumers of published work, they are probably not good choices. Methodological transparency encourages field researchers to engage in more rigorous reflection about the craft of their research.

Some Modest Suggestions for Implementation

The arguments above support an active role for APSA in adopting standards and creating incentives to promote norms of methodological transparency and data sharing in the discipline. First, as Box-Steffensmeier and Tate (1995) suggest, APSA should formalize those norms in a voluntary code of professional ethics and, in so doing, give them greater clarity, salience, and status. Second, APSA should encourage and reward data collection by promoting with those norms a standard method of citation of original data used in secondary analyses. That citation should ensure that the data archived *is* indexed in the Social Science Citation Index. It should give prominence to data collection, by citing the researchers who collected the data under the author of the article. Third, APSA should establish a new award, presented at the annual conference, to confer recognition and honor on a political scientist or group of political scientists for an exceptional contribution of original data to a public archive.

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What about qualitative work? Remember that most comparative research combines quantitative and qualitative methods. As a result, true verification of a researcher's ultimate conclusions, not just checking statistical findings, will often require deposit of both kinds of data. But let us assume, for the moment, that a piece utilizes only qualitative data. Miriam Golden, in the *PS* controversy, proposes that qualitative data be subject to the same archival standards as quantitative data. I cannot imagine an administratively workable version of such a rule. In my own current research, for example, I often interview deputies and ministers. I conduct the interviews in Portuguese, with no tape recorder and usually with a promise of anonymity. Without information identifying the informant, the interview is useless to anyone seeking to replicate my conclusions. With identifying data, anonymity is broken. But even if another scholar were granted full access, I cannot see anything useful that could be done with my notes. My inferences from these notes are nearly always consequences not just of the interviews themselves but of the whole context of the study I have undertaken. Of course this is not always true: sometimes I get some wonderful quotation or statement out of an interview. In those cases, if someone doubts that the interviewee really made the statement attributed, I can produce the original notes. What will another scholar do with this information? Return to the field site and seek out the interviewee to check the quotation? After all, if I want to fake an interview, I can easily fake the field notes and cover myself with an anonymity claim. Scientific progress requires openness and disclosure, but it also requires trust.

I agree that authors should be required to provide details on dates and location of field research, number of persons interviewed, and selection criteria used for respondents. But what will be done with this information? Golden's response (1995, 482) is that "we want some assurance that he or she asked the right people the right questions." If a researcher has administered a closed-ended questionnaire, Golden's position is understandable, because in that case the field data are more nearly equivalent to quantitative data. But if an interview is open-ended (and this is really what qualitative usually means), not even the researchers themselves know if exactly the right questions were asked. Golden has the right to ask for assurance that "any one of us could go to the same foreign land and ask the same kinds of people the same kinds of questions and interpret the results in the same way" (p. 482), but field notes really will not provide that assurance.

The farther is a data base from being purely quantitative and machine readable, the more we judge its rigor not by individual interview questions or responses but by the author's ultimate use of the data. In other words, each time we read a scholarly piece, we assess the clarity of the methodology. Each case is judged independently, although with the same essential standards. If an article tries to show, for example, that politicians in a certain context will behave in a particular way, readers assess the appropriateness of the sample of politicians, the indicators of behavior, the likelihood of truthful responses in the particular interview situation, and so on. Field notes, by themselves, will not contribute much to that assessment, because the researcher's claim is not based on a simple aggregation of the individual interviews.

What we are really talking about here is verification rather than replicability. If we have good reason to doubt an author's claims, we go out into the field and attempt to replicate the results with new data. I doubt that it would be possible, except in a very small number of cases, to use field notes to verify qualitatively based claims. My guess is that the attempt would almost never be made. If a replication standard

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existed for qualitative data, I think we would spend a lot of time preparing field notes that no one would ever use.

Our problem is not that too much unreliable research gets published. In fact, if we want to improve the quality of research. we ought to have higher journal acceptance rates and additional publication outlets. Science develops most efficiently not by establishing methodological guardians at the gates of journals but by letting the profession at large (i.e., readers) separate the wheat from the chaff. Just as William McPhee (1963) showed many years ago in the case of mass and pop culture, the simple process of continually reevaluating the value of everything on the market is the most efficient way to upgrade overall quality.

