

# APSA-CP

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Comparative Politics of the  
American Political Science Association

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## Letter from the President

### Data, Field Work and Extracting New Ideas at Close Range

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At the annual meeting of the Comparative Politics Section Executive Committee in September 1998, the committee had an important discussion about problems of collecting and disseminating different types of data. This letter addresses some initiatives and concerns that grew out of our discussion.

The field of comparative politics has recently seen a wide-ranging debate on new approaches to theory and method. My comments below reflect the view, expressed by scholars coming to this debate from quite different perspectives, that these concerns with theory and method need to be reintegrated with a focus on the kind of inductive learning that can arise from deep engagement with data. I consider some questions about the academic infrastructure needed to support that reintegration, including the problem of encouraging the collection and dissemination of data sets, opportunities for publishing data-rich country studies, and issues of funding and training for field research. I also discuss the contribution of new ideas that can emerge from the close analysis of cases, and the choice between single-country and multi-country doctoral dissertations.

### Quantitative Data Sets

The Comparative Politics Section has long had a strong interest in encouraging the development of publicly-available quantitative data sets as an essential foundation for cumulative research. Part of the background for this interest is the trajectory followed by the tradition of quantitative cross-national research. Notwithstanding a promising start in the 1960s, the initial payoff of this approach in terms of substantive findings was modest. This was due in part to shortcomings in the data sets then available, and also to the limited repertoire of statistical techniques conventionally employed at that time.

In the past 20 years, however, better data have become available,

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new methodological tools have been developed, advanced training in quantitative techniques has become more common, and a rich body of work has emerged. Recent recognition for this work includes the award of the section's 1998 Luebbert Article Prize to Przeworski and Limongi's innovative study "Modernization: Theory and Facts" (*World Politics*, January 1997), which examines the emergence and persistence of democracy in 135 countries.

One of the problems in building a viable tradition of quantitative comparative work is that the enormous effort entailed in creating the requisite data sets is often not matched by corresponding professional rewards. Out of a concern with addressing one aspect of this problem of professional rewards, the Executive Committee has established a new Data Set Award, which complements the Section's book, article, and paper awards. The new award will be given annually for a publicly-available data set that has made a significant contribution to the comparative field. I have appointed a committee of Jennifer Widner (Chair), Barry Ames, and Peter Lange to make the initial award and to establish a framework for guiding future award committees.

### **Publishing Data-Rich Country Studies**

The executive committee also discussed opportunities for publishing single-country studies that present the richly-detailed qualitative data that are an indispensable foundation for comparative research. A central concern is that leading comparative politics lists, such as those of the Cambridge and Princeton University Presses, shy away from single-country studies, in part because the market for such books is considered too limited. As a consequence, the professional recognition that derives from being published with one of these prestigious presses is rarely bestowed upon what are potentially influential studies that are critical for the progress of our field.

What has now emerged is a new division of labor, in which a different set of presses has assumed a leading role in publishing high-quality country studies. In my own subfield – Latin American politics – this shift is exemplified by the list developed at the Pennsylvania State University Press by Sanford Thatcher. After two decades at Princeton Press, where Thatcher was well-established as one of the leading social science editors in the United States, he became director of the Penn State Press in 1989. In the past decade there, his approach to publishing books on Latin America has been based in part on the premise that, for quite a few countries, a strong market *does* still exist for single-nation studies. This market overlaps with, but is partly distinct from, the market for general books in comparative politics. Building on this premise, Thatcher has published an impressive collection of country studies focused on Latin American politics. These books are often immediately released in paperback, and in 1999 the list will include 12 new titles.

In this new division of labor, innovative country studies on Latin America that two decades ago might have been published by Princeton, California, Stanford or Johns Hopkins, are now often published by such presses as Penn State, Pittsburgh, North Carolina, Notre Dame, Westview or Lynne Rienner. In writing tenure evaluations for scholars who have published an initial book with presses like those in this second group, I have on more than one occasion felt it was appropriate to under-

*(Continued on page 4)*

# News & Notes

Gary King recommends to the *Newsletter's* readers this **archive of qualitative data**: <http://www.essex.ac.uk/qualidata/>. ❖

The University of Pittsburgh has announced a **fund-raising project to honor Richard W. Cottam**, its recently deceased University Professor emeritus of political science. Pitt seeks an **endowment to support doctoral students** in political science and allied fields and programs who are at the dissertation stage working on subjects pertinent to Cottam's interests in the following areas: international relations theory, foreign policy, nationalism and ethnicity, diplomacy, theories of peace and conflict, and the international politics of the middle east. In announcing the project, political scientist Bert Rockman spoke of Cottam's dedication to "all the facets of his calling. His scholarship on Iran, international relations and diplomatic theory, foreign policy and nationalism was notable and provocative. His dedication to teaching in all of its aspects was legendary."

Referring to Cottam's influence on his colleagues in the academy, Rockman said: "He challenged us all in ways that made us rethink what it is we thought we knew." Speaking of Cottam's intense interest in foreign policy, Rockman said that Cottam was "motivated to connect the world of scholarship to the world of practical affairs in a way that would make the world we all live in safer."

Contributions to the "**Richard W. Cottam Memorial Prize Fund**" should be mailed to the Office of Development, University of Pittsburgh, Pittsburgh, PA 15260-3700. ❖

**Political Power and Social Theory** is a research annual committed to advancing our interdisciplinary understanding of the historical linkages between politics and social class. It welcomes both empirical and theoretical work and is willing to consider manuscripts of substantial length (up to 80 pages). Publication decisions are made by the Editor in consultation with the Editorial Board and anonymous reviewers. There are no specific deadlines, and manuscripts are handled in the order in which they are submitted. Potential contributors should send four (4) copies of the manuscript, with all references to the author erased, to the following address:

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score the fact that these presses routinely publish excellent studies.

### **Financial Support for Field Research and Changes at SSRC**

A related observation should be made about the allocation of financial support by organizations such as the Social Science Research Council for the kind of research that produces these country studies. In 1996 SSRC carried out a reorganization in which the area studies committees that had long been jointly sponsored with the American Council of Learned Societies were replaced by a new system of Regional Advisory Panels. It has sometimes been assumed that this reorganization reflected an abandonment of a commitment to area studies on the part of SSRC and of the foundations that support its programs. However, SSRC continues to view area-based research as an indispensable component of internationally-oriented scholarship, as was strongly emphasized in the original statement describing the reorganization (SSRC *Items*, Nos. 2-3, 1996, p. 32). Compared to ten years ago, the level of annual support offered by SSRC for graduate student research based on field work has in fact been *higher* over the past few years – including support for language training, dissertation field research, and a major new program of predissertation training in preparation for field research. Moreover, in 1998 SSRC received a substantial increase in its core support from the Ford Foundation for these programs.

### **Training in Field Methods**

Given the essential role of field research and data-rich country studies as a foundation for broader comparative analysis, it is unfortunate that systematic training in field methods is not a more standard part

of the graduate curriculum in political science. In graduate teaching, we give an appreciative nod to Richard Fenno's idea of "soaking and poking," or to Daniel Lerner's classic discussion of field interviewing in his famous chapter on "The Grocer and the Chief." Yet systematic training in field methods is all too rare.

A welcome exception is a graduate course on qualitative methods at the University of Minnesota, initiated by Kathryn Sikkink, which includes units on participant and non-participant observation, elite and non-elite interviewing, archival research, and strategies for the inductive analysis of qualitative data. Several other political science graduate programs are considering expanding their training in these aspects of methodology. Another innovative effort to provide training in the diverse skills required for carrying out successful field research is the annual conference held for recipients of the SSRC International Predissertation Fellowships. Over the past several years, this conference has included sessions on archival research, focus groups, oral history, elite interviewing, ethnographic methods, the use of census data, issues of sampling and statistical analysis in small-N survey research, ethics and confidentiality in field work, and problems of research design in exploratory field work. With regard to textbooks and new methodological studies focused on these topics, Sage Publications has been a leading press, paralleling their prominent role in the field of quantitative methods. Sage's book series on "Applied Social Research Methods" and on "Qualitative Research Methods," as well as their Qualitative Methods catalog, are all listed on their web site and are useful starting points in looking for teaching materials.

### **Extracting New Ideas at Close Range**

I would also like to call attention to the role of data-rich country studies as a source of new ideas, hypotheses, and research agendas, and not just as a source of data for broader comparative research. The sociologist Alejandro Portes has underscored the special contribution of researchers who are experts at "extracting new ideas at close range." These scholars are deeply engaged both with theory and with the close analysis of cases, giving them an unusual capacity to see the general in the particular.

Examples from the Latin American field of classic country studies which are based on this kind of research, and which grew out of doctoral dissertations, would include Alfred Stepan's *The Military in Politics: Changing Patterns in Brazil* (1971), which established a broad intellectual agenda for studying the military in the Third World; and Philippe Schmitter's *Interest Conflict and Political Change in Brazil* (1971), which was a crucial step in the emergence of the comparative literature on corporatism. An example from another region and another generation of scholars is Frederic Schaffer's *Democracy in Translation: Understanding Politics in an Unfamiliar Culture* (1998), which explores the contrasting meanings of "democracy" in different political contexts, building on field work among Wolof-speakers in Senegal.

The ongoing contribution of a senior scholar, Guillermo O'Donnell, provides further examples of extracting new ideas at close range. Drawing on a deep knowledge of the Latin American region, and especially of Argentina and Brazil, O'Donnell has a remarkable history of producing conceptualizations and hypotheses that have opened new research agendas across the com-

parative politics field. His recent work includes an important critique of the concept of democratic consolidation, as well as a new conceptualization of executive dominance, which he characterizes as “delegative democracy,” and of its consequences for the institutionalization of regimes. He has also explored the issues posed for democratic theory by the sometimes problematic nature of citizenship and the legal system in Latin American democracies, and by “brown areas” in which the authority of the national state is severely attenuated.

It would be interesting to explore, for different world regions, the evolution of this kind of work based on a close, creative engagement with cases. Doubtless one would find variations in the role of different generations of scholars and in the substantive topics on which they focus. For present purposes, I would simply emphasize that the importance of extracting new ideas at close range is recognized not only by specialists in particular countries or regions, but also in new work on theoretical modeling in comparative politics and international relations. In the *Analytic Narratives* volume (1998), Bates, Greif, Levi, Rosenthal, and Weingast underscore the contribution to theory-building of “a close dialogue with case materials” (p. 3). They advocate an approach that “pays close attention to stories, accounts, and context,” that employs Geertz’s method of thick description, that is driven by a “fascination with particular cases,” and that “contributes to the idiographic tradition in the social sciences” (pp. 10, 13, 14). Robert Powell’s forthcoming Princeton Press book on formal modeling in international relations, *In the Shadow of Power*, expresses a similar idea. In exploring alternative sources of innovation in modeling, he observes that “new ideas, of

course, can also come from the empirical realm,” and he points to the importance of a “detailed historical knowledge and deep sense of the cases...” (chap. 1).

### ***Implications for Single-Country Versus Multi-Country Dissertations***

These observations concerning data-rich studies and the inductive component of research point to a question about the recent trend toward multi-country doctoral dissertations in comparative politics. In my previous letter I observed that the intellectual success of old and new work in comparative-historical analysis has encouraged this trend, and up to a point that is certainly a positive development. For some areas, such as Western Europe, multi-country dissertations are relatively common, and they are greatly facilitated by the remarkably good monographic studies and statistical data available on countries in that region. More broadly, plausible models for multi-country projects can be drawn from the comparative-historical tradition, the comparative case-study tradition, and the quantitative cross-national tradition. Nonetheless, more than a few colleagues in the comparative field are convinced that the trend toward multi-country dissertations has gone too far.

One concern is that too many multi-country dissertations are analytically thin and data thin, and that others end up being hard to complete. I am told that among the multi-country dissertations funded in the past few years by SSRC, a significant proportion of the grant recipients encountered difficulties that eventually led them to reduce the number of cases, or to abandon multi-country comparison altogether in favor of a single-country study.

A second concern is that the idea of a “comparative dissertation”

should not be conflated with the idea of a “multi-country dissertation.” Systematic within-nation comparison, including a focus on change over time, also makes a dissertation “comparative,” and the resurgence of interest in federalism and in comparisons of sub-national political units reminds us that within-nation comparisons are indispensable for some topics. Further, dissertations focused primarily on one national case often succeed in placing that case in a strong comparative perspective, thereby combining intensive analysis of one country with broad comparison.

A third concern is with the intensive learning that graduate students can derive from immersion in the analysis of a single national unit. Due to personal and professional obligations that routinely arise later in a career which can make it difficult to arrange extended periods of residence abroad, the traditional 12 to 15 months spent “in the field” doing dissertation research often end up being the best opportunity that many scholars ever have to become deeply engaged in the intensive analysis of politics in another country – and often in building valuable personal contacts and language skills. From this perspective, a career sequence that moves from a single-country dissertation to multi-country research is not only a common one, but a logical one, and a large proportion of the scholars who have gone on in their careers to do significant work based on multi-country comparisons in fact began with single-country dissertations.

Finally, choices about the scope of comparison in dissertations are important not only for the individual scholar, but also for the comparative field more broadly. If the best students were to stop doing single-country dissertations, we would end up with a more limited supply of the well-crafted, theoretically-informed

country studies that constitute an essential building-block for comparative research. It would be a major setback to our field if young scholars did not produce outstanding country dissertations like those which led to the books of Stepan, Schmitter, and Schaffer noted above.

Three implications are suggested by these various concerns. First, if a multi-country dissertation is undertaken, a special burden is placed on the dissertation committee to ensure that the student has the appropriate combination of skills to carry it out, and that the research design effectively creates opportunities for coming up with new findings. One approach is to build into the research design opportunities for close analysis of data that may lend itself to extracting new ideas at close range. Second, it would be a mistake if scholars who write single-country dissertations are passed over for jobs simply because they have studied “only” one country. Instead, a more complex judgement must be made about the gains in knowledge that derive from their research. Also, given that entry-level hiring decisions necessarily depend on a conjecture about the future research trajectories of new Ph.D.s, one consideration in that

conjecture should be a recognition of the learning and research skills that can grow out of a single-country dissertation. Third, overall, striking a productive balance between single- and multi-country dissertations, in both graduate training and faculty recruitment, is an ongoing challenge for our field.

