Letter from the President

Comparative-Historical Analysis: Where Do We Stand?

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Following the intellectual success in the 1960s and 1970s of an earlier generation of comparative-historical analysis, led by scholars such as Gerschenkron, Moore, Bendix, Lipset and Rokkan, Tilly, and Skocpol, this approach has been extended and consolidated by a series of valuable studies published in the 1980s and 1990s. This new work includes ongoing contributions by Tilly and Skocpol, as well as books by Luebért, Linz and Stepan, Pierson, and many other authors noted below. In its earlier iteration, this literature played a central role in advancing the idea that countries may follow different paths of national political development, and that political and social conflict are often crucial features of these alternative paths. In both the earlier and more recent iterations, these studies have offered new explanations for outcomes of great political and normative importance: contrasting types of national states and of specific state institutions, national political regimes (e.g., authoritarian or democratic), the structure of national political economies, revolutions and rebellions, political parties and types of party systems, and major public policies, including the creation and reformation of the welfare state.

At the same time that comparative-historical studies have established themselves as an enduring tradition of research, important critiques have been advanced concerning the scope of comparison and the methodology employed. These critiques need to be evaluated as we assess the evolving role of comparative-historical scholarship, which I am convinced continues to make a central contribution to the field of comparative politics.

Comparative-Historical Analysis

The tradition of comparative-historical analysis can be identified in different ways. At the risk of excluding some important studies, I focus on three defining attributes: 1) a sustained focus on a well-defined set of national cases; 2) a con-
cern with a substantial time frame and with the unfolding of causal processes over time; and 3) the use of systematic comparison to generate and/or evaluate explanations of outcomes at the level of national politics. Within what is typically a small-N framework, these studies employ varying mixes of qualitative and quantitative data, with increasing use of the latter as better quantitative data sets have become available.

While many studies possess all three defining attributes, some may lack one of them, and in this sense they have a "family resemblance" to this tradition. For example, they may construct their historical comparisons within a single national case, yet they are centrally concerned with placing that case in a comparative perspective that draws on this broader literature. Other variants are identified in Skocpol and Somers's well-known typology of approaches to comparison.

Although many comparative studies are certainly not a direct outgrowth of this tradition, the continuing vitality of comparative-historical analysis has encouraged a number of scholars to embark on broad, systematic comparison which they otherwise might not have undertaken. I am also convinced that this body of scholarship is part of the inspiration for the current emphasis on multi-country doctoral dissertations in comparative politics. The comparative-historical tradition has in this sense contributed to resetting the parameters of comparison in our field. Given the recent focus on deductive work in many general discussions of theory and method, it is likewise appropriate to underscore the inductive component of these comparisons. In conjunction with bringing a variety of different theoretical and conceptual tools to their research, many scholars in the tradition of comparative-historical analysis are centrally concerned with how the process of comparison itself contributes both to the iterated fine-tuning of concepts, and to the discovery and refinement of new explanations.

Some Critiques

An insightful essay by Ira Katznelson has raised questions about the scope of comparison employed in these studies, and debates initiated by John Goldthorpe and Stanley Lieberson within the field of historical sociology have raised important methodological issues, three of which are addressed below.* Katznelson's concern is that this approach has not succeeded in sustaining its initial creativity and ambition in the subsequent generation of scholarship. Instead, he sees a loss of intellectual momentum, a narrowing of research questions and comparisons, and a failure to command the center of attention in debates on comparative analysis to the degree achieved by the earlier generation of work.

Several observations can be made regarding the scope of comparison. If one considers, from the perspective of the 1960s, the striking novelty of what, for example, Moore or Bendix achieved — with the juxtaposition of large research questions, broad comparisons, and the impressive marshaling of historical evi-

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Please contact the Assistant Editor, David Yamanishi, at falsstaff@ucla.edu for assistance.
News & Notes

The Section's annual Business Meeting will be held on Friday, September 4, at 5:30 pm in the Sheraton Hotel's Beacon B room.

The Nominating Committee of the Comparative Politics Section has proposed the following nominations for new officers of the Section. The nominees will be voted on during the Section Business Meeting (Friday, September 4, 5:30 pm, Sheraton Beacon B) at the 1998 APSA meeting in Boston.

The members of the Nominating Committee were:

- Herbert Kitschelt (Chair), Duke University
- Margaret Levi, University of Washington
- Guillermo O'Donnell, University of Notre Dame
- Elizabeth Perry, Harvard University
- Matthew Shugart, University of California, San Diego

The committee's nominees for the Executive Committee are:

- Ian McAllister, Australian National University, ianmcallister@unimelb.edu.au
- Jennifer Widner, University of Michigan, jwidner@umich.edu

The committee's nominee for Treasurer is Atul Kohli, Princeton University, kohl@wws.princeton.edu

The Gregory M. Luebbert Book Award Committee has awarded the Luebbert Prize for best book published in 1997 to:


The committee's citation reads as follows:

"Gary Cox's Making Votes Count is a book that counts. It makes an innovative contribution to the study of comparative electoral systems through its investigation—both formally and empirically—into the phenomena of strategic voting and regulation of entry into electoral systems. Cox probes the conditionalities of Duverger's law, using these insights to conduct a broad-ranging exploration into electoral coordination and its failures under a variety of institutional configurations. Data from electoral systems around the world are marshalled to test these propositions in a careful and judicious manner. In short, Making Votes Count is an outstanding contribution to the field of comparative politics, one which, in the committee's opinion, is likely to shape research agendas in the study of comparative electoral systems for many years to come."