If it ain't broke . . . From my perspective as a Latin Americanist, the state of comparative politics looks pretty good. Latin American political science, at least, is undergoing a renaissance. The return of competitive politics has renewed interest in parties, public opinion, elections, and legislative behavior; the stuff. in other words, of modem political science. Everywhere Latin Americanists, many from the region itself, are better trained. New theoretical approaches, including social choice theory, are no longer viewed purely as right-wing invasions from the North. The issue is no longer "quantitative versus qualitative" but rigor versus sloppiness. In this case it appears that rigor (i.e., science) is already the clear winner. Now what we need to do is follow Paul Sniderman's advice (1995, 465), encouraging "imagination, originality, creativity, seeing what others not only failed to see but did not even suspect."

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knew how to do organized collaborative, cross-national research.

The decision was made to undertake this research in June 1990. By March of 199 1, we had a sample for three countries and a questionnaire; by June, we completed the data collection and coding for Poland, Slovenia, and Sweden. That was possible because we had done research in these countries before (Poland beginning in 1966; Slovenia, as one of three Republics of Yugoslavia, in 1966; and Sweden in 1984). We were able to analyze the data and develop some confidence that we could get scales and theoretically interesting cross-national comparisons that would not violate general expectations (e.g., that Swedish leaders were overwhelmingly more democratic than either Polish or Slovenian leaders, the latter being more nationally oriented in face of a crises of independence than either the Swedes of Poles, etc.). We obtained at least "face" validity.

Comparative, cross-national or cross-cultural, research generates confounding effects. The most obvious of these is comparing terms for concepts, such as authority, leadership, honesty, across different languages and cultures. Then the observers for obvious reasons are coterminous with the "systems" or countries and communities being observed (ideally they should be randomly distributed among the respondents without regard to country or locality). Then there are honest errors, often stubbornly defended by some collaborating researchers; practical, but problematic adjustments in samples, depending on countries; different, but unknown political events transpiring in the country and locality (a threat of take-over of a political party, being one example). The list goes on.

The Organization of the Research

The research is decentralized in execution and analysis. Each country has a Research Coordinator with few exceptions located in a University or academic institution; there is an International Steering Committee; a Project Director (the author); an International Coordinator (Dr. Krzysztof Ostrowski); and a Data Base Manager (Ms. Tatiana Iskra). The International File is constructed in Warsaw. National files are a bit different, generally containing data either of special interest to the country researchers or of special sensitivity to the respondents.

Major decisions about the research are generally made at meetings of the researchers or by the International Steering Committee. The last one in Krakow adopted principles of confidentiality. During these meetings the researchers have had opportunities to visit a locality in the community and talk with the political leaders interviewed; something that is done at every opportunity.

Checking the Data

A program has been developed to check the "consistency" of the data that is deposited in Warsaw. A few such errors have been discovered. Other errors come from a misunderstanding of a question, in which case it is deleted for that country from the international file.

We have decided that what is in the "International" file are raw data. All transformed, analytical variables that were initially used are being deleted (see B. Jacob, K. Ostrowski, and H. Teune,eds., *Democracy and Local Governance: Ten Empirical Studies.* Honolulu: HA: Matsunaga Institute for Peace, 1993). Responses to the open-ended questions are not being coded. They are in the international file as raw responses in the original languages (three at present), accessible through various programs for text analysis. (see T. Iskra, K. Ostrowski, and H. Teune. "Designing a Data Base for Comparative Research," paper presented to INTERCOCTA, Tampere, Finland, Dec., 1995).

Governance of the International Data File

Research Coordinators who deposit data from their country can become members of a Consortium, the officers and rules of which are now being established. It is understood that the international file will be made accessible to social scientists. Some researchers have requested special provisions involving consultation with them before the data are used in publication. Each contributing member is a signatory to a protocol concerning confidentiality.