#### ***A Field Built on Diverse Skills***

The themes explored above serve as a reminder that the intellectual vitality of comparative politics depends on the contribution of scholars with diverse skills. David Laitin, in one of his letters from the president in this *Newsletter* (Summer 1993), discussed alternative strategies for avoiding in comparative politics a narrowing of the intellectual agenda such as occurred in linguistics with the Chomsky revolution. The priorities I have emphasized here converge with the strategy advocated in Laitin’s letter: by bringing together scholars with strong theoretical tools, good methodological skills, and a talent for creative engagement with cases that yields new research questions and hypotheses, comparative politics can successfully avoid this fate. ♦



## Letters to the Editor

Dear Professor Golden,

For several years now I have been receiving the *Newsletter* and have generally found it to be stimulating and informative. You should be congratulated for the time and effort invested in its publication. As someone who works at the interstices of “area studies” and comparative politics and who has done extensive field research (China and Taiwan), I have to take issue

with one particular claim in your Introduction to “Is There an International Division of Labor in Comparative Political Science?” (Summer 1998) In it, you argue that “with the demise of area studies in the US, we are in danger of losing area expertise. But what we lose others may well do better: the area expertise of natives working in their own countries is likely to be richer and deeper than those

of us working in the US can generate anyway.” These arguments are either false or based on unwarranted assumptions about how knowledge is generated outside of the US.

First of all, who pronounced area studies dead? Perhaps according to rational choice theorists in the US – who are still a minority – but not according to many other fields. Just recently there have been con-

ferences on how to mesh area studies with concerns about “globalization” at Chicago and Duke. Attended by historians, sociologists, anthropologists and political scientists, the general consensus emerging from the meetings was that it would be foolish to abandon language training and in-depth knowledge of particular countries – the strengths of area studies – to focus on the homogenization of cultures given the breakdown of national borders. At the same time, area specialists need to be aware of economic and social forces beyond the physical boundaries of their respective areas. In short, the consensus calls for a balance between theory and close case analysis. That said, I should also point out that no serious scholar now is atheoretical, doing only “descriptive” monographs. This argument against area studies may have been true when the field first began, but is hardly true today.

Second, and more important, the second part of your argument reveals a real naivete and ignorance about the way knowledge is produced elsewhere in the world. You and others seem to assume that there are all of these “natives” who will produce useful, descriptive monographs that US-theorists can then use as their raw material to generate new theory. But is this the case? Many foreign scholars do not have the necessary financial resources or intellectual tools that would make their studies of their native countries very useful to you; most do not write in English, so unless you can read Chinese, Korean, Thai, Indonesian, Czech, Uzbek, and Russian, good luck reading them. You are also forgetting that many intellectuals abroad are not nearly as independent from the state as US scholars can be. Research institutions in China, Taiwan, Korea, Singapore and Indone-

sia are often funded by the state, and their research goals shaped by the state’s own interests. Do you really think scholars in many countries can apply for and receive a plum research grant and then go ahead with their research without being censored (or self-censored) at some stage? In China, to give one specific example, scholarship still has to conform to Marxist social and economic analysis. I ask you: how will you use China as a comparative case if the journals are in Chinese and Marxist jargon? The same could be said of many other countries where you expect natives to do the nitty-gritty for you. I challenge you to name 5 really good books in comparative politics written by these natives in the last 10 years. The fact is that US and Europe-based scholars – some in political science and others not – have been far ahead of the natives because the latter frequently lack the resources, independence and analytical tools necessary for good research. The natives who have done good work have been trained in the US or Europe and then returned to their home countries. This is a positive thing, but how many of these people do you think there are?

If the latest crises in East Asia has taught us anything, it is that we should be careful about unwarranted generalization and abstractions. How is it that economists, with all their fancy models, failed to predict the crises. Simple: they knew nothing about what happened beyond the capital; trained only in economic theory, they knew very little about the specific countries they were dispensing advice to; they were easily bamboozled by lying officials; and they believed the numbers presented to them by the state and various research institutions. Moreover, even though many East Asian countries were

called “tigers,” “emerging markets” and the like, it’s now more than obvious that Thailand is not Indonesia and Taiwan is not Korea. Their responses to the crises have been quite different. I wonder how theory will capture all these differences, especially considering that many academics in these countries now have to moonlight to supplement their incomes and hardly have the time or energy to produce the vaunted descriptive monographic raw materials you and others expect them to produce.

These attacks on area studies have gone far enough. Given the current atmosphere, fifteen years from now there won’t be enough American scholars who can read languages aside from English, French or Spanish, and all of those terrible area specialists hired in the 1960s and 1970s will have retired. I wonder just how policy will be made when this happens.

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Dear Professor Golden,

The *APSA-CP Newsletter* symposium asking “Is There an International Division of Labor in Comparative Political Science?” made an unevenly emerging trend more explicit. Colleagues from Taiwan, Italy and Brazil outlined current practices while drawing inferences for the professionalization of comparative politics. We are indebted to Jih-wen Lin (“Comparative Politics in East Asia: A Discipline for the Nation, or of the Nations?”), Gianfranco Pasquino (“Comparative Politics in Comparative Perspective”) and Maria Hermínia Tavares de Almeida (“Two Academic Traditions”) for sharing their insights (Summer 1998).

This dialogue might also consider the activity and potential of

the Philippine Political Science Association (PPSA). Like their Taiwanese colleagues, many PPSA members in the top three universities of the Republic of the Philippines earned their doctorates in the US. The PPSA assists political scientists "in the performance of their teaching, research, and advocacy functions." During the 1996 and 1997 National Conferences of the PPSA, "global and national processes of democratization" dominated the agenda. Meanwhile, the PPSA has reconfigured "a new political science curriculum," as well as "the 'political science' component" required of all tertiary education institutions ("The Teaching of Politics and Governance," National Conference of the Philippine Political Science Association, 28-29 October 1998, page 1).

The international dialogue among comparative politics specialists might be deepened in other ways. Highlighting issues related to the international division of labor can situate the discussion in a context crucial to the future of comparative politics. If American scholars acknowledge the implications of the *verstehen* expertise of comparativists in other countries, then four corollaries follow. First, conference panels at meetings held in the US should include colleagues with that kind of expertise as paper presenters and discussants; and American scholars should participate on comparative politics conference panels abroad. Opportunities to give two conference presentations and five invited lectures during my tenure as a Visiting Research Fellow and Fulbright Scholar at the University of the Philippines-Diliman (1995-96) greatly enhanced the theoretical acuity of my dissertation research on the domestic and international politics of foreign policy-making in the Philippines and Japan. This

suggests that the theory-data "trade deficit" may yet be balanced. Second, translating our work and publishing it in journals of national political science associations will increase confidence in its validity and reliability. Third, efforts to include members of those associations among external reviewers for journal articles should be increased. Fourth and finally, sharing of unique data sets needs to become more common so as to stimulate replication.

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Dear Professor Golden,

In "Is There an International Division of Labor in Comparative Political Science?" (Summer 1998), you raised the possibility of an emergent international division of labor between native area specialists and general comparativists in the US. Such a division of labor, while not without some appeal, raises some disturbing issues. While I have no objection to rethinking the division of labor in social science disciplines across national boundaries, any new division of labor ought to focus on the roles played by different kinds of scholarly endeavors and NOT the roles played by scholars of different nationalities.

To begin with, the premise behind the idea of a new international division of labor – the demise of area studies in the US – is itself open to question. Graduate students in top doctoral programs still enthusiastically pursue language training en route to specializing in certain countries or regions. Research grants continue to be given each year for area research to thousands of graduate students and faculty. And the proceedings of aca-

democratic associations devoted to the study of various areas continue to be well attended and insightful. These observations notwithstanding, even if we grant that the relative significance of area studies within US political science has declined in recent years, the idea of a new international division of labor between native area specialists and US theory-builders is based on several further assumptions about both the relationship between the investigator's background and his/her skills and the relationship between theory and history in general.

First, there is no *a priori* reason to believe that a native scholar is better equipped to identify, extract and interpret crucial pieces of information in his or her own region. There is certainly a lot of excellent scholarship by native country experts, and these experts undoubtedly have had an easier time dealing with language issues and operating within the social contexts in which they operate. At the same time, however, the process (and challenge) of learning a foreign language and understanding a less familiar social setting is itself one of the reasons why non-native area specialists can develop a different lens through which to understand and frame their empirical research. Moreover, non-native area specialists are always working at least implicitly with a comparative referent (presumably the country they were raised in or trained in). For these reasons, on the whole, non-native area specialists are likely to be more interested in relating country-specific historical processes or data points to larger theoretical questions as they tackle the difficulties of gaining expertise and uncovering empirical information in a region.

Even if the distinctiveness of the intellectual gains from non-native area scholarship is open to

question, the very possibility suggests that we cannot blindly assume that native scholars have an inherent comparative advantage in doing empirical research in their countries, especially for investigating problematics developed by comparativists elsewhere. There is no guarantee that native scholars from different regions will share the same points of concern or the same conceptual apparatus; nor is there a guarantee that the information they supply – even if available in English – will be framed in terms that will enable theorists in the US to adequately grasp the significance of crucial facts within their own problematics. And, if native scholars are going to be as useful to comparativists in the US, it will only be because we insist on providing their training in the US or because we presume to dictate what the important questions of the day are so that they can return to their native areas and find the raw data with which we comparativists can engage each other in theoretical debates.

There is also the question of the manner in which data gathered by area specialists is used in nomothetic scholarly endeavors. Certainly, native scholars can help gather data for the testing of formal theories through cross-national statistics, but there is an excellent, well-established tradition of comparative-historical research that is inductive in its logic but still nomothetic in character. This tradition relies on a detailed knowledge of a few select cases, and the historical character of this knowledge makes the comparison across cases a more challenging problem for theory-building. It is this tradition that stands to lose the most if area studies in the US were to go into further decline. Not only has the literature produced by US area specialists proven enormously impor-

tant in providing comparative-historical scholars with a solid grasp of their cases, but given that US area specialists have an ongoing exposure to broader conceptual frameworks and theoretical debates in comparative politics here, their research has been explicitly or implicitly framed in a language accessible to other comparativists (whether generalists or specialists in other areas). If comparative-historical research were to rely solely on the work of native area specialists, it would prove enormously difficult to interpret the significance of qualitative information obtained in each of the cases so as to enable meaningful comparisons over time and space. Such works as Moore's *Social Origins*, Bendix's *Work and Authority in Industry*, and Skocpol's *States and Social Revolutions*, whatever their individual merits, have collectively made an important contribution to comparative politics, and it is hardly surprising that the case studies in these works rely primarily on English-language sources. I find it difficult to believe that these works could have been produced without excellent English-language literature produced by non-native area specialists.

Finally, as long as we are rethinking the present division of labor between area specialists and general theorists, then we may want to consider the potential intellectual returns from a “middle-range” specialization in the study of a few select areas on an ongoing basis. General comparativists, even those working in the comparative-historical tradition, do not usually seek a detailed knowledge of the broader historical or social contexts from which they extract their crucial facts. Nor do they care to seriously engage the particular scholarly debates unique to the study of any given area. Area specialists

(whether native or not), on the other hand, generally do not pay much attention to historical processes and scholarly debates emerging from areas outside of their own area of expertise. In between, there remains a gap that needs to be filled by a new generation of scholars who are “semi-specialists” in three or four distinct areas. Such scholars would not be equipped with the same language skills and the same ability to do extensive fieldwork as the true area experts. At the same time, they are more interested in delving into the historical and social contexts from which generalists seek to extract the relevant data points, and they are serious about engaging the scholarly debates of a set of countries on an ongoing basis. To take one example, while I myself am an Indian-born scholar, I have gained expertise as a “middle-range” specialist with background and ongoing interest in the study and historical comparison of three countries: India, Russia and Japan. The value of this particular package of skills may not be self-evident, but such a peculiar form of specialization can play a critical role in improving the communication between area specialists (whether native or not) and generalists engaged in nomothetic scholarly endeavors (whether formal or comparative-historical).

In marked contrast, an international division of labor between American comparativists and native area specialists would only further weaken the communication between area specialists and generalists and may even produce a new intellectual hierarchy with serious long-term repercussions for the production of knowledge across national boundaries. Even if US comparativists may not always privilege nomothetic scholarly endeavors over the extraction of empirical information by area schol-

ars, control over the process of manufacturing theoretical knowledge is clearly going to be in the hands of the former.

This would not matter as long as comparativists and area specialists coexist within the same countries and within the same departments since they may be able to recognize their interdependence within the larger community of scholars who share the investigation of certain theoretical problematics. But, by identifying area expertise with native scholars and theoretical knowledge with US comparativists, the proposed international division of labor will end up being viewed by native scholars as nothing short of a new academic dependency in which American comparativists (in the core) retain

control over the research agenda and engage in the manufacturing of theory, while native scholars (in the periphery) compliantly provide the raw materials (country-specific data) and markets (for theoretical knowledge produced in the US). Fortunately, as long as national boundaries matter at all in the identification of scholarly traditions, it is not likely that native scholars will docilely submit to such an arrangement in the absence of force; and this means that general comparativists in the US will have to continue to rely at least partially on American colleagues in area studies after all.

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## Good Reads

### Hirschman on Reform

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I have been re-reading Hirschman. There are plenty of good reasons to do this. My reason was probably not the most common. The objective, in a word, was self-flagellation.

I should explain. Like many others, I have been struggling recently to find something meaningful to say about the politics of economic reform. Whole forests have disappeared since political scientists first set out down this road. I'm probably not the only one to wonder if the results—my own small contributions first in line—were worth the trees. At moments of frustration, it seems to me that we have created a vocabulary to talk about how reforms get—or do not get—enacted and implemented,

a burgeoning thesaurus of phrases—“support coalitions,” “winners and losers,” “political entrepreneurs,” “heresthetics” and so on. We pull these out and dust them off when the subject comes up in conversation. Yet the progress from phrases to theory seems to have been slow, highly abstracted or else largely negative—discoveries that purported generalizations (e.g. the “authoritarian advantage”) were not as general as some (including the generals) had believed.

Which brings me to Hirschman. I am not sure to what extent this is a generational thing, but when I was in graduate school it was taken for granted that well, Hirschman had the answers. If one wanted to understand the way economics and politics intertwined, the place to start was *Exit, Voice and Loyalty*. Alongside certain works of Thomas Schelling and one by

Mancur Olson, Hirschman's were the closest an apprentice political economist could get to a canonical text.

Had we learned anything new, I found myself wondering, since Hirschman's tentative explorations of the process of Latin American economic reform in the late 1950s? Besides the expanding thesaurus of “conceptualizations,” could we claim any unmistakable progress? In the self-flagellating mood, I expected to find every minor discovery I thought I was making already captured in some obscure paragraph in *The Strategy of Economic Development* or *Journeys Toward Progress*, stated elegantly and precisely, illustrated with apt empirical examples.

At the same time, I hoped to take consolation in the discovery that reform itself had not changed, that the problems of enacting policies against the opposition of pow-

erful social groups in rural Colombia in the 1950s resembled those in Poland or Russia in the 1990s. If there were no new theories under the sun, I hoped that we might at least find solace in seeing that there were also no new political phenomena, that we were still trying to hit the same target.