The Gregory M. Luebbert Article Award Committee has awarded the Luebbert Prize for best article published in 1997 to:


The runners-up for the Gregory M. Luebbert Article Award are:

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dence – one would not necessarily expect that subsequent studies which followed the model of these authors could command the same level of attention. In addition, compared with the grand sweep of Moore and Bendix, many recent studies identified with this tradition have undertaken more limited comparisons, focusing on a shorter time-span, on a single world region, and in some instances on just two cases, or even a single national case. In fact, what these more sharply focused works may lose in scope of comparison they routinely gain in depth of insight, as in the books of Barkey, Bergquist, Chaudhry, de Swaan, Ekiert, Gould, Hall, Luebbert, Pierson, Scully, Skowronek, Wickham-Crowley, and Yashar, as well as Paige (Coffee and Power) and Skocpol (Soldiers and Mothers). At the same time, a number of recent studies sustain a longer historical reach or encompass a broader range of cases, including Downing, Ertman, Evans, Goldstone, Goodwin, Mann, and Silberman, as well as Haas (Nationalism, Liberalism, and Progress), Tilly (Coercion, Capital, and European States), and books on democratization by Haggard and Kaufman, Linz and Stepan, and Rueschemeyer, Stephens, and Stephens.

The more limited and the more sweeping comparisons are both valuable, and this tradition of research gains from the juxtaposition of the two. Hence, I think Katsnelson is too pessimistic about the direction this literature has taken.

With regard to the methodological critiques, the first of these suggests that the political and social processes studied are conceptualized at an excessively macro and aggregated level. This is seen as an obstacle to careful analysis, and especially to the assessment of causation. The idea of "huge comparisons," which was initially advanced as an endorsement of the analytic scope of these studies, can also be understood as suggesting a focus on phenomena that are so broadly defined that it is hard to get strong analytic purchase on causal processes. However, many studies in this tradition in fact employ some variant of a "micro-foundations" approach, in that their explanation of macro outcomes focuses in part on the goals and strategic calculations of individual actors or specific clusters of actors. For example, Liberalism, Fascism, or Social Democracy by the late Gregory Luebbert (after whom the Comparative Politics Section book and article prizes are named) explains broad outcomes, i.e., national regimes and national political economies in inter-war Europe, in terms of the calculations and coalitional options of specific class actors and political parties. Other examples include Goodwin, and Collier and Collier. I might add that the idea of bridging this presumed divide between a macro and a micro focus is underscored in the introduction to Robert Bates et al.'s forthcoming Analytic Narratives volume. These authors, some of whom come out of quite a different tradition from that of comparative-historical work, likewise combine a careful focus on micro-foundations with a strong commitment to explaining broad outcomes. I think more scholars in the comparative-historical tradition should link these two levels of analysis.

A second methodological critique suggests that reliance on J. S. Mill's methods of agreement and difference as basic tools of causal inference is a serious limitation of comparative-historical studies. These methods do not lend themselves to the analysis of multiple explanatory factors or interaction effects, nor to the incorporation of a probabilistic view of causation or any notion of measurement error. Yet in fact, many works of comparative-historical analysis employ a complex mix of tools for causal inference, including not only the matching and contrasting of cases entailed in these two methods of Mill, but also ordinal comparison and diverse forms of "within case" analysis, including process tracing. Used together, these tools provide a considerably stronger basis for causal inference. No one imagines that the use of such tools can deal with error, or with a probabilistic view of causation, in a way that is equivalent, for example, to regression analysis. They can, however, offer other kinds of insight into causation not provided by regression analysis or other large-N approaches. This suggests that scholars may face an important trade-off in the type of data, and the corresponding tools for causal inference, with which they choose to work.

A third methodological critique of comparative-historical research concerns its extensive reliance on secondary sources, and especially the filtering of information and the particular interpretations of events built into such sources. It is indeed true that scholars engaged in research on many contemporary, as opposed to historical, topics may have more control over the data available to them. In addition, it is certainly admirable when scholars doing comparative-historical analysis can utilize primary sources (e.g., Yashar, and Skocpol's Soldiers and Mothers), although currently this approach may have more adherents in historical sociology than in political science. In fact, the practice of basing comparative studies on secondary sources is widespread in political science and is hardly a distinctive issue in comparative-historical research. Further, given the availability of a massive monographic literature on major historical features of politics and political economy in countries after country, it would be a great loss if scholars did not build synthetic research on this potentially invaluable "database." In doing so, more schol-
ars need to follow the practice that is essential in analyzing any kind of data: being explicit about rival interpretations of the data (in this case, including the interpretations contained in the secondary sources) and candid about the implications of these interpretations for the overall conclusions of the study.4

The use of secondary sources also has a distinctive strength, in that it lends itself to the replication of research.5 Thus, again, there appears to be a trade-off in terms of alternative methodological priorities. In works of comparative-historical analysis based on secondary sources, the bibliography in effect identifies a data set that can be accessed by any scholar to reevaluate the findings of the study. Whereas the political science discipline has had to establish elaborate norms and procedures for making available the data sets employed in published quantitative research, with comparative-historical studies the secondary sources identified in the footnotes and bibliography typically constitute a body of data that is routinely available through libraries.

These methodological critiques thus raise three issues that practitioners of comparative-historical analysis need to consider carefully. Two of them, concerning the macro-micro linkage and the need to move beyond Mill’s categorical methods, are in fact already being addressed by some scholars in this tradition. In addition, existing practices have compensating strengths, as with the alternative kinds of insight into causal relations that derive from small-N research and the relative ease of re-analyzing secondary sources. Hence, one must think in terms of trade-offs, and these trade-offs require close examination.