Accessibility of the International File

The data from several countries are being put onto a World Wide Web page, along with documentation, such as questionnaires, articles, references, etc. This page will be updated with new information, including perhaps some visuals. A user of the page can ask

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questions and request analysis from the University of Pennsylvania. The raw data are on display for use in a PC environment. Some parts of the page will have restricted access.

Confidentiality

Countries vary on their stringency concerning confidentiality. A few have strict laws; others. loose ones. There are professional norms and different views among the researchers about how confidential their data should be.

The data are coded by country, community. and political position (implicit is the date of that position). There is one mayor in a particular Polish city in 1995. That person can be identified. Individuals. whose names are masked. are grouped by position in the international file. Mayors and Deputy Mayors and some administrators are coded as administrators. Localities are given approximate area identities in some countries. The latter two steps make it nearly impossible to compare mayors cross-nationally and to define regions precisely. It also makes it very difficult for anyone to identify a particular person from the questionnaire.

Religion. political party affiliation. and ethnicity are among the variables that are sensitive. In some countries asking such questions is illegal. Religion is variously coded and kept in country codes; religiosity is not (attending religious meetings, self-designation as "believer"). Political party affiliations are being coded generally, one example being "left", "left-center", "center". etc. Ethnicity is left to the Research Coordinators, but will enable comparisons at levels of generality as "majority" and -'minority." Western countries with a few major political parties and where the research is now being extended might require other kinds of codes to insure confidentiality.

All of these data might be available in national files and can be released at the discretion of the Research Coordinator for a particular country. There is general agreement that these data, however, should be released with caution about confidentiality.

Concluding Comments

Care has been taken to check the data that is put into the international file. In almost every case, the Data Manager and others have met to discuss the data file with the Research Coordinator. Whenever possible, the value scale items in the questionnaire have been and can be constructed into many kinds of measures. Most of these items have been used for over 30 years in cross-national research They appear to be robust.

The reliability and validity of this research will be established in the processes of analysis and the gathering of other data. It is good to hear that another research program, initiated without the knowledge of the Democracy and Local Governance program, on a population sample along with a different leadership group, found that during the past few years the commitment to democratic values in three of the same former communist countries, measured differently, has remained more or less the same, while the commitment to a market economy, also measured differently, has waned. There are theories about why these changes occurred in those countries, some of which can be examined in the two sets of data.

The Democracy and Local Governance Research program is a macro, cross-national, cross level (individuals, localities, regions, countries, and trans-national-regions), and cross-time study of democratization. Cross-time and cross-level, as well as cross-system, research is the core of the logic of comparative analysis.

These data will be available not only for replication but also for melding into other kinds of data and research programs to provide a social science foundation for understandings and explanations of the significant historical dynamics of change during the last two decades of this century.

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information effects that have served to transform policies themselves into independent variables. In three empirically-rich comparative chapters, Pierson provides evidence for his argument in his studies of old-age pensions, health, and income-maintenance policies.

The book provides an account of why, rhetoric aside, the political costs associated with retrenchment meant that the welfare state survived both Reagan and Thatcher. Using strategies of obfuscation, divide-and-conquer, and compensation, both governments sought to minimize political costs by selecting policy areas considered most vulnerable. For example while housing and unemployment-insurance benefits were cut in both countries and pensions were restructured in Britain, income-transfer programs and health care were relatively untouched. Unlike other approaches that analyze social spending by sector or category, Pierson argues that policy outcomes can only be understood if programs - and their specific political impact - are disaggregated.

The result of this "policy feedback" approach is a persuasive account of why some programs are more politically vulnerable than others. For example changes in indexation rules minimized political opposition to privatization of supplementary pensions in Britain because it will be gradually phased in, while trust-fund crises brought on by specific financing provisions in the U.S. Social Security heightened its political salience, encouraging political mobilization and making substantial reform unlikely. However, Pierson's argument that the class power-resources approach cannot explain retrenchment because opposition to programmatic retrenchment was led by program beneficiaries rather than labor unions is biased by his case selection (a possibility acknowledged by the author). Labor seems to have played a more prominent role in reform debates in other European countries, and a test of his "retrenchment politics" argument should also include these cases. Furthermore, if labor is not defending the welfare state. then who is'? If the mobilized constituencies of the individual programs were reaggregated, one wonders if a "middle-class" power resources approach might contribute to explaining the resilience of popular universal programs which benefit the middle classes.