I had a second reason as well. Recently I have been worrying about what I call the “paradox of reform”. If existing institutions represent an equilibrium that is sustained by the self-interested behavior of powerful actors, how given the existing distribution of power can reformers change institutions? In other words, what is it exactly that reformers *do*? This paradox crops up regularly in different contexts. Tollison and Wagner broach it in a 1991 article in *Kyklos* on “Romance, Realism and Economic Reform” (their somewhat unsatisfactory answer: reform is always impossible). But it is not unique to public choice analysis. This was also one reason for Marx’s scorn for social democrats: if the 19<sup>th</sup> Century bourgeois states were the instrument of the bourgeoisie, what foolishness to think that reformers could transform them into something else. What, I wondered, had Hirschman said on this subject.

### *Plus ça change...*

One gets the sense quickly on opening *Journeys Toward Progress* that one has stumbled into unusual company. Within a few pages the conversation has touched on Plato, Pascal’s quip about Descartes’ conception of God, Schumpeter and Lorenzetti’s frescoes at Sienna’s Municipal Palace. The frontispiece is by Paul Klee. Were it not deftly done, all this might seem a little overpowering.

But the display of learning soon gives way to a meticulous examination of the details of anti-

drought policies in Brazil’s North-East, land reform in Colombia and inflation control in Chile. There is plenty here that suggests the similarity of reform problems across time and space. For a Russian specialist, one has to work to repress the facile conclusion that all large states or empires are alike. In Latin America, Hirschman recounts, “many of the laws dictated in Madrid were already then received in Lima, Mexico and Bogotá with the refrain: ‘One obeys, but one does not carry out.’” (*Journeys Toward Progress*, p. 97) The phrase sounds as though it might have been translated from Russian. Replace Madrid with Moscow, Lima with Ljpetsk, Mexico with Murmansk, Bogotá with Bashkortostan, and the translation is complete.

The account of Chile’s monetary policy in the first 60 years of the century also arouses *déjà vu*. An eminent US advisor flies in to counsel Chileans to “balance the budget” and impose a strict gold standard. There is even something oddly familiar about the name of one of the commission’s organizers—a certain Mr. Saks. A series of attempts to stabilize at the expense of powerful social groups fails, as does an attempt to persuade all to sacrifice for the mutual benefit. Finally, stabilization is accomplished at the expense of the working class, at a moment of President Ibanez’s political *weakness*. All this has parallels in Russia’s fight with inflation in the 1990s—perhaps even the transitory nature of victories. So does the emergence of “structuralists” who favor price and capital controls over the painful rigors of orthodox monetary stabilization efforts.

A passage that Hirschman quotes from John Kenneth Galbraith on land reform could be directly transposed to the debate on post-communist privatization.

“Unfortunately,” Galbraith writes, “some of our current discussion of land reform in the underdeveloped countries proceeds as though this reform were something that a government proclaims on any fine morning—that it gives land to the tenants as it might give pensions to old soldiers or as it might reform the administration of justice. In fact, a land reform is a revolutionary step; it passes power, property, and status from one group in the community to another. If the government of the country is dominated or strongly influenced by the land-holding group—the one that is losing its prerogatives—no one should expect effective land legislation as an act of grace.”

Hirschman’s theoretical suggestions also recall today’s debates. The model at the end of *Journeys Toward Progress* of uncertainty and “more than one issue” has a thoroughly contemporary flavor. One point made earlier in the book—that an individual landowner may prefer a gamble in which there is some small chance of total expropriation to the certainty of a small tax—also seems to me to have been made a number of times since. The question of the relationship between crisis and reform appears, and is dealt with as unsatisfactorily as is common today. Hirschman first accepts the view that crisis increases the chance of reform, but, though quoting a definition of “crisis” from Harold Lasswell, he does not explain how a “crisis” can be distinguished from mere aggravation of a problem. Later, he admits that at times “action of either a reformist or a revolutionary nature may be stimulated when the problem begins to *recede!*” (*Journeys Toward Progress*, p. 273) Problems are solved not because of crisis but because they are becoming easier to solve.

A description of how North-Easterners in Brazil used the threat of secession to extract aid from central authorities reminded me of patterns in 1990s Russia. But here subsequent experience does seem to have taught us something new. The threat of secession is of limited use, Hirschman argues, because “the threat is really one of an all-out centralized clash; it cannot be graduated since a region cannot secede ‘just a little bit.’” (*Journeys Toward Progress*, p. 258) Tell that to Tatarstan.

Some original insights seem, regrettably, to have fallen from view. Hirschman’s conception of ideology could stand reviving. Ideology, in his account, is not a superstructure in the Marxist sense but an “in-between-structure,” connecting otherwise unrelated policy goals and helping politicians to shift the focus of action. Ideological arguments show that solving problem B is necessary in order to solve problem A. The implication is that powerful ideologies consist of causal arguments that are plausible but wrong. (If they were right, they would constitute knowledge, not ideology; if implausible, they would be ineffective.)

A few features seem more dated to a late 1990s reader. The attribution of problems of development to the lack of a “vigorous growth mentality” or of a sufficiently strong “achievement motivation, as measured by experimental tests”, and the musings on a “Latin American style of problem-solving” have a certain 1950s ring to them. (If *Journeys Toward Progress* reads at times like the work of an economist who has just spent weeks in the company of Charles Lindblom, that, of course, is because he had. The book resulted from a trip the two made through the capitals of Latin America.)

### *Journeys toward progress?*

Have we learned anything new in the decades since Hirschman’s early work appeared? I have to admit I found plenty of evidence that political economy advances through a process of cyclical, selective forgetting and rediscovery. Even in 1958 it was considered a cliché that the mere fact of market failure did not mean state intervention would necessarily succeed. (*The Strategy of Economic Development*, p. 65) How many times since has this been authoritatively reasserted? To a superficial consumer of the recent literature on endogenous growth—and this may reveal my lack of understanding of the subject—the first five or six pages of *The Strategy of Economic Development* read as though they could have been written last week. These pages describe how attempts to understand growth have progressed from a focus on “objective, tangible” inputs—natural resources, capital, labor—to “more and more subjective, intangible, and unmeasurable ones” such as investment in human capital and the supply of entrepreneurial talent. Besides such “nonconventional inputs,” development theorists have started to focus on factors such as “minimum standards in public order, law enforcement, and public administration.” Entrepreneurial talent, Hirschman suggests, is itself endogenous—a by-product of the process of economic development. What accounts for successful development is not the increase in particular physical inputs so much as the way they are combined and the way the process of growth itself creates feedbacks that generate new inputs.

Though so much of contemporary political economy did seem to be foreshadowed—or improved upon—in works published four decades ago, I was not sure in the

end how dismayed one should be by this. One response would be to accept that though the theories themselves may not have advanced so much, the sophistication of the models and tests of them has increased. Another might be to accept that perhaps the genuinely valuable service political science can perform is not the discovery of universal truths or the construction of an ever-higher edifice of empirically supported deductive reasoning, but the determination of which of various models and analogies is appropriate to particular cases, past or current. We are not reinventing or rediscovering the wheel so much as showing how it can be used—with what sort of tires, what diameter—in different kinds of carts.

### *The paradox of reform*

So how do reformers overcome the constraints against reform? What is it that reformers *do*? The only answer I could find in Hirschman was one that has surfaced many times since but that somehow leaves me unconvinced. Reformers are those who seize the moment. They exploit the lapses of others. “The Reformer—Naïve or Wily?” one heading asks. The answer—he starts out naïve, pursuing reforms that, because of the paradox of reform, are not achievable. But in the process the reformer becomes wily, “a master tactician who manages to slip through a workable reform to the surprise and dismay of both landowners and revolutionaries!” (*Journeys Toward Progress*, p. 272) How? The example Hirschman gives is of a situation where the reformer’s adversaries seem actually to be conveniently paralyzed by the collective action problem. In another example, success is explained by “a judicious exploitation of a crisis atmosphere combined with a remarkable talent for forming tempo-

rary alliances.” Again, one is supposed to admire the reformer’s skill rather than to seek too concrete a definition of what it is he does. Reforms are “extraordinary feats of contriving in the course of which some of the hostile power groups are won over, others are neutralized and outwitted, and the remaining diehards often barely overcome by a coalition of highly heterogeneous forces.” This perfectly reasonable description is where we seem to have been stuck for decades.

Let me suggest a few alternatives that perhaps provide evidence of the discipline’s advance. We have images of radical instability and total predetermination of outcomes. Perhaps reform is what gets done between the flux of “cycling” and the rigidity of equilibrium. The Rikerian agenda-setter manages to create and relocate rigidity amid the welter of alternative outcomes (call this “reformer-induced equilibrium”). The Northian political entrepreneur perceives how opportunities

created by exogenous changes—shifts in power or interests—can both serve his interest and enhance the efficiency of institutions at the margin. The ideological entrepreneur—this actually follows from Hirschman’s image of ideology, though he does not spell it out—manages to convince powerful interest groups that in order to resolve the problem that concerns them another seemingly unrelated problem must be resolved first. The constitutional—Weingastian?—entrepreneur creates a focal point around which actors coordinate in a world of multiple equilibria.

To close with a question, I found myself wondering whether asking “When do reforms occur?” might actually be akin to asking “When does white win in chess?” To search for a theory of white victories would be foolish. But to explore the process by which the white player won in specific games—to classify the openings, gambits, and end-game strata-

gems—is an obvious pathway to knowledge. This analogy almost definitely goes too far. Still, perhaps we would do better to spend more time trying to be good chess critics, and less on trying to set odds accurately or predict outcomes. With this conclusion I suspect Hirschman himself might agree. ❖

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# Symposium

## The Role of Government in Economic Development Recent Trends and Interpretations

### **The Role of Government in Economic Development: The Debate from East Asia**

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For over two decades, the puzzle of East Asia's seemingly miraculous economic development sustained lively and often contentious debate in the social sciences. Japan's stagnation in the 1990s and the economic crisis of 1997-98 threw that discussion into reverse gear; the question was no longer why the region had succeeded, but why it had fallen so far so fast. In both controversies, the role of government played a central role.

The new growth controversy was sparked by papers by Alwyn Young and Jong-il Kim and Lawrence Lau, popularized by Paul Krugman in a widely-read 1994 article in *Foreign Affairs*. Previous analysis of East Asia assumed that high growth was driven by improvements in efficiency associated with policy reform, the adoption of export-led growth strategies, openness to trade, and technological innovation. The Young and Kim-Lau papers found that productivity growth in East Asia was surprisingly modest. Rather, growth could be traced largely to sheer accumulation of human and physical capital: employment growth, increases in education and massive investment in physical plant and equipment.

Contrary to Krugman, the factor accumulation interpretation does not turn the miracle of East Asian growth into a myth. Rather, it begs the critical question of why East Asia was able to invest at such high rates. Orthodox interpretations emphasized "fundamentals," such as stable prop-

erty rights and macroeconomic policy and market-oriented or "market-friendly" policies. However, the new growth accounts did not rule out revisionist approaches that put greater emphasis on active state intervention. Ha-Joon Chang and Dani Rodrik developed compelling models of the growth process in which "take off" required governments to solve important coordination problems. Even the World Bank's study of *The East Asian Miracle* makes grudging concessions to the revisionist line of analysis pioneered by Alice Amsden, Robert Wade and others.

The writings of both the "fundamentalists" and the revisionists begged the question of why political elites chose the policies they did and why they were capable of implementing them more or less credibly. The seminal work on the role of such political processes in East Asia's growth was Chalmers Johnson's study on *MITI and the Japanese Miracle*. Johnson's primary aim was a revisionist one: to debunk economists' accounts of Japanese development by showing the pervasive role of state intervention in the economy. However the book's more important contribution was in detailing the economic policy-making process, and particularly the independent political role played by a powerful, meritocratic and insulated bureaucracy. The idea that meritocratic bureaucracies could contribute to coherent and credible policy quickly became one of the standard institutional lessons drawn from the East Asian experience.

In the 1980s and 1990s, a flood of work appeared on the political economy of Japan and the East Asian NICs emphasizing the role of the "developmental state." In *Pathways*

*from the Periphery*, I argued that difficult adjustments such as fiscal consolidation, trade liberalization, and devaluation resembled collective action problems that were resolved through a concentration of political authority and executive initiative. The revisionist accounts by Alice Amsden and Robert Wade both drew similar conclusions about the significance of a politically powerful state.

The developmental state hypothesis faced a number of criticisms, the most fundamental of which was the puzzle of why strong states didn't behave in a predatory fashion, distribute rents to cronies or engage in massive corruption. Attention increasingly shifted to an analysis of the political relationship between the government and the private sector, and how it contributed to coherent and credible policy. Campos and Root provided a useful inventory of this new institutional analysis, focusing on the effort to woo political support from big business (through protection of property rights), while delegating authority to relatively insulated and meritocratic bureaucracies and establishing government-business deliberation councils. These institutions increased the flow of information while serving to check both government discretion and private sector rent-seeking.

The stagnation of Japan and the financial crisis of 1997-98 are now leading to yet one more turn of the analytic wheel, in which the benefits of "strong" states and close business-government relations are once again being called into question. As it turned out, strong states *did* behave in a predatory fashion, distribute rents to cronies, and engage in corruption, and these factors contributed to the vulnerability of the countries

in the region to the increased capital mobility of the 1990s. However, more standard tools of comparative political analysis are also helpful in explaining the crisis. In particular, it is clear that constitutional and party arrangements contributed to the ability or inability of governments in the region to respond to the crisis. This can be seen by analyzing the politics of the crisis in the three most seriously affected countries, Indonesia, Korea and Thailand.

Indonesia's political system under Suharto provided for an extraordinary centralization of decision-making authority in the presidency, a concentration of authority that was turned over a thirty year period in the direction of wide-ranging reform. And in the early months of the crisis—in the late summer and early fall of 1997—Suharto once again acted decisively. Yet when policy began to vacillate and show preference to cronies and family members, confidence unwound very rapidly, in part because there were no checks on Suharto's behavior, in part because the regime provided no means for solving basic succession problems. The authoritarian nature of the regime enabled him to cling to power for several more months, during which time the economic damage was greatly compounded.

New democracies in Korea and Thailand also faced tremendous difficulties in responding to the crisis. With a fixed term and substantial powers of legislative initiative, the Korean president would appear well-positioned to respond aggressively to such problems. However, policy-making in presidential systems depends on whether government is unified or divided, i.e., whether the president's party enjoys a legislative majority or support from a majority coalition, and whether the president and party leadership have control over their own party. If divided government pertains, or if the president's party is internally weak, divided or undisciplined, then presidential sys-

tems can produce legislative gridlock.

Korea experienced both patterns in 1997-98. In 1997 Kim Young Sam enjoyed a legislative majority, but his administration fell victim to divisions within the party and ultimately between the executive and the legislature. The source of these divisions was the no-re-election rule, a succession struggle within the party for the presidential nomination, and subsequent efforts of both the presidential candidate and the party in the legislature to differentiate itself from a failed incumbent. As the financial crisis unfolded in the second half of the year, the government was periodically paralyzed, a situation that appeared unlikely to change until after the election. Investors responded accordingly. The significance of these factors was demonstrated clearly when the election re-established a strong executive and produced important policy initiatives.