Conclusion

In conclusion, I would like to underscore two other points. First, there is an ongoing need for synthetic studies that integrate the findings of books like those discussed above, and especially for efforts at synthesis that cut across the different substantive topics addressed in this literature. Such synthetic work could give us greater insight into what is and is not being accomplished by undertaking comparisons of this scope. Second, and relatedly, the theoretical underpinnings of this tradition of research require more attention. For example, along the lines of current work by Paul Pierson, the idea of “path dependence,” as applied to political analysis, needs to be developed into a more fully articulated set of arguments about the historical and institutional foundations of political change.6 Such refinements should call attention to an advantage of this approach: the longer time frame within which causal processes are examined creates an opportunity to move beyond taking actors and preferences as given, and to consider instead how they are constituted. The effort to strengthen these analytic underpinnings will help to bring into sharper focus the distinctive contribution of the comparative-historical tradition.

1 A bibliography of comparative-historical studies and methodological commentaries relevant to this letter is available at: <http://www.polisci.berkeley.edu:9000/faculty/dcollier.html>.


3 See the introduction to Collier and Collier, Shaping the Political Arena, as well as James Mahoney, “Nominal, Ordinal, and Narrative Appraisal in Macro-Causal Analysis” (American Journal of Sociology, forthcoming).

4 On dealing with these rival interpretations, see Ian Lustick, “History, Historiography, and Political Science,” American Political Science Review 90:3 (September 1996), 605-618.

5 For a valuable debate on replication, see this Newsletter 7:1 (Winter 1996).

Is There an International Division of Labor in Comparative Political Science?

Editor’s Introduction
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Although we study politics in other countries, most of the officers of the Organized Section in Comparative Politics teach in and publish from the United States. At the officer’s annual meeting, held at the 1997 APSA meetings, we decided to ask colleagues in other nations to tell us something about the differences between what we do in the US and what they do in their home countries. We were interested in a number of issues. Particularly 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 deeper and richer than what those of us working in the US can generate anyway. But does this mean that practitioners in the US will end up doing non-specific, increasingly abstract “theory” while comparatists in other countries will do the nitty-gritty descriptive work? Is there, in other words, an increasing division of labor in the study of comparative politics? And whether there is or not, how do we describe the salient differences between comparative politics as practiced in the US and comparative politics as practiced elsewhere? The three essays that follow, drawn from three regions of the world, speak to these concerns, opening a debate that we hope to continue in future issues of the Newsletter.

Comparative Politics in East Asia:
A Discipline for the Nation, or of the Nations?
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Decades before Plato wrote about the philosopher king, Confucius in China established the precept that the state should be run by learned persons, and that the ultimate goal of a scholar is to take power and rule the nation by virtue. While the philosopher king remains an ideal in the West, scholar officialdom in East Asian society has been not only a fact but also a social class. In prewar Japan, university professors belonged to the imperial bureaucrats. The Koreans respect their scholars not only for their knowledge, but also for their active involvement in politics (no matter which side) and even for their intellectual authority.

It is thus especially common for political scientists in these nations to be caught in conflicting roles. As students of the science of statecraft, these scholars not only study their nation, but also are expected to serve the nation. An intriguing question is thus the state of comparative politics (if there is such a discipline) in the East Asian nations. Can it be comparative at all? If yes, is the researcher’s nation just a data point in the spectrum of possibilities? Or is the nation’s unique history the sole source of variation?

From several perspectives, the case of Taiwan may shed light on the above questions. First, the nation is undergoing a tremendous political transformation that cannot be fully explained by tradition or history, which provide insufficient variation in the independent variable. A comparison with other regimes is required. Second, political science in Taiwan has been under strong influence from abroad, especially the United States. Usually more than half of the faculty members in the political science department of Taiwan’s leading universities or research institutes are U.S. trained PhDs who are familiar with the latest developments in political science.

The above two features make Taiwan a critical test for the hypothesis that there is an international division of labor in comparative politics. If, with an obvious need and sufficient capacity to produce theory-oriented (or at least theory-linked) research, political scientists in Taiwan still focus on single case studies, it is more difficult for scholars in other Asian nations to be theory-builders. To obtain a general view, I examine the articles published in the latest issues of Taiwan’s major political science journals: the Chinese Political Science Review and the Taiwanese Political Science Review. I investigate the following questions: What is the distribution of fields? For articles that may be counted as comparative politics, how many involve some minimal comparison with other nations? What are the major research topics? What are the most commonly adopted research ap-
approaches?

Before we examine the data, a problem of definition has to be clarified. In the simplest sense, comparative politics is the study of "foreign" countries. In the United States, for instance, domestic politics of any non-U.S. country can be listed as a subject of comparative politics. Ironically, the situation in the non-U.S. countries is exactly the opposite. While political science departments in the U.S. tend to distinguish American politics and comparative politics into two separate fields, in most non-U.S. countries comparative politics studies the domestic nation. Accordingly, there are two ways to evaluate whether a work in this field (in a non-American country) meets the standard of methodological rigor. First, a study can be genuinely comparative, in that enough countries are selected to constitute the database for a systematic study. Second, the cases in the study can be sub-units (such as voters, time periods, or electoral districts) in a single nation. Empirical findings in such a study are usually more statistically reliable than a cross-national comparison among, say, five nations. Such a project can still be comparative if the findings are compared with similar studies conducted about other countries.

The latter type dominates articles in our sample. In the 13 articles (62%) that can be counted as comparative politics, only two involve some minimal cross-national comparison, and the key research questions are predominantly about Taiwanese politics. Nevertheless, 10 out of the 13 comparative politics articles are quantitative analyses, and many employ sophisticated statistical tools. The topic most frequently addressed is electoral and voting behavior, geared to different research questions such as democratization or regime transformation. All in all, these articles are similar to their counterparts in American politics in research design, if not in their conclusions.