Retrenchment politics involve both stasis and change. In explaining how political costs shape policy (and ultimately distributional) outcomes this book provides a compelling explanation of why some welfare state programs emerged from the Reagan and Thatcher years relatively intact while others were dismantled.

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New Frontiers in European Industrial Relations, edited by Richard Hyman and Anthony Femer. London: Blackwell Publishers, 1994. 414 pp.

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Though Hyman and Ferner's New *Frontiers in European Industrial Relations* contains an excellent collection of articles on important issues by well-established experts, it does not directly address "new frontiers." It offers a wealth of accepted knowledge on current trends affecting labor, employers, and governments, but few insights that challenge the widely held view of the decline of the postwar settlement, the collapse of Keynesian policies, and the decentralization of collective bargaining. The paths of inquiry are, for the most part, useful for the comparative analysis that they undertake. This volume along with its predecessor, *Industrial Relations in the New Europe,* serve as excellent reference texts for the student of industrial relations; however, these "new frontiers" should be well-worn paths to anyone familiar with the field.

This book contains a collection of essays on topics ranging from workplace representation to tripartism and the impact of European integration. The only new ground explored, however, is in the cross-national analysis of the growth of the number of women in the labor force by Jill Rubery and Colette Fagan and the analysis of past and future trends of industrial relations in Russia and Eastern Europe, The authors unfortunately side-stepped such challenging areas as the growing role of small and medium-sized companies in industrial relations as well as a more thorough look at the role of employers and employers' associations in Europe.

The book does cover a wide range of topics in the field, and the cross-national comparisons provide a foundation for some new theoretical construction. Colin Crouch's contribution, "Beyond Corporatism: the Impact of Company Strategy," is an excellent article that puts forth a deductive model illustrating the continued differences in employer strategies and their relationships with unions across Europe. Despite the fact that this article and several others in the collection emphasize the many remaining national disparities, the authors seem to accept unconditionally the theory that European integration will produce a weaker union movement devoid of national or local character.

A number of recent publications have raised strong evidence that contradicts the dire predictions of the future of European industrial unionism. The continued use of Keynesian strategies of the welfare state belies the assumed economic constraints that have caused the crisis of the state (Garrett, 1995). Indeed, the spectre of "social dumping" within the European Union that several of the articles address has not taken place according to the evidence from Erickson and Kuruvilla (1994). In addition, Peter Swenson (1991) presents a model of cross-class alliances in Sweden and Denmark that challenge the traditional focus of industrial relations on organized labor. Wallerstein (1995) goes even further, generalizing Swenson's argument and applying it to social democracy and corporatism, revealing striking findings about these topics.

These recent additions to the literature tackle the new frontiers of industrial relations. Hyman and Femer's contribution provides us with a solid and descriptive understanding of the actors in the arena of industrial relations, but stands a safe distance removed from the frontiers of research.

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Colomer, Josep M. *Game Theory and the Transition to Democracy: The Spanish Model.* Brookfield, Vt.: Edward Elgar, 1995.

The debate between proponents and adversaries of the use of rational choice methods is one of the main current debates in comparative politics. An important issue in the debate, and the main challenge for rational choice theorists, is how to relate the relatively rigid concepts of rational choice theories to the richness of the available empirical material. In *Game Theory and the Transition to* ***Democracy,*** Josep Colomer tries to do this for the case of the successful transition to democracy in Spain.