Unlike Korea, it was virtually impossible to achieve decisive policy leadership under the Thai political system. Thailand's particular combination of parliamentary structure and multiple weak political parties produced a consistent pattern of shaky and short-lived coalition governments. Not only did this contribute directly to the accumulation of economic distortions that underlay the outbreak of the crisis in Thailand, it also greatly compounded the task of dealing with the crisis once it was underway.

Moving from the short-run politics of the crisis to the longer run, there is already a chorus of skeptics on both the right and left arguing that the Asian model has now run out of steam. For neoclassical economists, the crisis validates their longstanding critique of statism and the prediction that Asia's high growth rates were unsustainable. On the left, critics are rushing to underline the dangers of premature liberalization and the various failings of the IMF's

intervention. On both sides, new doubts are being raised about the advantages of authoritarian rule, as Indonesia fared substantially worse than its new democratic counterparts.

It is much too soon to make such judgments. The crisis is most likely to underline once again that there is no single Asian model, and that a diversity of political circumstances underpinned countries' growth, the financial crisis and the prospects for recovery. As before, the analytic task is not to outline a single "Asian model," but rather to define the range of models, albeit with some overlapping characteristics, that have underpinned the region's growth in the past and are likely to continue to do so in the future.

It is certain that the countries in the region will undergo important changes as a result of the crisis, but these changes are not likely to take a linear form; states are likely to pull back from some forms of intervention while simultaneously taking on new tasks. We are likely to see a further reduction, already well underway, of government involvement through industrial policy and controls on the entry of foreign firms. The much celebrated pattern of industrial planning in Japan and particularly South Korea clearly had drawbacks as well as advantages, including the socialization of risk, the generation of moral hazard, high levels of industrial concentration, weak corporate governance and corruption. Notwithstanding the rise of resentment against foreign economic interests, most of the countries of East and Southeast Asia will have little choice but to agree to the further liberalization of key markets, particularly in the financial sector, in order to attract funds from abroad.

However, at the same time as we see governments reducing their economic roles on some fronts, we are likely to see them increase their presence in other ways, particularly with respect to regulation and the provision of a social safety net. The weak-

nesses of prudential regulation have become a central theme in analyses of the crisis. Systems of corporate governance are also likely to change. We are already seeing increased pressure for more transparency in corporate accounting and reporting practices, which will in turn bring pressure for the overhaul of the large, highly diversified industrial conglomerates that have been prominent features of industrial organization not only in Korea, but in Thailand, Indonesia and Malaysia as well.

The economic shakeout is also likely to accelerate the process of political change throughout much of Asia. This is most obvious in Indonesia and Malaysia, but we are likely to see an intensification of pressures for coalitional realignment in other countries as the ranks of the economically disaffected swell. How this will in turn affect patterns of growth in the future will no doubt sustain yet another turn of the analytic wheel. ❖



### Research Priorities in Comparative Politics

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Recently, the *Newsletter* has offered a forum for scholars to debate methodological and theoretical approaches – and shortcomings – in the subdiscipline. Such debates are relatively common across the social sciences. My own reaction to them is that the investment required by a discipline in methodological and theoretical rigor should depend at least in part on the importance of the research question and what we already know about it. For example, in the early days of the unprecedented wave of democratization of the 1980s and 1990s, simple descriptions of the process constituted a significant contribution to our understanding and were a prerequisite for the development of hypotheses and their testing. The only necessary methodological characteristic of a valuable contribution was accuracy. In contrast, in the 1980s we knew much more about the American Congress; therefore, much greater methodological rigor was reasonably demanded of scholarly contributions to its study.

My argument in this note concerns exactly the research questions of comparative politics, and concludes that the subdiscipline would benefit from a shift in its core research questions to a greater focus on explaining phenomena that transcend the narrowly political. This is a presumptuous conclusion, particularly coming from a researcher in political economy whose training is in economics. To defuse the charge of presumption, I hasten to clarify that I am especially concerned about the relationship between comparative politics and the policy concerns that arise daily in institutions that are grappling with the economic development of poor countries – i.e., most of the world.

Top researchers often avoid a focus on policy issues because they typically do not provide space for the theoretical or methodological advances upon which great careers are based. The political economy issues that arise in economic development do not have this drawback. Many phenomena with profoundly political roots, that raise important theoretical and empirical challenges, remain puzzles. For example, under what conditions do governments adopt secure property rights? Why have some countries embraced reform and others not? Do early reforms lay the groundwork for later reforms? What is the consequence for the efficiency of policy outcomes of policy-making structures that favor stability? If one looks cursorily at the comparative politics literature, or to the political science literature more generally, one could easily conclude that more emphasis on these issues is warranted.

Just such a cursory review of the tables of contents of the *APSR*, the *AJPS* and *Comparative Politics* over the last year or so provides a sense of what I mean. I asked whether an article seemed to address the political roots of phenomena such as policy failure or reform, political violence, or the rule of law, or whether instead it dealt with such phenomena as the following: the behavior of political competitors under different electoral rules or informational circumstances, the determinants of procedural processes in legislatures, the conditions under which countries democratize, the determinants of party development or political participation. That is, I roughly assessed whether articles had as their dependent variable economic or broad social phenomena, or aimed at explaining more specifically political phenomena.

Five issues of *Comparative Politics* revealed only eight articles, out of 28, that attempted to explain phenomena outside the political process. Of these eight, five – less than 20 percent of the total – focused on developing countries. In the general

interest journals, excluding those dealing with political theory or philosophy, 33 articles out of 113 were concerned with investigating the political roots of economic or social phenomena. Just one of these 33 articles analyzed developing countries. All methodological approaches, whether rational choice or interpretivist, exhibit this pattern.

One might conclude from these numbers that editorial choices and the universe of research from which journals draw are substantially inward-looking. However, the preponderance of articles attempting to explain inherently political phenomena is not, in itself, problematic. Issues of political participation, party structure and the strategic choices of political competitors during elections all confront interesting questions of political economy. The investigations of whether committee structure in the US Congress is a function of partisan objectives or costly information (which, although ensconced as an issue of American politics is easily seen as a comparative question), or of how social polarization affects voting patterns in fragmented relative to dichotomous party structures or of whether the behavior of political actors changes under different rules governing votes of confidence are all crucial building blocks in understanding how institutions, information and political preferences interact to shape government.

Nevertheless, the ultimate justification for research on issues as large as political participation or as specialized as votes of confidence is the belief that they contain significant implications for government policy-making and affect in substantial ways the well-being of a society's members. This belief is largely untested and the literature rarely analyzes these implications, however. We know much more about elections and political parties in Chile and Zambia than we do about the politics behind the abysmal performance of the state-run copper industry that has

ruined the second country or behind the remarkably good and innovative performance of the state-run copper firm that has sustained the first. Baldly stated, one of the most significant and thorny intellectual challenges facing social scientists is the persistence of poverty in most of the world, and yet one would not guess this from the 150 or so articles that are contained in my crude sample.

Some might respond, reasonably, that important work *has* been directed at the politics of economic development. For example, scholars from Huntington to Przeworski have examined the relationship between democracy and growth. This work is pitched at a high level of aggregation, both institutional and economic, narrowing the range of important political hypotheses related to economic development that can be explored. Another line of research has generated numerous analyses of the political economy of policy reform (structural adjustment, privatization, etc.), but this work is often descriptive, and even when explicit and precise about its hypotheses often lacks the rigorous institutional sensitivity of the best work in American politics. Still another emerging literature has brought new insight to the interaction of political structure – the presence of checks and balances – and the security of property rights, but the variety, emergence and durability of different forms of checks and balances have yet to be explored.

The point is not to disparage these research areas (which is any case self-destructive, since colleagues and I have also toiled in them), but rather to point to the large puzzles that remain. These include such key questions as the political determinants of the shift from arbitrary and capricious to predictable government regulation, from insecure to secure property rights, from the absence to the presence of checks and balances (and the reverse), from the denial of property rights to half the population (say, women) to eco-

nomically inefficient regulation – leading to the disappearance of entire crops or industries – to more efficient government intervention in the marketplace, from governments that let imprudent economic policies continue until crises explode to those that change course early. Each of these issues has been linked empirically to economic development and poverty alleviation; in most cases, though, we are far from a theoretically or empirically well-grounded understanding of the political and institutional conditions under which improvement occurs.

More than the politics of economic outcomes deserves greater attention. For example, although political and especially ethnic violence have been a constant theme in political science, this work has not yet provided lessons offering systematic guidance on how one constructs or reconstructs institutions in post-conflict societies. Countries such as Zaire, Burundi, Rwanda, Bosnia and Indonesia remain major challenges for both international development agencies and the scholarly community. Recent work (e.g., the contributions of Laitin and Fearon) has combined game theoretic insights, familiar from American politics and international relations, with an institutional analysis that is promising in this regard. It emphasizes two useful points: first and less novel (but often underappreciated): social polarization and the political institutions of society ought to be considered jointly when analyzing the roots of political violence. Second and more novel: violence is not only a question of unhappiness with the existing distribution of resources, but also a function of the credibility of agreements among competing social groups.

Much remains to be done to extend these insights and to confront them with competing hypotheses. Case studies will necessarily be a key part of this work, and the greatest illumination will be provided by

individuals with as much understanding of local political competition and institutions as Americanists have of the US legislative process. They will have a special advantage in possessing the detailed knowledge required to uncover the competing hypotheses that might apply in their particular countries or regions, and in being able to uncover the evidence that will allow scholars to observe which of these hypotheses is most robust.

With that observation, it is apparent that the discussion has returned (inexorably) to methodology. Still, just as policy concerns influence my thinking on the choice of research objectives, it also affects methodological preferences, although in a very mild way. One methodological characteristic of the new political economy that is frequently criticized is its reductionism (or, more kindly, parsimony). A little reductionism, however, is a welcome attribute for those trying to demonstrate the importance of political economy concerns for policy. Two characteristics of the best analyses that tend towards the reductionist are the clarity of their hypotheses (both new and competing) and conclusions regarding not only whether a hypothesized relationship exists, but also whether the relationship is important. These are essential in attempting to formulate policy, although neither is unique to the new political economy.

However, methodological issues should not distract from the main message of this note. I sympathize with the view that some shift in methodology should reap considerable intellectual progress, even if the existing mix of research questions remains as it is. However, it seems to me that the main source of future intellectual progress in comparative politics, and even in political science, will come from giving greater attention to political explanations of phenomena and puzzles that transcend the narrowly political. Of course it is crucial to conduct this research in a

way that is rooted in clear and falsifiable hypotheses, but valuable contributions do not need to entail virtuosic displays of econometrics nor sophisticated theories about the role of information asymmetry. For the questions that I consider to be of utmost scholarly and social importance, a wide range of methodological approaches can contribute significantly to advances in our understanding, as long as they exhibit insight (possibly, but not necessarily, framed in formal theoretical terms), a careful statement of competing hypotheses and a diligent collection of facts (whether from elite interviews or votes in a legislature) that are capable of refuting these hypotheses. These are modest methodological demands, appropriate to a set of questions that will challenge scholars for a long time to come. ❖

#### **Trust, Trade and the Role of Government**

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The state plays several essential roles in economic development. The first is as an enforcer of contracts. To be sure, markets can exist without states, but the kinds of markets we identify with advanced economies depend on stable government and rule of law. The second is as the provider of public goods that private industry requires but might not supply: standardized weights and measures, bridges and roads. Once again, all markets do not require governmentally provided infrastructure, but complex and sophisticated networks of trade certainly seem to.

Political philosophers and institutionalist political economists have long recognized these first two salutary effects of state behavior on the economy. There is, however, a third role, which is only just becoming understood. The state can be crucial in reducing the costs of exchange by

reducing the transaction costs associated with exchange. Of course, some states use taxes to line the coffers of officials rather than for public goods or have other policies that shift resources away from efficient uses. States, as North argued, are the source of both manmade economic growth and manmade economic decline. The contemporary ethos of neo-liberalism emphasizes the second half of North's equation, but the contribution of an effective state is more than defense against the Hobbesian "war of all against all." This is not to claim, however, that an efficiency-enhancing state is a sufficient condition of growth. A centralized state neither necessarily ensures against the descent into political violence and economic dissolution, nor, as Michael Taylor, Elinor Ostrom and others have demonstrated logically and empirically, is it the only or even best way to promote positive economic and political cooperation at the local level.

In what follows, I explore factors that produce a growth-enhancing state, that is, a state able to efficiently enact and enforce contract law, provide public goods, lower the transaction costs of exchange and remain relatively stable over time. My emphasis will be on the state as the stimulant of generalized and interpersonal trust. In the huge literature on "social capital," there is relatively little discussion of the role of the state, particularly as it influences the creation of generalized trust. However, government institutions facilitate the production of the trust that lubricates the economic cooperation essential for economic growth, and trustworthy government reduces societal conflict and enhances compliance with taxes, the draft, and other government programs whose efficient operation is essential for enforcing contracts, lowering transaction costs and supplying public goods.

#### ***An Extractive but Growth-***

### ***Enhancing State***

Whatever government does, it must pay for it. How to build the state capacity to efficiently extract resources from the population is one of the keys to both state and economy building. But an extractive state that depends only on coercion is an inefficient state. Efficient extraction depends on the contingent consent or, at the least, the quasi-voluntary compliance of a population, who believes that they are getting something in return for their money and service and that enough others are also paying their share.

How does a state build such a capacity? The initial insights of Schumpeter are echoed by Michael Mann and Charles Tilly, among others: wars result in increased taxation, which is sustained in peacetime. But why does this happen and what kinds of taxes are imposed with what sorts of implications for economic growth and government performance? In *Of Rule and Revenue* I attempt to offer a parsimonious account of variation and development of revenue extraction through time. The ruler maximizes revenue to the state subject to the constraints of her relative bargaining power vis-a-vis agents and constituents, the transaction costs of monitoring and enforcement, and time horizon. Policies are the outcome of a bargaining relationship between the ruler and the various groups who compose the polity. Different rulers have different terms of trade with the key actors inside their borders, but they also have different discount rates. Some are worried about rivals and so are willing to set high rates and, thus, chance alienating subjects and undermining long-term economic growth. Others are fairly secure in their rule and thus are concerned about maximizing revenue over time. Therefore, they set tax rates that encourage economic agents to produce. Predatory rulers do not steal all there is to take. When they are likely to develop revenue systems to promote economic investment and

growth and when they are likely to design institutions that undermine growth is predictable and explicable.