At the superficial level, such a phenomenon is not difficult to explain. The behavioral approach has been a major source of inspiration for Taiwanese scholars trained in the 1970s and 1980s. Regularly held elections are not only key to Taiwan's democratic transition, but also provide a rich database for analysis. It is simply impossible for researchers in Taiwan to neglect the importance of electoral study. The nature of electoral surveys also makes it easier to reach an economy of scale. To conduct these surveys requires not only mammoth funding but also organized teamwork. As a result, electoral study in Taiwan has been transformed into an industry from which regular products can be anticipated.

Of course, comparative politics in Taiwan is not all based on electoral surveys. Partly influenced by the global shift of research paradigm in comparative politics, two research programs emerged in Taiwan in the last ten years. The first concerns positioning Taiwan's experience in the transition from authoritarian rule in the global resurgence of democracy. Numerous projects on this issue have been conducted, some through international cooperation. The second project involves a dialogue between new institutionalism and democratization theory. As Taiwan moved to the phase of democratic consolidation, the most pressing issue became the question of how divergent demands (e.g., responsiveness vs. representativeness) can be met by institutional engineering. New analytical frameworks, especially those related to rational choice theory, are being proposed to give prediction and prescription about Taiwan's institutional choice.

To summarize, the development of political science in Taiwan parallels that in the U.S. to a greater extent than many would expect. The major difference is that the Americans study their own country in the field of American politics, while the Taiwanese scholars do so in comparative politics. Nevertheless, one should not hold the impression that comparative politics in Taiwan does not compare at all. What matters is the strategy of conducting a comparative study. Conversant with the current developments in comparative politics, methodologically sophisticated scholars can use their nation as a crucial case to either confirm or reject widely accepted hypotheses. When countries outside the U.S. all adopt this strategy, the international division of labor facilitates the development of comparative politics by encouraging scholars in different regions to utilize their comparative advantage. Concentrating on one's own country is less costly than collecting data on many comparable foreign nations. It is also easier for readers to understand and appreciate research products in which they are the protagonists. Properly framed, country-specific studies can still be compiled to answer some theoretical questions.

If there is an international division of labor among comparative politics specialists in different countries, it is because the factors of production are distributed heterogeneously. It is impractical and unnecessary for a scholar to produce highly abstract theories if his/her country is facing an urgent and idiosyncratic problem. What really matters is whether there is an international trade between the theorists and the area specialists, and whether this trade is fair and profitable. In the Taiwanese case, at least, there seems to be a trade deficit. Taiwanese scholars import far more theoretical products from the U.S. than the U.S. imports empirical products from Taiwan. It is unlikely that the situation in other Asian countries is any better. However, it is hard to tell which side benefits more from this unbalanced intellectual trade.
Comparative Politics is a difficult scholarly discipline to practice. It requires conceptual clarity, some theoretical sophistication and a lot of knowledge of the politics of two or, preferably, more countries. It also demands, at least from a European point of view, some, preferably a great deal of, historical awareness. These overall capabilities are rarely acquired in most universities (and rarely found in most scholars). If and when scholars become fully aware of the need for these overall capabilities, most of them still tilt towards some of them: either towards a theoretical knowledge, that is made of sets of notions without a specific theoretical perspective, or towards a historical (theoretical) knowledge, that is not based on an appreciation of the historical components of two, or several, concrete political systems.

Unfortunately, much of what goes under comparative politics is often simply the juxtaposition of two (or three, rarely more) country studies, or subsystem studies. One could do even worse. For instance, it is ironic, though quite revealing, that the Book Review section of the APSR labels all single-country studies as Comparative Politics and that American politics, a typical case of specific country studies, is entitled to a section of its own. Of course, most country studies — and even more studies of American politics — are not genuinely "comparative politics." Some selected few country studies could, however, depending on their theoretical sophistication, qualify, if they advance our knowledge by obliterating the practitioners of the field to better shape or reshape their approach and analyses on the basis of some theoretical knowledge. So far this translation of theoretical progress into the analysis of the politics of specific countries has been extremely infrequent.

All this premised, one has to underline that there has been some improvement in the field of comparative politics since the sixties. I would take as a starting point the publication of Almond and Powell, Comparative Politics: A Developmental Approach, and of the journals Comparative Politics and Comparative Political Studies. However, and obviously, there had been excellent studies, both in book and article-form, published before then and, of course, there are bad books and useless articles still published now. The dividing line does not run between American scholars, who are doing "theory", and European scholars, who should be actually doing the hard work of (empirical) comparison. After all, some of the best scholars in the field of comparative European politics are European scholars working in US universities and there are many good US scholars working, for instance, in the field of comparative Latin American politics. It may also be true that many European scholars are obliged to "go comparative." In fact, in order to provide a decent understanding of the workings of one's own polity, comparison with other political systems is absolutely indispensable. On the other hand, many US scholars have believed and still believe that their political system has been and is "exceptional" and, therefore, that there is nothing to be gained and a lot to be lost by relying on a comparison with other political systems. Only recently has this assumption been challenged, but the results of the various analyses and studies have so far affected only a limited number of US political scientists. Most US scholars in the field of US politics remain adamantly convinced that their political system is unique and they neither contribute to nor take advantage of comparative political analysis.