Underlying the analytical framework are the usual rational choice assumptions of rationality of the actors and methodological individualism (collective outcomes are explained as products of individual choices). More concretely, in the Spanish case Colomer distinguishes between the opposition, soft-liners, and hard-liners, each of which is subdivided into two groups. This gives a total of six groups, ranging from "revolutionaries" on one side of the political spectrum to "involutionists" on the other side. For these six groups, the author infers a preference ordering over three possible outcomes: continuation of the Francoist political system, legal reforms of this system, and a rupture with Francoism. Choices and interactions of these groups, which have different preferences over the possible outcomes, result in the outcomes that are discussed in the book.

To analyze some of these outcomes, Colomer uses several concepts or "tools" from social choice theory: "agenda control," manipulation of issue dimen-

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sions," strategic voting," and "vote trading." In chapter 1 he shows how, after Franco's death in 1975, a transitive collective choice of the new state form was reached. An intransitive choice, resulting from individual choices by the respective groups, was avoided as a consequence of agenda control and pairwise voting on the alternatives. In chapter 3, the author shows how in July 1976 the Chairman of the Council of the Realm, a body which nominates to the Spanish king candidates for the function of President of the Government, managed to have Suarez, a reformist, chosen as the new President of the Government. Suarez was hardly preferred by the members of the Council, but by manipulating the dimensions of voting, the Chairman was able to achieve this outcome. Finally, in chapter 6, Colomer uses the concepts of strategic voting and vote trading to explain the contents of the new democratic constitution that resulted from long and intensive debates in 1977 and 1978.

Other tools that Colomer employs are taken from game theory. These include a "prisoner's dilemma," "tipping games," and "incomplete information." In chapter 2, Colomer explains the dynamics of the anti-Francoist movement in the mid-1970s as the result of tipping game phenomena. In chapter 4, the author analyzes the interaction between Suarez and the "continuists" (who favor continuity, but are willing to accept reforms) with respect to the proposed Bill for Political Reform at the end of 1976. as well as the interaction between Suarez and the democratic opposition in the same period. The game that represents the former interaction had a "cooperative equilibrium" which explains the acceptance of the reform bill. The game representing the latter game. on the other hand, lacked such an equilibrium, which accounts for the absence of a real dialogue between Suarez and the democratic opposition. In chapter 5. the logic of the prisoner's dilemma sheds light on the interaction between the reformist government and the Communist party, which was legalized in May 1977. Finally, in chapter 7, Colomer uses the notion of imperfect information to analyze the behavior of the captain-generals who staged a (failed) coup in February 198 1. They miscalculated the reaction of the king, which makes their actual behavior understandable.

It should be said that the actual power and success of the use of the specific rational choice tools in this book is somewhat mixed. On the one hand, the analyses of some of the events, such as the process by which Suarez was elected President of the Government, the creation of a new constitution, and the failed coup in 1981, are highly insightful. On the other hand, some of the empirical stories are not quite consistent with the analytical concepts that are used to analyze them. For instance, the transitive choice for a new political system in 1975 seems the result of the existence of such a choice in pairwise comparisons as such, rather than agenda control by some actors. Also, the story on the interaction between Suarez and the continuists in 1976 suggests that the outcome depended crucially on the fact that the actors made non-simultaneous moves, while the game used to analyze the interaction assumes simultaneous moves. These weaknesses illustrate exactly the difficulty of applying rigid concepts to empirical material.

Thus, *Game Theory and the Transition to Democracy* illustrates both the promises and the potential pitfalls of the use of rational choice methods in comparative politics. For both aspects, the book deserves attention from comparative scholars interested in transitions to democracy and applications of rational choice methods.

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Kato, Junko. *The Problem of Bureaucratic Rationality: Tax Politics in Japan,* Princeton University Press, 1994. 327 pp.

Through a detailed examination of the interaction between Japanese politicians and bureaucrats over the enactment of a value-added tax, Junko Kato employs a rational-choice approach to examine the incentives and, consequently, to explain the behavior of Liberal Democratic Party leaders and backbenchers as they reacted to the strategic dissemination of information by Ministry of Finance bureaucrats. The book's primary intent is not to explain Japanese tax reform, although it does this very well, but rather to use tax reform as a case study for elaborating on the politician-bureaucrat relationship. Kato's investigation represents a response to the traditional descriptions of this relationship, offering a complex account of the interaction not only between, but within the LDP, the opposition parties, and the Ministry of Finance (MOF). Contrary to the conventional expectation that bureaucratic control of information leads to bureaucratic manipulation of the policy-making process in Japan, Kato argues that sharing policy information with politicians is an integral and necessary strategy for bureaucrats to achieve their objectives.