One of my major findings is that cost-effective revenue systems require low-cost mechanisms for increasing compliance among taxpayers and that even the most autocratic rulers depend on a certain amount of quasi-voluntary compliance. The creation of quasi-voluntary compliance requires a credible and valuable return for tax payments. Rulers and taxpayers are engaged in a kind of tit-for-tat arrangement; each cooperates as long as there is some assurance that the others will.

One of the most intriguing puzzles is why democracies tends to be more productive of economic growth. Mancur Olson argues that the more encompassing the interest of the ruler in the polity, the more likely the tax system will provide the proper incentives for growth and the rulers will provide public services in return for their extractions. An alternative, and more compelling, view is that sophisticated rulers and elites created or took advantage of political institutions, such as parliament, that not only facilitated bargaining over taxes but also provided a means to sanction rulers who renege on agreements; in other words, such institutional arrangements made the promises of rulers credible. Credible commitments and self-enforcing institutions significantly reduce the citizen's need to make a personal investment in sanctioning and monitoring government and thus enhance citizen trust of government. Credible commitments, reputational effects, and other such self-enforcement mechanisms that encapsulate interest require, however, institutional arrangements that will produce the feared sanctions if need be. Thus, trustworthy government actors are generally those who are embedded in trustworthy institutions. These institutions can take the form of the rules and norms of professional societies, the grievance procedures available to

their clients and subordinates, or legal proceedings.

### ***How the State Affects Trust and How Trust Affects Governance and Exchange***

Laws, enforced by the state, provide insurance and sanctions against illegal opportunism; trust, on the other hand, lubricates both exchange and governance. Interpersonal trust is essential to economic growth because it facilitates the making of contracts, reduces the costs of exchange and eases renegotiation when the situation changes. DeTocqueville noted how important trust is to business relationships. Avner Greif has explored its role in medieval long-distance trade and Jean Ensminger its role in pastoral economies. Robert Putnam, James Coleman, Francis Fukuyama, and many others have emphasized the importance of trust in the development of complex capitalist economies. What most of them fail to credit, however, is the major role the state plays in creating the kinds of trust that lead to both better government and a more productive economy.

The ability of a state to generate interpersonal trust rests largely on the trustworthiness of the state. The amount of socially and economically productive cooperation in the society affects, in turn, the state's governance capacity and the kinds of economic exchange. Confidence in the trustworthiness of the state has additional consequences for governance as well. It affects the level of citizen tolerance of the regime and also the degree of compliance with governmental demands and regulations. Destruction of the belief in the state's trustworthiness may lead to widespread antagonism to government policy and even active resistance, and it may be one source of increased social distrust. The effect is breakdown in state capacity, even where there is strong governmental infrastructure.

Once confidence in the trustwor-

thiness of the state has been destroyed, its rebuilding often requires extraordinary efforts. The franco-phones in Canada, the Irish in Britain, the blacks in the United States, the aborigines in Australia, and many others who have experienced discrimination (or worse) require compensatory programs and iron-clad commitments to ensure them that—this time—policy promises will be upheld. The effects may be counterproductive, however. Affirmative action programs and special dispensations may inflame those who do not receive the benefits and who consequently believe government is acting unfairly to them. But this is a chance worth taking.

A trustworthy government is one whose procedures for making and implementing policy meet prevailing standards of fairness and which is capable of credible commitments. The assessment of the actual policy can make a difference, but often citizens are willing to go along with a policy they do not prefer as long as it made according to a process they deem legitimate. They are less willing to comply with a policy they like if the process is problematic.

Indications of considerable citizen free-riding that government could but is not controlling are likely to provoke additional citizen non-compliance. So, too, is evidence of discriminatory government practices, violations of policy bargains or poor bureaucratic treatment of citizens. States enforce rights and rules other than those associated with economic and material property contracts. By protecting minority rights, states facilitate cooperation among individuals who have reason to be wary of each other. By legalizing trade unions and enforcing child labor laws, states reduce the costs to workers of monitoring and sanctioning employers—and thus may raise the likelihood of both trust and productivity.

States also help to produce interpersonal trust and reduce transaction costs through regulations directed at

reassuring consumers that they are getting what they believe they are paying for and that they will be safeguarded against a wide variety of human-created dangers. Technically these regulations often constitute a form of contract enforcement, and abuses by the seller are subject to the courts. However, the effect is a kind of generalized trust or, more correctly, confidence in the market and a greater willingness to engage in productive trade. Whether the public is aware of it or not, it is government regulations that provide the backdrop to consumer willingness to give out credit card numbers over the phone, use ATMs, get on and off airplanes, even buy property. Those countries with less trustworthy governments tend to have significantly higher transaction costs of exchange—as so well illustrated in Robert Bates' *States and Markets* or Hernando De-Soto's *The Other Path*.

### Conclusion

The anti-government ideology embodied in neo-liberalism may prove an antidote to reliance on the state for inappropriate tasks and may prove a corrective to inefficient and transaction-cost-increasing regulations. However, the effect of this ideology has been to obscure the important roles the state plays in promoting a productive economy. These are roles the state plays not only historically or in developing countries but also in the most advanced industrial and democratic countries in the contemporary world. And these are roles the state can and should continue to play. ♦

### Reforms, Property-Rights Systems and Development

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The last half century has witnessed many efforts to improve the economic and ecological perform-

ance of developing countries through reforms of property-rights systems. Tragically, many of these reforms have not achieved their intended outcomes. Some have, instead, generated counterproductive results. Reform efforts need to be viewed as experiments that test the relevance or validity of the theory informing policy. Since many reforms of property-rights systems have been based on conventional theories of how property-rights systems affect economic development and the sustainability of natural resource systems, the multiple failures need to be viewed as evidence challenging the relevance or validity of conventional property-rights theory for the affected policy area. Currently accepted theories are based on idealized models of private property and government property while frequently erroneously equating common property with the absence of any property rights.

In conventional theory, the concept of private property is conceptualized narrowly with a primary focus on the right to alienate property through sale or inheritance as *the* defining attribute of private property. The right of alienating property is thought to give a “well-defined” property right. Individuals and firms with a clear and alienable title to a property are posited as being able to make the economically most efficient decision about how to manage properties with the characteristics of purely private goods. This is because the owner, or designated heirs, is assured of reaping the long-term benefits that are generated through investments. If a property achieves a lower than feasible return, others who have the capacity to use it more efficiently will offer to buy the property through market transactions. Market institutions are the mechanisms that enable owners to realize financial returns on their past investments and to shift properties to those owners with the highest valued uses.

For goods and resources that are characterized as public goods, private

property and market mechanisms are predicted to fail. Individuals will either overproduce negative externalities or underproduce positive externalities. The “solution” to market failure in conventional theory is to turn to government ownership. National governments are posited as having superior powers and as having the means to use these powers to increase the productivity and efficiency of economies. Many natural resources in developing countries have been “nationalized” so as to prevent the presumed perversities of leaving decisions about them to private individuals. The failure of these reform efforts has been well documented (for general reviews, see Wunsch and Olowu, 1995; V. Ostrom, Feeny, and Picht, 1993).

A third broad type of ownership — common property — has been treated ambiguously. When a group of individuals has held the rights to access, harvest, and manage a natural resource, but not the right to sell their interest in the resource, these rights have been treated as if they were without economic value. As discussed in more detail below, the term “common-property resource” has been used repeatedly in conventional economic theory as roughly the equivalent of an “open-access resource.” The absence of “well-defined” property rights — meaning the right to alienate — has led to the prediction of overharvesting and potential destruction of natural resources. The policy advice based on this theory has been to make the resource government property or to privatize it.

Trying to achieve greater access to resources by poorer members of a society, while also creating property systems with incentives generating higher levels of efficiency, is a challenging goal. A similar challenge is reforming land-rights systems so that common-pool resources, such as forests and pastures, are harvested sustainably. Both are particularly difficult when the policy tools recommended by conventional theory are

blunt and frequently produce more harm than good. A medicine cabinet with only two prescriptions in it—assign fully alienable rights for private goods or assign a resource to government ownership—is unlikely to generate successful reforms given the substantial ecological, economic, social, and institutional variety that occurs in most countries.

Before one can develop reform tools that are likely to provide access to resources to poorer members of society within the context of institutions that generate efficiency-enhancing incentives, and before one can reform property rights that lead to sustainable harvesting of common-pool resources, one needs a more precise conception of property-rights systems than exists in much of conventional theory. First, it is important to recognize that common-property regimes do involve real property rights and are not simply the same as open-access regimes. There are serious confusions about common property that have generated past misunderstandings and inadequate reform efforts.

It is important to recognize that property consists of multiple bundles of rights and that the right to alienate a property right is a very important, but not the only, relevant right. One has to address the question of “who, what, when, where, and how” of a property system so as to broaden the points of intervention available to policymakers wishing to improve economic and ecological performance. It is also important to recognize that common-property regimes are potentially efficient and more equitable regimes when utilized to cope with particular combinations of ecological characteristics. Further, many common-property systems evolve in conjunction with private-property systems so that a mixed property regime is created rather than a purely private- or a purely common-property system.

#### ***What Have We Learned Related to Future Reform Efforts?***

Many of the previous efforts to reform the structure of governance and property-rights systems in developing countries with the avowed aim of increasing the access of the poor to resources, or saving resources from overharvesting and destruction, have not succeeded. One reason for these failures—but not, of course, the only reason—has been an overly simplistic view of the institutional options available and the presumed ease of implementing policy reforms that will operate as panaceas to solve extremely difficult and complex problems.

Common-property regimes were confused for several decades of policy analysis with open-access regimes where no regulation of access, withdrawal, management, exclusion, or alienation occurs. While many common-property regimes do not confer individual rights of alienation onto users, they have been shown to be capable of achieving relatively high levels of efficiency when used for the management of local common-pool resources. The particular set of rights and how they are organized by common-property regimes varies dramatically from one setting to another as does the performance of these systems. It is important, however, that common-property institutions be viewed as one of the institutional tools that individuals can use (frequently together with private property) to create incentives associated with efficiency and sustainability. Further, access to resources is generally more available to the poor in common-property regimes than in private regimes, holding other factors constant.

After years of reform that tried to eradicate common-property regimes, it is a strange irony that in some policy circles common-property regimes have become the new panacea. This is equally as dangerous as the earlier reliance on government or private property as a panacea. In the forestry sector, after years of recommending control of forests by powerful national agencies

(“custodian forestry”), many policy analysts now recommend some form of devolution to the control of local users. Substantial evidence supports the claim that local users can organize to manage forest resources and do much better than distant, national ministries (Gibson, McKean, and Ostrom, forthcoming). Simply declaring a change in national policy, however, and turning over large amounts of forested land to rapidly created “user groups” may be another overly simplistic policy that will see its own failures in another decade or so.

In a recent briefing paper by the International Centre for Integrated Mountain Development, for example, the following statement was made: “Nepal has currently the most pioneering forestry legislation in the world. It was approved in 1993 and the rules gazetted in 1995. Approximately 350,000 hectares of forest have been handed over to 4,500 forestry user groups” (ICIMOD, 1997). One would think that the comment would have been followed by a cautionary note that a rapid, wholesale transfer of forested land to a very large number of user groups within an extremely short period of time was a danger signal that a new policy was being implemented in a manner unlikely to succeed. Extensive research shows that Nepali villagers do have substantial capabilities for self-organization—especially in regard to irrigation (Lam, 1998). Simply giving them back the forested areas that were taken away from them forty years ago, however, is no guarantee that most of these user groups will manage these lands sustainably and efficiently. By now, many of these lands are substantially degraded and will require costly investments. Fortunately, there are several different kinds of experiments going on right now in Nepal and some of these innovations are being monitored. Consequently, it will be possible in several years to understand which proc-

esses of implementing the massive turnover of forest lands to local users have been the most successful in helping forest users overcome the very high costs of initial organization and of making the investment needed to ensure the long-term sustainability of their recently returned and degraded forest lands.

Policy analysts need to move away from viewing any institutional arrangement as a panacea and learn how to analyze the attributes of a resource system and of the individuals using that system. Sole reliance on the state, the market, or on common property to “fix” any problem is unlikely to succeed. We need to offer a set of intellectual tools that policy-makers and citizens can use in crafting more robust local, regional, and national institutions and in learning how to cooperate effectively with one another so as to achieve higher long-term productivity. Real long-term gains are achieved by individuals who are motivated to work effectively together and are rewarded in the long-term for their efforts. Devising effective, autonomous, and honest court systems and multiple arenas where peaceful contestation can occur is as important as enabling problem-solving individuals to utilize a mixture of property-rights systems well-matched to the situations they face.

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#### A Clash of Capitalisms

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In the face of the Asia crisis and the gathering world slump, deep differences have opened up between the United States on the one hand, and Asia and Europe on the other. The differences bear directly on the American design for a new world financial architecture and on American leadership of the world economy. This note defines the differences, suggests an explanation and ends with some research questions for comparative politics.

#### *The Policy Differences between the US, Asia and Europe*

The policy differences start with the causes of the crisis in Asia, Latin America, Russia and much of the rest of the world.

#### *The United States*

The US government believes that the crisis has predominantly “homegrown” causes. These include undue maintenance of a pegged exchange rate, failure to dampen excess demand and lax supervision of national banking systems. (“Crony capitalism” has become the generic term of choice for these homegrown causes.) In Federal Reserve Chairman Alan Greenspan’s words, “The current crisis is likely to accelerate

the dismantling in many Asian countries of the remnants of a system with large elements of government-directed investment, in which finance played a key role in carrying out the state's objectives. Such a system inevitably has led to the investment excesses and errors to which all similar endeavors seem prone."

Consistently with the "homegrown" or "crony capitalism" theory of the Asian crisis, the United States has proposed a "new global financial architecture" based on transparency, accountability, globally uniform standards, minimization of scope for "moral hazard" and free capital movements.

The International Monetary Fund (IMF) has tended to agree with the US—its relationship to the US Treasury being like the dog's to the talking machine in the old RCA Victor logo. The IMF has enjoyed a near monopoly of the rescue effort in Asia. The Fund has used its control of bailout funds to obtain two kinds of policy changes from the crisis-affected countries. One is to restrict domestic demand using higher interest rates, lower government spending and stiffer taxes, the objective being to stabilize the currency and make it easier to repay foreign debts. The second is to undertake liberalizing reforms in finance, corporate governance and labor markets. In particular, the IMF has pressed the countries to make it easier for financial capital to move in and out of the country (to liberalize the capital account)—though in the wake of the crisis it has also emphasized a complementary strengthening of domestic financial regulation and supervision. ("Regulations, yes, restrictions, no.") The Fund's subscription to the "homegrown causes" theory of the crisis—which points to things far from the functioning of international capital markets—makes it easier for the Fund to require further capital opening as a condition of its bailout loans.

#### Asia

Since the second quarter of 1998 Asian governments have been moving to relax the austerity program, expand domestic demand and introduce various restrictions on the free movement of finance across national borders. This trend accelerated in August and September, leading the *Wall Street Journal* to identify a region-wide policy backlash that constitutes "the most serious challenge yet to the free-market orthodoxy that the globe has embraced since the end of the Cold War."