However, and exactly because they are coming from different traditions and utilizing different perspectives, all scholars in field of comparative politics should be aware of three major tasks to accomplish with the goal of producing improved and more convincing results. These three tasks can be better articulated in terms of imperatives: first, the imperative of conceptual clarity and precision; second, the imperative of historical knowledge and sensitivity; third, the imperative of institutional awareness (in order to understand and take account of the constraints and opportunities offered by norms, rules, structures). With reference to these three tasks, one can also evaluate in a more precise and more fruitful manner the differences between US and European political scientists and suggest ways to make some methodological and substantive progress in the field without necessarily producing undesirable and, probably, impossible uniformity.

The first task, that of conceptual clarity, presents two problems. On the one hand, there remains both a tendency to be sloppy in the definition of concepts and, at the same time, to stretch some concepts excessively. On the other hand, there is also the tendency to believe that one's own research and analyses are so important and innovative that they absolutely need new concepts, or new meanings injected into old concepts, to be satisfactorily expressed. Unfortunately, not only does this terminological or conceptual innovation confuse the concrete results even of useful research, it often also prevents the testing of research and the accumulation, that may require or not some reorientation, of new knowledge. In some cases, old concepts are given new, unnecessary, unilluminating meaning; in other cases — as for instance with the
concept of democracy — myriad adjectives are called into the picture not just to allow a better understanding and a more precise classification of different types of democracies, but to distort its fundamental meaning altogether. It would, of course, be extremely presumptuous to suggest what to do in a totally unequivocal and non-negotiable manner. However, if scholars want fruitfully to engage in comparative politics, an agreement should be reached on the way to “treat” the fundamental terms of politics. Briefly, one can build upon ancient and revered concepts by starting with their classic, history-sanctioned meaning and duly justifying, with reference to history, theory and research, the additions and the changes introduced into that particular concept. While it may not necessarily be true that European scholars always perform this task with the desired thoughtfulness, it may be true that US scholars have not engaged intensely in the terminological debate and have ended up producing, or contributing to, a tower of Babel effect. One may hypothesize that, in some cases, the “tower of Babel effect” is the product of the exposure to other cultures and of (a misplaced) cultural relativism. Building on their tradition and on the study of political thought, it seems to me that European scholars have, in general, been less likely to fall into this terminological-conceptual trap. This short article is not being the best place to engage in sociology of knowledge and of the savants. Therefore, let me simply conclude that the best starting point for the production of satisfactory results — and by this I mean “capable of being intersubjectively appreciated” — is a wise use of terms, meanings, and concepts.

The second imperative to be taken into account in comparative politics concerns the importance of historical knowledge and sensitivity. On the whole, it remains true that most political scientists do not really know how to come to terms with history (and, therefore, try to do without it). It is also very true that most, if not the majority of historians are unwilling and unable to present their analyses and explanations in terms acceptable by and useful to political scientists. This is the famous controversy between nomothetic scholars — the social scientists in general — and idiogetic scholars — precisely the historians. The controversy is by no means over; it is only subdued. However, not much of politics, and even less of comparative politics, can be learned and practiced without the knowledge and the use of the history of particular countries, of particular subsystems, of particular phenomena. Apparently, perhaps because of their training, perhaps because of their cultural, academic, and political environment, European scholars have been more inclined and more capable of making some use of history when engaging in comparative politics.

The classic and outstanding reference here is the work of Stein Rokkan. The Norwegian scholar must indeed be cited because not only was he able to resort effectively to comparative history for comparative political purposes, but he was also capable of providing excellent case studies of Norwegian politics framed in a comparative perspective. Obviously, some topics cry out for a comparative historical-political perspective while others may be susceptible to a different treatment. To be more precise, no comparative study of the processes of democratic transition and consolidation is bound to produce appealing results without being grounded in the history of each particular country and most institutional studies would lose a lot in relevance when they were not capable of taking into account the way institutions were constructed, shaped, utilized, and reformed; the way they were, at the same time, the object of a constant struggle as well as they way they were defining the terms of that struggle, creating constraints and opportunities for all the different players.

Perhaps nowhere else than in the rallying cry “bring the State back in” can one find a paramount difference between comparative politics as practiced in the United States of America and by scholars trained in US institutions and the European tradition and practitioners. Purified of its ideological component (the State being not surprisingly exposed as a non-neutral actor in the class struggle), that perhaps was the driving force for many left-wing US political scientists and sociologists, the “bring the State back in” school succeeded in “bringing” to the attention of US scholars something that in Europe had never failed to constitute an important, irrepressible object of analysis for European political scientists. Confronted with the process of European unification, the State may have been obsolete, or perhaps obsolescent, but it was ob-sinate (both adjectives come from Stanley Hoffmann’s characterization). And most European political scientists were perfectly aware that any analysis of their polity and any comparison with other polities implied some knowledge of the State and of its institutions and would produce a better understanding of both, especially thanks to the multiple possible comparisons. Conversely, without bringing the State in and assessing its institutions, most analyses would remain suspended in a vacuum and there could not be any comparative advantage. Thanks to neo-institutionalism and to some (but certainly not all) game-theoretic approaches, institutional analysis is back and may contribute a lot to our comparative understanding of the workings of other polities as well to the designing of democratic institutions. Both fields of analysis appear to be as exciting as they are growing, quantitatively and qualitatively. While it is obvious that some institutional analysis can
be carried out without relying on comparisons, but losing something, the contrary is less likely. More precisely, most European scholars would probably stress the need for comparative politics to be aware of the importance of institutions, as arenas, as sets of constraints and opportunities, as legacies of the past and as promising harbingers of the future.