The Ministry of Finance had tried since the early 1970s to introduce a major indirect tax in order to stabilize government revenues as demographic change, in particular the aging of Japanese society, transformed the tax base. The MOF twice failed, in 1979 and 1987, before finally coaxing the LDP into passing a value-added tax in late 1988. There is little reason to believe that the LDP or any other politically active group favored implementation of a value-added tax (VAT) in the beginning. Kato posits that bureaucratic maneuvering convinced LDP leaders, as well as rising stars within the LDP, to support the indirect tax. According to Kato, many LDP members gradually came to support the generally unpopular tax measure because of the dual incentives they faced.

Kato ascribes two objectives to LDP members. The first goal is reelection and the second is the expansion of personal power within the LDP. These objectives can be contradictory in that expanding personal power can require behavior damaging to reelection opportunities. The mechanism fostering this contradiction is found in the need for party leaders to advocate unpopular but necessary policies even when such policies may damage the immediate reelection interests of party members. Supporting unpopular policies, such as the VAT, is considered, Kato asserts, a sign of political maturity that can translate into increased stature within the LDP. Of course, only politicians with strong *jiban*, personal support groups, can afford to take such stances

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without jeopardizing reelection. These politicians then pursue their second objective: influence within the party.

It is these LDP members that are induced by bureaucrats into championing bureaucratic preferences. Consequently, Kato contends, by enlisting the support of LDP members anxious to increase their stature within the party, bureaucrats, who have their own institutional incentives to secure steady sources of government revenues, have substantial latitude in which to manipulate policy-making. Kato is not arguing that bureaucrats independently dominate politics. Rather, Kato argues that LDP intraparty dynamics facilitate bureaucratic influence, illustrated by the establishment of the VAT, because LDP members need to demonstrate policy expertise for advancement within the party. Bureaucratic power primarily resided in their ability to set the agenda concerning what kinds of tax reform to consider, and in focusing on selling certain types of tax reform. They were able to make their case directly to politicians in the Government Tax System Research Council where their bureaucratic expertise was considered vital and consequently, though only after much time had passed, were able to see their preferences for tax reform realized.

Kato's investigation of Japanese tax politics represents a step forward in the discussion of the politician-bureaucrat relationship but is by no means the final destination. While usefully pointing out the shortcomings of much analysis of legislator-bureaucrat relations and offering a viable hypothesis, the alternative hypothesis that politicians have simply been well served by their bureaucratic agents deserves to be addressed. After all, politicians' preferences have consistently held sway. The VAT, as implemented, was a substantially watered down version of the initial MOF legislation precisely to protect LDP constituencies. Is it not possible that the ruling LDP was capable of determining that the VAT, as passed in 1988, was both acceptable and necessary? Politicians acquire expertise not simply to increase their stature but also to enable themselves to formulate policy that conforms to their preferences. In the case of the VAT, the LDP seems to have done just that.

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Pierson, Paul D. *Dismantling the Welfare State? Reagan, Thatcher and the Politics of Retrenchment, New* York: Cambridge University Press, 1994.

In this comparative study of social policy reform during the Reagan and Thatcher years, Paul Pierson argues that while class "power-resources" and institutional approaches provide credible accounts of the expansion of the welfare state, they do not explain the politics of retrenchment. In the former, a weakened labor movement would lead to expectations of welfare state decline, not resilience, while in the latter, Thatcher and Reagan achieved comparable results in cutting social spending despite institutional advantages conferred upon majority governments in Britain's centralized parliamentary system. Pierson suggests that retrenchment plitics are distinct because the welfare state has created interest group constituencies, "lock-in" effects that create social and economic networks, and

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