Malaysia introduced tight exchange controls on September 1, 1998, in order to curb the fall of the currency against the US dollar and permit the government to go ahead with a massive expansionary program. The IMF's managing director said that Malaysia's exchange controls were "dangerous and indeed harmful." US Treasury Secretary Rubin said, "The actions Malaysia took yesterday are of concern to the US and obviously...not the path that we think best lends itself to economic growth and stability over time." Another senior US official warned other Asian countries that they must not follow Malaysia's example. In Asia, on the other hand, the Chinese and Japanese governments expressed support. No Asian country criticized the move. It is telling of the wider Asian response that the respected Thai economist who is a contender to head the World Trade Organization, and the only Asian contender, announced that he found Malaysia's capital controls "reasonable" and "understandable." He presumably calculated that sentiment in Asia and in the world at large has moved sufficiently in favor of capital controls for his views not to cost him the presidency.

In August and September the iconically free-market Hong Kong government introduced restrictions on various kinds of speculative trading against the Hong Kong dollar and Hong Kong stocks in the face of

intense speculative attacks by hedge funds. The government's meticulous explanation notwithstanding, Western banks and investors were furious with the government for daring to intervene to fend off unwanted speculators. Without a level playing field, they said, investors will pass Hong Kong by.

The Japanese government has been considering capital controls for Japan and given its blessing to their use elsewhere in Asia. The vice minister of finance for international affairs, Eisuke Sakakibara, said in September, 1998, that his government wished the G7, the club of the seven major industrial countries, to review policies towards capital liberalization. China has voiced support for these developments.

We now see in Asia a much greater willingness than before for governments to discipline not just labor, but also finance.

#### Europe

Meanwhile, in Europe it has become acceptable for political leaders to subscribe to sentiments akin to those of French Prime Minister Lionel Jospin, who, in a speech in early September, 1998, about the state of the world economy, said, "Capitalism is its own worst enemy. The crises we have witnessed teach us three things: capitalism remains unstable, the economy is political, and the global economy calls for regulation." The French newspaper *Le Monde* declared, also in September, "The policy of *laissez faire* is no longer certain. After years of abstention governments are making a comeback in the economy and are daring to confront the power of global companies." With the new German Social Democratic/Green government of early October, 1998, 13 out of 15 EU member states have center-left governments, and the Franco-German axis looks likely to push the EU in a decidedly leftward direction.

European leaders have been go-

ing out of their way to disassociate themselves from the US plans for world recovery and for the new world financial architecture. The EU is cautiously proposing a new global architecture in which the world would be divided into monetary zones, each with some degree of protection through government controls on the movement of capital.

#### *Asian and European Convergence*

These policy trends in Asia and the EU reflect a little-noticed convergence of views in the two regions, which puts them in opposition to those of the US and the IMF. They are converging on a more skeptical stance towards the merits of free financial markets. On balance, they believe that the Asian crisis had its roots in the radical financial liberalization undertaken by governments of the region over the 1990s, encouraged by the IMF. The fast liberalization, plus domestic interest rates higher than foreign interest rates, exposed their economies to liquid capital inflows. The inflows fueled an enormous speculative balloon in the stock market, the property market, and in industrial capacity. Most of the inflows were in the form of private bank loans to private domestic firms. In the rush to financial deregulation, the build-up of private foreign debt occurred with little overall coordination and regulation by governments on either side of the borrower-lender relationship. When the managers of the capital then wanted to withdraw it all at once, the “reward” for liberalizing capital became a ghastly collapse. Longer-term solutions, say the Asians and the Europeans, therefore require the international financial markets to be more deeply embedded in regulations and restrictions set by both governments and multilateral organizations.

Historians looking back on the “Long September” (August to October) of 1998 may see it as a fulcrum in the world economy since the end

of the Cold War.

#### *A Clash of Interests*

The great slump has brought the policy differences between the US, Asia and Europe to the fore. But what are the interests that underlie the differences between US plans for the world economy and those of Asian and European countries?

#### *The United States*

The US has a powerful interest in maintaining and expanding the free worldwide movement of capital. Trade liberalization has progressed during the past decade to the point where it has been replaced at the top of the agenda of US foreign economic policy issues by capital liberalization.

From narrow to broad, the interests behind capital liberalization are these. First, Wall Street banks and brokerage firms want to expand their sales by doing business in emerging markets, which until the crisis were growing much faster than the home market. They have been restricted by capital controls and a variety of other impediments. They want these impediments removed.

Second, multinational companies also want other countries’ restrictions on their capital markets lifted, but for a more indirect reason. They wish the rest of the world to play by American rules for both international finance, trade and foreign direct investment—to adopt the same arrangements of shareholder control, free labor markets, low taxes and minimal welfare state that US corporations enjoy at home. US firms could then move more easily from place to place and compete against national or regional firms on a more equal basis. This matters especially in Asia, because the Asian system of long-term market relationships and patient capital has put US firms at a disadvantage. The US sees free capital movements as a wedge that will force other economies to change their arrangements in its direction.

Indeed, the Asian crisis is dreadful confirmation that financial liberalization and capital account opening do indeed make it more difficult to sustain the long-term relationships and government industrial policy arrangements that prevailed in the Asian political economy.

There is, in other words, a powerful confluence of Wall Street and multinational corporation interests in favor of open capital accounts worldwide.

Third, US savers and pensioners also have an interest in having their money funds and pensions funds able to invest wherever the returns are highest, without the impediments of host governments.

Finally, the most encompassing interest of all, the US needs to tap the rest of the world’s savings, which is easier if world financial markets are well integrated. The US savings rate is very low by international standards. Its ratio of gross domestic savings to gross domestic product ranks at the bottom among the major industrial countries (together with Britain); its household savings rate out of disposable income is the lowest. To maintain its high level of both consumption and investment, the US must borrow from the rest of the world. The alternative of financing investment out of higher domestic savings would require a sharp cut in consumption (to allow the extra savings), causing chronic recession.

This powerful set of interests explains why the US Treasury has been leading a campaign to get the main international economic and financial institutions to promote capital liberalization. One such effort is the revision of the IMF’s constitution (its articles of agreement) to require countries to commit themselves to capital account liberalization as a condition of Fund membership. Another is the World Trade Organization’s financial services agreement. Many developing countries, particularly Asian ones, opposed this agreement as it was being negotiated in

1996-97. Then came the crisis in the second half of 1997. By December, 1997, the Asian leaders dropped their objections and signed on to an agreement that commits them to open banking, insurance and securities markets to foreign firms. They saw no choice. Either they signed or US and IMF help in dealing with the crisis would be less forthcoming.

Since the summer of 1998, when it became clear that the crisis was spreading close to home, the conversation in the United States has shifted somewhat. Until then, capital controls were dismissed as an idea of the loony left. Now mainstream publications such as *Fortune* and *Business Week* weigh their pros and cons. It is becoming more widely accepted that the deflationary wave will be difficult to stop if nothing is done to limit capital movements. Panic can swamp "fundamentals," and panicked markets need dams, dikes, spillways and other control structures if they are not to cause immense damage. But while the conversation in the United States is shifting, the official US position remains the same, and opposition to capital controls from Wall Street is strong.

#### Asia

Asian governments are undertaking expansionary monetary and fiscal policies to counter the slump. They fear that, without limits on inflows or outflows, they may face a further currency collapse as investors, fearing inflation, again rush for the exits.

In the longer term, Asian governments see capital controls as a way of buffering economies from the instabilities caused by fast inflows and outflows. The Asian political economy is characterized by high rates of household savings intermediated through banks to firms. Firms typically carry high levels of debt relative to equity. On the one hand, this feature has allowed them to invest at a higher rate than would have been possible had they been limited to financing through retained earnings

or sale of equity, and thereby it helps to explain the prolonged fast growth of the region. But on the other hand, corporate sectors with high debt/equity ratios are vulnerable to external shocks and require some protection. The policies and the long-term relationships of "alliance capitalism" and the "developmental state" gave this protection. There is now greater appreciation among Asian governments of the dangers of exposing this type of financial system to fast liberalization—especially in view of the relative lack of experience in dealing with the international capital market and the thinness of banking regulation and supervision. Experience and effective supervision take years to build up.

In addition, the growing Asian sympathy for capital controls is based on the realization that Asian economies do not need to draw on the rest of the world's savings. They are the world's biggest savers (typically saving 35 percent or more of GDP, compared to the United States's 15 percent). They cannot productively invest even their domestic savings, let alone the extra savings that come from abroad.

China's star has risen among other Asian countries for the way it has been relatively little damaged by the crisis, thanks partly to its nonconvertible currency. All foreign exchange earnings must be turned over to the central bank, which sells it for authorized uses. The authorized uses include imports, debt repayments and the purchase of foreign direct investment that brings in technology and expertise. The authorized uses do not include speculation or capital flight.

Asian perceptions of Asia's interests are colored by resentment at American triumphalism. The Malaysian prime minister has warned his people that they risked being "recolonized" by foreign powers. The Chinese government has said of the US, "By giving help it is forcing East Asia into submission, promoting the US economic and political model

and easing East Asia's threat to the US economy."

#### Europe

The German election at the end of September led to a change in priorities. The new Social Democratic/Green coalition government made reducing unemployment the number one objective, not minimizing inflation. This brings the German government much closer to the priorities of the French socialist government, based on the shared conviction of the need to govern markets. Their convergence on demand expansion and employment creation has opened a new tension within Europe between them and the Bundesbank and European Central Bank, which both remain committed to minimizing inflation.

The new German government has fortified the EU's opposition to capital liberalization. European policy makers are worried about the fragility of European financial systems in the face of free capital flows, especially when policy shifts towards demand expansion. The financial systems of continental Europe are dominated by banks, over half of which are government-owned and subsidized. The profits of banks come largely from interest income. (US banks, in contrast, get more of their profits from trading income, such as dealing in swaps and derivatives.) Opening these systems to free capital movements would squeeze banks' interest rate spreads and hence their profits. But the banks are already thought to be suffering from a build-up of bad loans, though the opacity of European financial information makes the quality difficult to judge.

The fragility of European financial systems could jeopardize the launch of the euro on January 1, 1999. The Europeans see the euro and the European Monetary Union as the culmination of 45 years of post-war reconstruction. They see it as above all a political project, and are

prepared to incur sizable economic costs in order to sustain it—a point which American commentators often underestimate. They will do whatever it takes to keep the euro and the monetary union on track in the face of the current global crisis, including controls against sudden changes in capital flows.

Europe also calculates that its policies of capital controls and euro-stability will make the euro a more attractive international reserve currency than the dollar. Europe would then acquire more weight as a shaper of decisions about the world economy than it has today.

Beyond European self-interest, European political leaders have been more prepared than their American counterparts to recognize the damage that volatile money has done in the Asian crisis, and to support capital controls as an integral part of the new world financial architecture. Having longer memories, they also recall that most of the OECD countries had capital controls until the 1980s. The recentness of their removal in the rich countries makes it hard to argue that capital controls are by their nature inimical to growth.

A vast canvas of political battle has opened up. The United States has powerful interest-based reasons to push for free capital flows. The Europeans and Asians see the US proposals as largely self-serving, and have strong interest reasons to resist. Their shared skepticism about the virtues of free capital markets provides the basis for a little noticed anti-American convergence of views between Europe and Asia about the new world financial architecture.

If the world deflation continues, and especially if the Europeans and the Japanese raise the stakes by talking seriously of a new Marshall Plan and debt write-offs as a new basis for world reflation, the United States may make tactical concessions on emergency capital controls as the lesser of two evils. But the differ-

ences in interests are real, and the United States—the Treasury, Wall Street and multinational corporations—will resume pushing for free capital markets world-wide when the emergency passes. Some leftist observers claim, on the contrary, that a gestalt shift away from free markets is well under way. According to John Gray, professor of European thought at the London School of Economics, “A year or so from now, it will be difficult to find a single person who admits ever having believed that the global free market is a sensible way of running the world economy.” If Wall Street crashes and the US economy goes into deep recession, Gray may be proved right. If not—or as soon as the end of recession is in sight—he will be proved wrong. The US interest in free capital markets is very robust.

In the changed political landscape, however, it is by no means clear that the US will win. The crisis has eroded the legitimacy of the idea of a single integrated world market system not only in Asia and Europe, but also amongst activist segments of the US population. It provides us with the opportunity to rethink the basic rules of the international system. We need to create rules and institutions that will allow different kinds of national or regional capitalisms to prosper side by side, rather than force convergence to one basic model.

My argument raises a question about the interests and political power of capital. The policy differences between the US on the one hand, and Asia and Europe on the other, suggest two rival hypotheses. First, capital everywhere has the same interest in free movement, but for whatever reasons is less influential in determining state policy in Asia and Europe than in the US. Or second, national/regional capitals differ in their interests, those of Asia and Europe being more inclined to seek state protection of one kind or another. The latter in turn may reflect

history (for example, the century-long tendency of German capital to reinvest in Germany, giving up higher returns abroad); or it may be more contingent, in line with the saying, “Capital is international when times are good, and national when times are bad.” ♦

### **Theory and Practice, Supply and Demand: Reflections on the Study of Europe**

Christopher J. Makins

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*The Newsletter invited Christopher Makins, Senior Adviser to the German Marshall Fund and author of the recent report *The Study of Europe in the United States* Washington D.C.: European Commission and the German Marshall Fund of the United States, July 1998) to contribute an article based on his report. The report raises many issues likely to be of interest to Section members regarding Europe specifically and area studies more generally. It is available electronically at [www.eurunion.org/whatsnew/index.htm](http://www.eurunion.org/whatsnew/index.htm) and in hard copy from *The German Marshall Fund, Suite 750, 11 Dupont Circle N.W., Washington D.C. 20036* (Tel. 202-745-3950).*

The report *The Study of Europe in the United States* was written to address specific needs of its two sponsors, the German Marshall Fund of the United States and the Delegation of the European Commission in Washington. As such, it was not directly concerned with many questions and concerns that, to judge from recent issues of the *Newsletter*, are animating comparativists and political scientists more generally. From the vantage point of the sponsors, it was not necessary, and would probably not have been desirable, for the report to take sides or offer conclusions on these difficult and controversial questions.

To say this is not to slight those questions and concerns, but rather to emphasize that the focus of my enquiry was a different one. Nevertheless, it would have been hard to complete the report without forming some ideas about the issues with which the *Newsletter* and the discipline have been concerned. The Editor's invitation to publish these thoughts was therefore simultaneously welcome and, for one who has long been outside the academic world, slightly daunting.

It is well to state the premises – whether they be assumptions, prejudices, or judgements – with which I embarked on my research. Several of these, while perhaps self-evident, are nonetheless important to the present purpose.