Perhaps, it is possible at this point to draw some preliminary conclusions. Though refraining from sweeping generalizations and taking into consideration not the best of the practitioners, the cream of the cream, but the whole of them, one could possibly draw two portraits. On the hand, many US scholars would be found to be not especially or adequately careful when dealing with various terminological problems, more interested in theorizing and/or engaging in country studies, less sensitive to the importance of historical knowledge, and only recently, though still a minority, not very appreciative of the impact of institutions on comparative analysis. On the other hand, one could find most European scholars unwilling to get rid of the various terminological and conceptual problems and, indeed, trying to build on the tradition of European political thought; not rewarded either by their country-specific audience or by the European academic community when producing single country studies, indeed encouraged to go comparative and, to a large extent by training and because of their respective national cultures, almost obliged to take into very serious consideration the role, the impact, the consequences of their States and their institutions. Obviously, many US scholars enjoy a very high, and often well-deserved, visibility, deriving from several factors among them the language, the sheer multiplications of citations due to the number of practitioners, and the several specialized journals available to them. However, it is interesting to underline, and this symposium, together with others already held, though not very successfully, in some leading journals, is a testimony to a very simple, important fact. Even US specialists of comparative politics are somewhat dissatisfied with the state of their discipline. Perhaps, if some of the points I have made are correct, then the by far numerically inferior European scholars in comparative politics are in a position to offer more than a couple of useful suggestions and guidelines.

Two Academic Traditions
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South of the border, political scientists seldom do comparative politics. They mostly study and write about their own countries. The reasons are multiple. First, comparative research is expensive and, besides Brazil and Mexico, most Latin American countries lack a well established and stable network of funding institutions to support research projects at universities and other academic institutions. Second, until recently, it was difficult to sell a comparative project to funding institutions—either international or public ones—or to other governmental agencies. International foundations had their own agendas; governments, when not hostile to academic activities, looked mostly inward and found comparative studies useless. Above all, the way scholars related to their societies and to real politics pushed them to concentrate on domestic issues. In most Latin American countries the borders between academic life and political participation were and still are thin and blurred. Prominent economists, political scientists and sociologists (especially the first group) draw their reputations as intellectuals as much from their engagement in national political debates as from their academic work.

Even scholarly legitimacy hangs, at least in part, on the success obtained outside the university's walls. Quite often, one's intellectual reputation depends on being frequently in the media as much as on outstanding academic work. On the other hand, academic prestige may be a gateway to a political career either in the legislature or the administration. To speak about current national problems and dilemmas is required for achieving public recognition.

The long periods of authoritarian rule of different flavors contributed to consolidating this particular way in which politics and academic endeavors intertwine. During the 1970s, under the new authoritarian regimes, small private multidisciplinary research centers, generally funded by international agencies like the Ford Foundation, IDRC, and SAREC, played important roles in maintaining free intellectual activities. In some countries, like Chile and Argentina, they took the place of university departments, which were under tough governmental control, as the loci of research and intellectual production. Private research institutes such as Cedes and Cisea in Argentina, and Flasco, Cieplan and Academia de Humanismo Cristiano in Chile were crucial to the survival of political science—as well as sociology and economics—in both countries. Even in Brazil, where the university system expanded and retained its research capacity, private multidisciplinary centers such as Cebrap, CedeC and Iesp were very important for Brazilian political science. In those centers academic research and opposition to the military government were closely associated. The study of the conditions that led to the rise of bureaucratic-authoritarianism, of its peculiar political institutions, of its social basis, of its internal tensions and of the conflicts among its supporters was supposed to lead to a better understanding of its liabilities—and therefore of the
opportunities to be explored by the democratic opposition. Researchers themselves frequently had close relations to opposition politicians and organizations, contributed to create or support opposition magazines and journals and often thought themselves playing a political role qua intellectuals.

When in the 1980s new democracies finally replaced the authoritarian regimes, scholars’ relations to political life continued to be close. This is true, especially for the generation of scholars – which happens to be my own – that entered academic life immediately before or during authoritarian rule. In Brazil, prominent political scientists participated in the creation of the center-left parties in 1979; some of them became professional politicians and others combined academic activities with participation in public political debate. Political conditions shaped the research agenda. The central issue of this agenda was the consolidation of democracy under very difficult circumstances, due to a history of political instability and contemporary conditions of deep economic disturbance.

Concentration on domestic issues did not completely exclude comparative scholarship nor necessarily gave birth to atheoretical narratives and idiosyncratic descriptions. Sometimes it did, but not always.

The assertion that developing countries shared some important features has been an ingrained assumption for generations of Latin American scholars after World War II. ECLA’s theory of center-periphery relations and the nature of Latin America’s industrialization process were crucial, in the 1960s and 1970s, to generate a comparative awareness in good political scientists of the region. The studies of populist politics, corporatist interest intermediation, industrialization policies under conditions of dependency, patterns of expansion of citizenship rights, and bureaucratic-authoritarian regimes had a clear comparative frame. Brazilian political scientists studying Vargas’ populism knew they were facing a phenomenon which was not a domestic idiosyncrasy but belonged to a family of events that also occurred in Argentina, Colombia, Mexico and so forth. For those who studied industrialization processes, the state’s structures and capacities as well as policy traditions were crucial issues long before our North American colleagues thought it necessary to bring the State back in. And again, Latin American scholars knew that State-led industrialization was not a national peculiarity but a pattern to be found in most developing countries.

The same can be said about recent efforts of Latin American scholars to understand the combined processes of democratization and economic reform in the 1980s and 1990s. Along with the comparative research sponsored by ECLA under the name of Proyecto Regional de Reformas de Políticas Públicas, some good national case studies, produced in Argentina, Brazil and Mexico, show a strong comparative awareness.