First, within the worlds of both scholarship and public affairs there is a greater need than ever for sound knowledge and understanding of other societies, political systems and cultures. The countries of Europe are high on the list of those of which this is true.

Second, there is no single or right path to the acquisition of the needed knowledge. The insights and skills of many disciplines and theoretical and methodological approaches are relevant. And many of the complex phenomena we need to know and understand better require the combined contributions of many different approaches. In Europe, one need only think of the emerging Economic and Monetary Union, of the European integration process more generally, of the responses of European societies to new political, social and cultural influences in an increasingly interconnected world, or of the problem of ethnic conflicts and tensions, to see how multi-faceted are the questions on which both scholarly and policy-oriented knowledge is needed.

Third, the academy has a responsibility and a role that goes well beyond the generation of fundamental knowledge for its own sake and

extends to helping our societies cope in practical ways with these challenges. This does not mean that all scholarly endeavor should be directed to producing knowledge that can be applied in practice or that all scholars should be engaged in applied work or in the public policy arena. There will always be a place in the social as well as in the natural sciences for scholars who focus solely on the generation of new scholarship and on fundamental research. It does, however, mean that the scholarly community as a whole has a responsibility beyond the walls of the academy, however it may organize itself to fulfil this. Conversely, it implies a responsibility of individuals and organizations outside the academy to make it possible for scholars to perform this role and to seek out the contributions that scholarship can make to their needs.

Fourth, the reunification of Europe following the Cold War has brought about a more normal state than that represented by the political division of the continent since the late 1940s. While the Cold War may have helped justify the parallel division of the scholarly community along East-West lines, that justification is no longer compelling. For all that there may still be distinctive concerns and challenges in the countries of Central and Eastern Europe arising out of their histories and cultures, it is a real anomaly to perpetuate in the academic world divisions that are disappearing in the world of international relations. Yet many such divisions remain.

Many of the recommendations that my report built on these premises have been accepted by its sponsors. The German Marshall Fund has reorganized, and significantly increased, its program of support for academic research on Europe. The implementation of the European Union's new network of ten European Union Centers at U.S. universities and colleges was in part shaped by the report's recommendations. The

German Marshall Fund and the European Commission Delegation have also recognized the force of the report's challenge to the two professional organizations in the field of the study of Europe – the Council for European Studies and the European Community Studies Association – to play a somewhat different role than in the past in relation to the field. Beyond that, however, the report's analysis had a number of implications for the scholarly community in general and for political scientists in particular.

First, the report did not prescribe any particular organization of the study of Europe or other regions. Nor did it enter into the ongoing debate about the inherent value of area studies centers or other institutional formats for meeting the requirement for both basic scholarship and policy-relevant knowledge of Europe. It was not necessary, given the report's basic purpose, to do so. But the importance of finding some institutional frameworks for a deep and continuing dialogue and sharing of knowledge and experience among different theoretical and methodological approaches within disciplines and between different disciplines was clear. The report's adoption of the phrase 'the study of Europe' in place of the more traditional (and admittedly easier to use) term 'European studies' was intended to signify that the narrow approach that the term 'European studies' had come to imply for many people was no longer appropriate. Not the least reason for this is the remarkable diffusion of scholarly practitioners of the study of Europe beyond the disciplines and departments traditionally associated with the subject into other departments and professional schools. The capacity for, and interest in, sustained dialogue among all the layers in the field

of the study of Europe, not to mention potentially valuable dialogue among scholars concerned with Europe and those concerned with other liberal democracies in North America and Asia that face common problems, has not kept pace with these changes at many institutions. This situation can be remedied only by serious efforts by scholars and administrators within the institutions themselves.

Second, the report stressed the importance of narrowing the gulf, which many think has widened in recent years, between the academic mainstream and the community of people engaged in the public policy debate about Europe (as well as other areas). This situation disserves the interests of all concerned. Not only would the world of public policy be enriched by the greater engagement of scholars of Europe, but the latter could gain a great deal in exchange. Changing the present situation presents challenges to all parties – academics, participants in the public policy world in government, think tanks, funders and other institutions. My observation of all three groups suggests a widespread and quite human tendency for each to look to one or both of the others to take the steps needed to bring about a *rapprochement* among them. Some academics complain that funders deliberately turned away from the scholarly world in order to fund more fashionable policy research and that policy makers and policy analysts are reluctant to engage with scholarly knowledge. The public policy world talks about the increasingly abstruse and theoretical approaches of scholars which make

dialogue about practical issues difficult, if not impossible. Funders may share one or both of these points of view to some extent and worry about how to achieve visible and practical impact from their activities.

The obvious way out of this predicament is for all the parties concerned to look inward and consider how they themselves, rather than others, can contribute to the desired outcome. For funders, as I recommended to the sponsors of my report, the key lies in encouraging projects that can contribute to bridging the gulf and institutions, of which there are a number both within and without the walls of the academy, that specialize in doing this. For the denizens of the public policy world, especially those “policy scientists” that have proliferated in think tanks and institutes over the last twenty or thirty years, the key lies in designing their programs and activities so that there is a larger space for those academics who are willing to try to apply their scholarship to practical policy issues. In the academy there needs to be greater acceptance of the responsibility to contribute to the world of public affairs, not instead of pursuing theoretically and methodologically interesting research questions, but as an additional element of a successful academic career.

This last point would no doubt require a reconsideration of the incentives and reward structures now in place in a number of disciplines, which notoriously privilege theoretical over applied knowledge, at least in the pre-tenure period. Merely to state such a proposition is to underscore the difficulty of bringing

about any such change. But it is hard to avoid the sense that the potential importance of applying the fruits of the more rigorous scholarship that has been the preoccupation of recent issues of the *Newsletter* has disappeared over the horizon of much of the debate. It would perhaps be timely to bring it back into view.

This conclusion relates directly to many other practical concerns that have featured in recent debates. Not the least of these are the vexed issues of the length of courses of study, the need to acquire technical skills essential to more rigorous methodologies and the time to completion of the doctorate. As has often been pointed out, these constraints disadvantage the study of other societies, especially non-English speaking ones, because of the language requirement and the need for field work at some stage in the process. These are indeed serious concerns. They require the priority attention of senior scholars, administrators and funders alike. Without such attention, the process by which language study and field research in Europe have tended to be squeezed out by other requirements, almost certainly to the detriment of both scholarship and public policy, could well persist. With such attention and a modicum of good will on all sides, it is hard to believe that some resolution cannot be found.

A second conclusion relates to continuing institutional divisions along Cold War lines. The principal reasons for breaking down such divisions were alluded to earlier. The only additional point that is worth making is that there would seem to be a tremendous scholarly opportunity in ac-

celerating this change. Countries that were once closed to Western social science methods are now open and represent a significant increase in the number and types of societies in which phenomena important to modern social science can be studied. Recognizing that international institutions have had almost as much difficulty adapting to the end of the Cold War as U.S. academic institutions, there is no reason why the latter should not move more rapidly in the desired direction.

None of the issues raised here is new. All have proved intractable in the past and will doubtless continue to be so. But reviews such as I was asked to undertake are opportunities to raise their visibility and urge new resolve in

addressing them. It is in that spirit that I offer these reflections to the *Newsletter*, in the hope that comparativists will help lead the way to finding new approaches to developing not only a richer understanding of Europe in this country, but also more effective ways of bringing that understanding to bear on the practical problems our societies face in an increasingly interconnected world. ♦

### **Why History Matters**

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Comparative historical analysis has been the basis for much of the most important work in com-

parative politics, as David Collier explained in a recent issue of the *Newsletter*. Using controlled comparisons of a limited number of cases to delineate similarities and differences and probe causal processes, comparative-historical scholars have set agendas of discussion and developed powerful generalizations about issues as diverse as democratization, the rise of modern national states, the roots and outcomes of revolutions, and the nature and effects of public social programs and government interventions in the economy.

Long ago, most qualitatively oriented comparativists stopped examining single cases in isolation. Genuine comparisons across nations, time periods, institutional

## Articles

sectors and public policy areas have been the norm for quite some time. Correlatively, much methodological reflection has focused on logics of comparative causal inference, arguing why in-depth examinations of multiple, robustly defined causal processes from two to a dozen “cases” may generate results equally or more valuable than statistical analysis of superficially specified variables across hundreds of cases.

Less discussed but equally important, however, is the relationship between historically grounded analysis and our theoretical understandings of politics and social change. The turn to history – or, as we would prefer to put it, the turn to the examination of temporal processes – is not just a methodological move, but a theoretical one. Compar-

tivists examine substantial stretches of time not just as a technique for generating cases or scrutinizing causal hypotheses, but because they believe that history itself is an important part of the explanation for significant political outcomes.

We share the belief that the investigation of temporal processes must be central to political scientists’ search for compelling explanations. Yet the claim that “history matters” remains one of the least well-examined domains of social science theory. For comparative historical research to fully flourish, broad assertions that “history matters” or that outcomes are “path-dependent” must be replaced by a systematic effort to develop clear concepts and hypotheses relevant to the exploration of temporal processes.

At the heart of such an effort is an understanding that sequencing can be a critical element of social life. There are excellent theoretical reasons to believe that the order in which events or processes occur may greatly influence the outcomes that unfold over time. Unfortunately, many of the “variable-centered” analyses that are predominant in political science ignore issues of temporal ordering. Investigations typically take a “snapshot” view of societies, and disputes among competing theories center on which factors in the current environment generate important political outcomes. The significance of such factors or variables, however, is frequently distorted when they are ripped from their temporal context.

Explanations may turn not

just on what is happening, but on when and in what order prior things happened. One useful way to clarify the role of temporal order in social processes is to identify different types of sequences and their characteristic consequences. Perhaps most important is an appreciation of the significance of processes involving self-reinforcement or positive feedback. As the economist Brian Arthur has demonstrated, where outcomes are self-reinforcing, social processes will have quite interesting characteristics. Outcomes are not independent of history – of the particular order of events in a sequence. What happens near the beginning of the sequence will be critical, and “small” events early on may have more of an impact on eventual outcomes than “large” events occurring later. Self-reinforcing processes will have a tendency over time to “lock in” to particular patterns. Outcomes that may have been highly plausible at an earlier stage become increasingly remote possibilities over time. In an effort to encourage greater conceptual clarity, one of us (Pierson) has argued elsewhere that the term “path dependence” should be reserved for sequences which reveal this type of self-reinforcing dynamic, rather than being treated as equivalent to the amorphous claim that “history matters.”

As Douglass North, James March and Johan Olsen among others have argued, there is good reason to believe that historical sequences involving self-reinforcing or path-dependent dynamics are widespread in social life. North notes that both the development of institutions and the

establishment of actors’ mental maps of the social world involve characteristics such as high start-up costs and adaptations based on expectations of others’ behavior that tend to generate positive feedback.

The same is often true of collective action processes, which are obviously of great interest to social scientists. For political scientists, it may be especially important to note that power itself may be self-reinforcing. Groups may be able to leverage initial advantages into a capacity to remake the rules of the game in their favor, further reinforcing disparities in political resources.

A review of many of the classics of comparative historical analysis, from Lipset and Rokkan’s work on political cleavages to Gerschenkron’s analysis of late industrialization’s implications, suggests that their claims about the explanatory significance of historical sequences often rest on at least implicit arguments about self-reinforcement. The same is true of more recent comparative work, including *Shaping the Political Arena* by David and Ruth Collier and *Political Parties and the State* by Martin Shefter. A notable recent example is Thomas Ertman’s *Birth of Leviathan*, which can serve as an illustration of the basic logic common to many of these arguments. Ertman’s powerful analysis of early modern state formation mixes an appreciation of historical specificity with a vigorous search for patterns across polities. Historical sequences are at the heart of his comparative argument. According to Ertman, the timing of the onset of military competition determines how states choose to

raise revenues, with those facing early threats obligated to rely on tax farming while those who face this momentous choice at a later date opting to develop proto-modern bureaucracies. Because initial choices of financial systems are self-reinforcing, with powerful vested interests organizing to protect the status quo, these early sequences of events produce distinctive path-dependent processes of state development.

There is then, a striking commonality between what prominent theorists have told us to look for and what a considerable body of empirically grounded comparative-historical research actually finds. Both point to the significance of path-dependent, self-reinforcing historical sequences. While such sequences strike us as a very common and powerful mechanism at work in social processes, they are only one way in which “history matters” for social outcomes. In this brief note, we can highlight only one other: arguments grounded in claims of “non-reinforcing event sequences.” Here the basic idea, as James Mahoney puts it, is that an event or outcome at a particular juncture “may trigger a chain of causally-linked events that, once itself in motion, occurs independently of the institutions that initially trigger it.” The ultimate outcome in such an event sequence is linked to some critical temporal juncture, but unlike the self-reinforcing processes discussed earlier the end point may not reflect the consolidation or “locking-in” of some initial path.

Historical sociologists have so far paid more attention to the influence of events than political scientists; indeed, they have gen-

erally shown far more interest in the effort to develop theories that take time and timing seriously. To provide a basis for theory building, event sequences cannot just be “one damn thing after another” (as in Paul David’s unfortunate definition of path dependence) but must involve clear and generalizable kinds of linkages – what Andrew Abbott calls an “inherent logic.” One promising possibility for our discipline involves arguments about political reactions, in which a critical juncture produces a strong political counter-movement rather than reinforcing the initial mobilization. Such watershed occurrences figure prominently in the Colliers’ analysis of regime development in Latin America and in Gregory Luebbert’s argument about the origins of fascism and social democracy in interwar Europe. In each of these studies, the author points to cases where happenings at an initial juncture triggered a sequence of events eventually culminating in an intense counterreaction. Thus for Luebbert a critical factor in interwar Europe was whether socialist movements had opportunities to mobilize the agrarian proletariat in politics and the labor market. Where such organizing drives occurred, socialists triggered an event sequence which drove the family peasantry into the arms of nascent fascist movements, facili-

tating the latter’s rise to power.

We are suggesting, in short, that students of comparative politics who examine historical cases and temporal processes have major theoretical contributions to make. Institutional developments, political conflicts and power disparities may often be self-reinforcing processes in which conjuncture and sequence are causally decisive. Spelling out theoretically exactly how and why conjuncture and sequence can be determinative is an important challenge for historically oriented students of comparative politics to undertake. In turn, to the degree that we succeed in this endeavor, we will not only better illuminate the general relevance of many of our finest empirical investigations. At the same time, we can challenge adherents of non-historical techniques and theoretical models to rework and improve the stock of theoretical ideas on which all students of politics and society depend. ❖



#### **Book Reviews Needed!**

The *Newsletter* invites doctoral students to submit book reviews for this section. If the book reviewed is recent, of sufficiently general interest to comparativists, and the review thoughtful and of publishable quality, then we will try to find room for it in the *Newsletter*. If you are interested, please contact the Editor or Assistant Editor for further information and style guidelines.