On the other hand, serious political science scholarship, past and present, in Latin America, is not devoid of theoretical concern. In the 1960s and 1970s, the relations between economic development and democracy made up the core of political scientists’ studies. The breakdown of democracies stimulated a critical revision of modernization theories. New theoretical approaches related the specific nature of economic development in Latin America – especially the social tensions and constraints posed by import-substitution industrialization – with the rise of bureaucratic-authoritarian regimes. If patterns of economic development were central to explaining democracy’s failure, other factors of a more political nature were also thoroughly scrutinized. The relation between populist politics and political instability was explored. Especially in Brazil, a number of studies tried to show the importance of state corporatism in controlling labor unrest and submitting unions to populist governments. Brazilian political scientists also related encompassing state intervention and the great decision capacity of insularized bureaucracies to the existence of a fragile party system, thought to be one important dimension of democracy’s fragility in the past.

In the post-authoritarian period, structural explanations of political issues lost importance, giving way to actor-oriented and especially institutionalist theoretical frameworks. New studies on the breakdown of Latin American democracies in the 1960s and 1970s emphasized the importance of the political elite’s behavior and strategies. The radicalization and confrontational strategies of elites were said to explain the demise of democratic regimes.

Institutionalist approaches were central to the studies that focused on the new Latin American democracies. Conditions of stability of new democracies or, more precisely, governability in a difficult economic setting became the central issue. And governability was thought to be a question of institutional design, of establishing adequate institutions. In Brazil, political scientists focused on electoral and party rules, state structures and forms of government in their relation to the workings of democratic regimes. In the 1980s and early 1990s, there was a huge debate between prominent political scientists about proportional representation, the fragmented multi-party system, presidentialism and federalism, because of their effects on governability. In Argentina, political scientists debated the causes and consequences for democracy of “decisionism,” i.e. the executive’s capacity to impose on the legislative branch, either through ac-
crused legislative powers or pressure and political blackmail. In Chile and also in Uruguay, political scientists discussed the role of pacts – among parties and among social actors – to obtaining governability and democratic stability. In any case, institutions are at the core of scholarly inquiry.

For Latin American scholars, theory certainly matters. When studying populism, bureaucratic authoritarianism, processes of economic reform under new democratic regimes and so on, political scientists were and are aiming to use mid-range theories, much in the same way as the historical institutionalists of North America. You might say they are searching for mechanisms, in Elster's sense of something “intermediate between laws and description.” Nevertheless, Latin American political scientists relate to theory production in ways somehow different from North American scholars. They hardly ever design research projects to test theories. These are instrumental to explaining real world problems, which political scientists are supposed to make sense of for their peers and especially to lay audiences. In part for this reason, political scientists don’t feel compelled to begin a paper by explicitly laying out their theoretical assumptions. And although university departments and research centers are far from being peaceful places, the preference for formal theories or for mid-range historically-sensitive theoretical approaches will never lead to the kind of academic warfare so common today in U.S. political science departments. The exposure to multiple intellectual influences from the United States and Europe made Latin American scholars prone to accepting the possibility of choosing different theoretical approaches according to the problem to be dealt with.

In short, I am trying to suggest that there is no such a thing as an international division of labor between North Americans (producing general theories) and Latin Americans (doing good or bad descriptive case studies). What we have until now are two different academic traditions, whose main features stem from the different ways the academic enterprise has been institutionalized and is socially legitimized in the United States and in Latin America. I say until now because I think that things are changing in the Latin American countries where academic institutions are stronger and where scholarship is more institutionalized. It seems that a new generation of young Latin American political scientists is emerging which is more distant from the political arena and closer to the North American style of work. It is possible that the intellectual tradition I have tried to describe will tend to wither away as our troubled political experience becomes history and as democracy asserts itself as the only game in town.

1 See ECLA’s Serie Reformas de Política Pública, especially the volume organized by Juan Carlos Torre.

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Good Reads

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Since my research is in the area of comparative political economy, what free time I have to read is largely concentrated in that area. Here I want to highlight three studies by economists that speak to our concerns as students of politics but that, given normal disciplinary boundaries, might not receive the attention they deserve in political science. These studies have all greatly influenced the nature and direction of my own work, and I thus recommend them quite regularly to graduate students. I consider that they are helping define some of the most important questions for the next generation of comparative studies.

Nicholas Barr (1992) has written a somewhat unusual review article for the Journal of Economic Literature, because it proposes a reinterpretation of the welfare state more than systematically reviewing the existing literature (although a lot of literature is touched on in the article). The interpretation is in my view plausible and important. Barr proposes that social policies have efficiency, not only equity, aims and justifications. The basic idea is that the welfare state protects against risk in situations where private markets (i.e. insurance) would not arise to do so or would do so only inadequately. Private insurance, Barr contends, cannot properly protect against such contingencies as unemployment, inflation, and medical risks (p. 754). These risks need to be covered in order to secure such efficiency objectives as income smoothing and the protection of living standards. Much of the article consists of detailed reviews of particular social policies—unemployment insurance, public pensions, and public medical care foremost among them—in light of this reinterpretation of their origins and goals, drawing on case materials from 10 OECD countries.

Barr explicitly contrasts his reinterpretation of the welfare state with the two standard conventional explanations: that social policies arose as a response to the logic of industrialization, and that they arose because of the ideological and political commitments of leftwing governments to working class constituencies. He suggests that each of these alternatives may explain some portion of social spending: the convergence theory may explain the initial emergence of a relatively small welfare state and the ideological theory, as he labels it, may explain the difference between the thin welfare state found in the US and the universal welfare states of Sweden and other social democratic countries. But Barr suggests that a substantial portion of state spending—perhaps something on the order of the 12–15 percent of GDP that all OECD countries spend on social services—exists in order to insurance citizens against excessive risks.