Note to authors and publishers: The *Newsletter* will not find reviewers for unsolicited manuscripts. But if you wish to help fill our bookshelves and landfills, keep them coming!

# Book Reviews

## **Rendering unto Caesar: The Catholic Church and the State in Latin America**

Anthony Gill

Chicago: University of Chicago Press, 1997

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In 1993 the *American Journal of Sociology* published an article titled "Work in Progress: Toward a New Paradigm for the Sociological Study of Religion in the United States" (Warner 1993). In that article Stephen Warner argued that the reigning paradigm for the study of religion had entered into frank decline and suggested new and potentially more fruitful directions and methods for the study of religion and society. Anthony Gill's *Rendering unto Caesar: the Catholic Church and the State in Latin America*, is among the first, and to date easily the finest, study to bring a "new paradigm" perspective to the study of religion and politics in Latin America. The book brings new arguments, evidence and methods to bear on a long-standing question: what provoked the anti-authoritarian political strategies of the Catholic Church in Latin America?

Like Warner's article, Gill's *Rendering unto Caesar* is likely to provoke lively debate. However, where Warner simply points to evidence and tendencies in the literature, Gill adopts a specific theoretical-methodological approach within the new paradigm: the deductivist or rational choice school. Thus, Gill's book is doubly provocative: it champions both a general rethinking of how religious actors affected the struggle against dictatorship and it takes up a theoretical position that is hotly debated currently among many

comparativists and area specialists.

Gill's argument is built around a hypothesis which put simply is this: in states where competition for the hearts and souls of the Catholic population was intense, the Church was forced to oppose dictatorship. Gill derives this hypothesis from the assumption that the Catholic elites are rational actors who always prefer to maximize the absolute number of church members. Gill also assumes that the faithful are rational actors, though he never specifies their preferences quite so clearly as those of religious elites. Following Stark and Bainbridge (1987), he argues that given the resource intensiveness of maximizing (or maintaining) members, the Church has traditionally worked with the state to preserve religious monopoly – via laws and regulations controlling the establishment and function of competing religions – and state subsidization. Given scarce resources, particularly the shortage of priests, and the possibility of establishing or maintaining a religious monopoly, Gill reasons that the Church should only turn against the state when the Church's most fundamental interest – its membership base – is threatened by state action. Gill argues that the break with the state and the move into opposition should have happened only when state policies that hurt the poor coincided with high levels of religious competition. Under those circumstances the poor had an exit option from Catholic religious monopoly and the Church was forced to abandon its preference for state benefits and turn against the government in order to keep the majority of its membership from defecting to other religious groups.

It bears pointing out that Gill's theory is an *a priori* theory about what should be expected from rational actors given a specified set of

preferences and constraints. To make his particular theory plausible Gill has to assume at least three things: (1) that the poor held the church responsible for its preferential option for them or, more accurately, that the local leadership *expected* that the poor would hold them accountable; (2) that the national Catholic leadership in each Latin American state really did believe that the poor evaluated critically the effects of state economic policy; and (3) that the leadership really believed that the poor would, when alternatives were available, exit Catholicism for other religious alternatives in large part as a result of a politicized decision-making process (critical reasoning about the regime's responsibility for poverty, and judgments about the consistency of the Church's rhetoric and action).

If the Catholic elite really did believe those three things then the story is even more interesting than Gill makes it out to be. The preferred exit option of the poor during the period was to Pentecostal religion and the vast majority of Pentecostal leaders during the dictatorship were either apolitical or outright supportive of the dictatorship. This begs at least two questions. First, if the Catholic elite believed that the poor made religious decisions rationally and politically with an eye toward their material interests under the regime and believed that authoritarian economic policies were evaluated negatively by the poor, how did the elite account for the fact that in areas where competition existed the poor exited to churches that opposed their political economic interests? Second, given that its main competitor, the one that supposedly determined the Church's strategy, took either a neutral or a supportive stance toward dictatorship and prospered, why didn't the Catholic church simply adopt

the same political strategies as the Pentecostals? Exploring these questions would have opened up opportunities to develop a richer account of how religious elites and mass publics make decisions about religious options. Gill might have used some of the public opinion surveys taken under Latin America's authoritarian regimes or any of the secondary literature regarding those surveys to show whether or not poor and working class people's attitudes actually matched the Catholic elite's expectations.

In his chapters on the Chilean and Argentine cases Gill does offer some tantalizing supportive evidence from a handful of interviews and primary and secondary documents, but these two cases occupy the extremes of complicity and opposition. The real empirical centerpiece of Gill's book is a probit model – estimated using data from all twelve cases of military dictatorship in Latin America – in which the dependent variable is dichotomous: each national Catholic elite is identified as either publicly opposed to the regime or not. To counter previous works that have suggested increased elite awareness of poverty, human rights abuses or internal Church reform as causes of Church opposition to dictatorship, Gill includes measures of each along with a variable representing religious competition.

The results of the probit are intriguing and support Gill's theoretical argument. However, the probit model Gill estimates is not really an adequate test of his hypothesis. Given that the model describes a long-term historical process, Gill ought to have collected time-series data and estimated an event count model. Absent a test that places the variables in temporal context any statistical model is, at best, exploratory. That Gill failed to construct a more rigorous quantitative test of his theory is easily excusable by anyone familiar with the difficulty of getting good data on the sorts of variables

Gill operationalizes. Nevertheless, longitudinal data on economic development and church growth are available for at least some of the countries included in the study. A more detailed analysis of secondary sources could have tracked the number of positive and negative statements made by national Church elites regarding the option for the poor. That kind of content analysis would have been a more accurate measure of church reform than the cohort-based measure Gill uses and would have better refuted arguments regarding the interactive role of Catholic theology and class-based interests in the emergence of progressive Catholicism.

These concerns aside, *Rendering unto Caesar* points Latin Americanists in an attractive new direction, giving us opportunities to rethink the way religious leaders and followers behave. In a brief postscript, Gill provocatively, if somewhat tentatively, extends the rational choice approach to the study of religion and politics in Eastern Europe and Asia. Gill's postscript fails to fully exploit all of the intriguing possibilities suggested by his theoretical framework, but given the advances he does make, his work ought to be seen as an indication of just how potentially fruitful the deductivist approach can be even in the study of a social phenomenon previously considered to be fundamentally irrational. To draw again on the parallel with Warner, Gill's book represents part of what will hopefully become a "work in progress" toward a shift in the ways comparativists theorize and study religion and politics.

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#### **Labor and the State in Egypt: Workers, Unions, and Economic Restructuring**

Marsha Pripstein Posusney  
*New York: Columbia University Press, 1997*

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*Labor and the State in Egypt* weds careful empirical research with a compelling theoretical framework, and effectively challenges the received wisdom on the role of labor in Third World countries. Recent work dealing with labor opposition and privatization has been unanimous on labor's impotence and inability to obstruct the process of public sector reform. Political economists studying reform in the Middle East have been just as dismissive of labor's oppositional strength, and have attributed the failure of economic reform in the region to state elites' lack of willingness to reform. Posusney responds directly to these arguments, and shows how a central weakness of these "pessimistic" views of labor lies in their (over)emphasis on the state and neglect of societal variables – most importantly, the structure of the union movement, and how the institutional configuration of the union confederation and its ties to the state have shaped labor's resistance strategies. By focusing on "the interplay between the state and society," Posusney explores the role of labor in the Egyptian political economy since 1952, and show how in addition to being a source of support and legitimacy for the regime, labor has also been a source of opposition and effective resistance to regime policies.

Posusney uses an institutional approach combined with an interpretive framework to analyze the bar-

gain of state corporatism in Egypt. Since the goal of Nasserist incorporation was the control of social groups and preemption of mobilization, the corporatist system established several prohibitions, among them the right to strike. In exchange for pledges of loyalty and industrial peace, union leaders were provided unprecedented legal guarantees concerning job security, promotions, and retirement benefits. Workers came to view their wages and benefits as entitlements in exchange for their contribution to national development. Subsequently, Posusney argues, efforts by Sadat and Mubarak to reform the public sector have led to labor protest because such policies are seen as violations of a contract and “attempts to extricate the government from the moral economy.”

Posusney’s central thesis, though, is that the structure of the union movement and the legal environment in which it interacts with the state determine the capacity of labor to resist and respond to different economic policies. The Egyptian Trade Union Federation was established as the single official union organization with a centralized hierarchical structure allowing only one federation to represent workers in any given industry, and the senior leadership was selected and co-opted by regime elites. The ETUF system successfully controlled and insured labor’s support throughout Nasser’s rule – and as long as benefits remained insured. Though Nasser’s goal of creating a structure that would control and demobilize labor was by and large achieved, the corporatist system masked a highly complex and conflictual situation. When Sadat and Mubarak attempted to retrench the state, schisms and conflicts appeared within the confederation system. Posusney examines the Socialist Union laws, the ETUF charter and countless by-laws involving union activity to explain the institutional roots of the conflicts that emerged in the 1990s. Since the Convention of 1961, the

hierarchical structure of the ETUF has been contested by mid and lower-level unionists claiming that they were not represented (44 federations were not represented at the confederation level). Second and third-tier unionists have also complained that collective action across unions is difficult, for the regime deliberately merged federations containing industries with little in common. Lower-level unionists have resented the indirect electoral system established by the 1964 law, for it subordinated the locals to higher bodies.

With the initiation of the privatization program in 1991, the schisms of the ETUF came to the fore and the “crisis of representation” came to a head. Regarding resistance to reform, Posusney shows how, given the institutional context, “leaders of an industrial federation may be hamstrung in addressing sectoral issues until they have gotten the approval of confederation leaders... [B]y the same token, local leaders are straitjacketed when it comes to individual plant issues, if they require consent of federation officials before taking any action” (p. 12). Posusney details the clauses of union electoral law to show how they stifled the initiative of local leaders and restricted their ability to address the day-to-day issues of workers. The upshot of this situation has been that workers have resorted to organizing outside of the union structure through informal organizations and even by joining opposition parties.

Posusney disaggregates the variable “labor” to show how a state-centered approach often misses the different actors and simmering conflict of interests that exist in the labor movement. Posusney addresses a notoriously difficult question: when and to what degree will unionists respond to pressures from below? She conceptualizes the dilemma of loyalty (to the state or to the rank and file) that unionists face during the process of reform by viewing unionists as rational actors who are either

‘sincere’ or ‘opportunist.’ She shows how the institutional context leads to situations where the interests of those on the lower rungs of the union ladder may clash with upper-level unionists. In the context of reform, for instance, senior unionists will react most strongly to national level issues which adversely affect large numbers of workers. Policies targeting the local level, however, will rarely be protested by senior leaders, because the affected segment of the workforce is too small a constituency – although the policies may gore the interests of workers and be protested by local leaders. Posusney provides evidence to show how privatization policies implemented in “one fell swoop,” when the entire public sector is put up for sale at once via subscription sales, have drawn protest from unionists at the national level, but when reform is attempted on a plant basis, upper level unionists are “unlikely to view support for plant-level issues as essential to their legitimacy, and will be prone to ignore them” – and thus, affected workers will resort to organizing and protesting outside the official union structure.

Posusney provides ample evidence refuting the ‘pessimistic’ argument about the Egyptian state’s lack of will to reform and labor’s incapacity to resist reform. Yet in focusing on state will, Posusney gives short shrift to the crucial variable of state capacity – and does not analyze the causes of the Egyptian state’s ‘incapacity’ to discipline social groups. Whether attempting privatization at the national or local level, reformers have been unable to gain the support or quiescence of the rank and file. Posusney notes that policies initiated at the national level are protested by co-opted senior unionists, backed by their constituencies, but, in reality, even if senior unionists approve the liquidation of a particular SOE, affected workers will protest and struggle (often outside the union structure). In sum, if state ca-

capacity is, as one scholar has argued, “the ability of state leaders to use the agencies of the state to get people in society to do what they want them to do,” it appears that the Egyptian state lacks the capacity to discipline and gain the cooperation of workers for the reform project. Posusney does state that “the regime overestimated its own ability and that of labor leaders to win the support of the base for retrenchment.”(p. 172)

From Posusney’s elegant discussion of the incorporation period, it is evident that Nasser’s objective was to demobilize the popular sectors and maximize the state’s autonomy, and, in fact, labor’s docility and support was achieved through generous redistributive policies. However, essential organizational ties of negotiation and co-optation, which in other Third World contexts are provided by the mediating presence of a party, were never established between state and labor. As John Waterbury has observed, “the Nasserist state enhanced its autonomy only by denying itself organized support. The Nasserist dilemma was how to forge a coalition that would mobilize the popular sectors ideologically and include them economically, but exclude them politically.” The Egyptian state’s isolation and absence of more institutional links with labor (and other social groups) has not been lost on Mubarak’s technocratic team. Given the ruling party’s poor electoral performance in urban areas, Mubarak has called for reform and campaigns to gain the vote of workers and draw the support of labor for the govern-

ment’s new development strategy. In short, in analyzing labor’s often-neglected oppositional role, Posusney does not give sufficient attention to the question of why the state has not been able to overcome labor opposition.

In the same vein, Posusney’s argument that the privatization process in Egypt has been unsuccessful not because of elites’ lack of willingness to reform but for fear of labor unrest, is well-supported but incomplete. Notwithstanding societal opposition, a willingness to reform is insufficient for successful privatization because of bureaucratic opposition. *Etatiste* and pro-labor interests exist within the state bureaucracy and often sabotage reformers’ policies. Egyptian reformers’ capacity to privatize hinges on their ability to overcome not only societal opposition (e.g. labor), but also anti-reform vested interests within the behemoth state bureaucracy. A discussion of Egypt’s policymaking machinery, the myriad inter-ministerial, inter-agency conflict, and the limited bureaucratic reform that has been undertaken to insulate and support reformers would be salutary. Finally, successful privatization also involves enlisting the support of the private sector. Clearly, big business’s investment in the privatization process depends on the state delivering an quiescent labor force – and Posusney assigning disproportionate attention to labor opposition is understandable. But the overtures Mubarak has been making to the alienated Egyptian private sector (via the ruling National Democ-

atic Party) are indicative of the state’s vigorous efforts to gain capital’s economic and political backing for the reform process. The difficulty of public sector reform is that the process involves the dismantling of a populist coalition, and the simultaneous construction of a pro-reform coalition. Thus, an analysis is needed of the mix of policies and institutional innovations Egyptian reformers have used to accomplish the daunting task of enlisting allies for the reform process while controlling and neutralizing opponents.

To summarize: in “bringing society back in,” Posusney may have given short shrift to state agency and capacity, but she has written an excellent book and a powerful corrective to the state-centered pessimistic views of labor in the context of economic reform. *State and Labor in Egypt* is a significant contribution to key debates in the discipline – debates involving the politics of economic reform, corporatism and labor politics, and an important step toward integrating the study of Middle Eastern politics into comparative politics. ❖

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