Barr notes two interesting and interrelated political implications of the argument that the welfare state has an efficiency rationale. The first is that, if a large underlying chunk of the welfare state is economically efficient, it is no surprise that even governments ideologically committed to reductions in state spending—such as those of Margaret Thatcher and Ronald Reagan—have been largely unable to effect roll backs of the welfare state. The second is that it is also no surprise that the welfare state is not as redistributive as some might prefer, and that middle class constituencies benefit as much or more than the truly needy. Neither of these observations is new, but both are put in a slightly different context by Barr’s reinterpretation of state spending as economically efficient. Indeed, in my view, Barr’s piece lays a research agenda for exploring anew the origins and growth of state spending in OECD countries.

In his NBER Working Paper, entitled “Why Do More Open Economies Have Bigger Governments?” Dani Rodrik (1996) has extended David Cameron’s (1978) study of the relationship between the size of government and economic openness to a much larger set of countries—in essence, the entire world. This is such an obvious extension—at least once we see it in action—that it is remarkable that no one seems to have thought of doing it before. Cameron’s original article, based on data from 1960 to 1975, covered 18 OECD countries, and made no claims for a more general argument. In his study, Cameron found that economic openness (measured as imports plus exports divided by GDP) was the best single predictor of increases in tax rev-
enue. Countries whose economies were relatively exposed to the international economy, in other words, also had relatively high levels of government spending. Cameron interpreted this as reflecting the strength of organized labor in the small, relatively open Social Democratic countries of northern Europe.

Rodrik’s data set encompasses more than 100 countries, as of the late 1980s and early 1990s. The measure he uses for the size of government is real government consumption; his measure of openness is the conventional one also used by Cameron. His results basically duplicate those obtained by Cameron: countries with more open economies have higher levels of government consumption. In a particularly convincing move, Rodrik repeatedly tweaks his regressions, using slightly different measures of the dependent variable, different data sets, different sets of countries, different sets of control variables, and slightly different periods. This allows him to demonstrate that his basic results are robust to these various changes in model specification.

In line with Barr’s interpretation of the institutions of the welfare state as mitigating risk, Rodrik interprets his results as showing that where populations are more exposed to the turbulence emanating from world markets, governments perform “an insulation function” (p. 13) by creating a larger “safe” sector that is sheltered from the international economy. According to this view, governments spend money to mitigate risks, both by creating social safety nets and by providing government jobs.

Rodrik thus offers a plausible extension of Barr’s thesis to a new domain. At the same time, Rodrik extends Cameron’s analysis both empirically and conceptually, in ways that are at once natural and convincing. For students of comparative politics, Rodrik’s work reiterates more strongly claims already made by Cameron: namely, that under-

standing government spending requires bringing in the international context.

Another study to highlight the increased importance of international factors in analyzing “domestic” politics is a forthcoming Journal of Economic Literature review article by Robert Flanagan entitled “Macroeconomic Performance and Collective Bargaining: An International Perspective.” Like the pieces that normally appear in the JEL, this is a review article, surveying some two decades of research studying the impact of collective bargaining institutions and practices on macroeconomic performance in OECD countries. But Flanagan also proposes two interrelated, original hypotheses. The first is that the impact of collective bargaining institutions and practices on macroeconomic performance may well have varied systematically over the past three decades. This may explain why studies in this area come to such different conclusions, some finding that highly centralized “corporatist” bargaining arrangements promote good economic performance, others that decentralized, laissez-faire arrangements do at least as well. Flanagan posits that these differences may be due to distinct temporal effects. Flanagan’s second hypothesis extends the first. The impact of labor organizations on economic performance, he suggests, should be studied within the context of economic openness; that is, the extent of national integration into the world economy may systematically change how labor institutions affect macroeconomic performance. It may be, for instance, that in the most recent period—the 1990s, approximately—collective bargaining institutions no longer have any systematic effects on macroeconomic performance, which because of increased openness to trade, are instead driven largely by international factors. (Alternatively, Flanagan also notes that a growing nonunion sector appears to carry with it some of the same effects as those stemming from increased openness; the latter phenomenon is of course especially relevant to a country such as the US.) If this is true—and it is a hypothesis that remains to be verified—it would explain why union officials claim to feel so irrelevant today even though their organizations and bargaining arrangements remain largely intact, at least on paper.

Like Barr’s essay, Flanagan’s offers a novel set of hypotheses to guide future research. And like Rodrik’s, Flanagan’s work emphasizes how indistinct the boundaries between comparative politics and international relations have become. All three of these pieces qualify in my mind as “good reads”: works to stimulate our imaginations in new directions.

References
Dissolution: The Crisis of Communism and the End of East Germany
Charles S. Maier
Princeton University Press, 1997

Reviewed by Daniel Ziblatt
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The 1989 collapse of the East German communist regime—and the demise of communism in general—has resurrected a whole series of old and important questions about the nature of change, the role of human agency in revolutions, and the apparently inevitable decline eventually faced by all political regimes. Did political and economic structural constraints imposed by Leninism doom the East German regime? Or was communism’s collapse in East Germany, as Claus Offe has provocatively argued, the result of a “contingent” and even “accidental” chain of events?

Charles Maier’s new book on the end of communism in East Germany undertakes to explore these questions and does so with grand Weberian goals of synthesis. His account—rather than pushing for one particular agenda—seeks to step back and provide a synthesis of the many, recently published accounts of communism’s demise in East Germany (see McFalls, 1995; Joppke, 1995; Kopstein, 1997; and Offe, 1997). The two goals of Maier’s work are the following: to provide a narrative of the end of East Germany and to account for the crisis of communism. This task involves both comparing (though not systematically) East Germany with other Soviet states as well as pointing to certain features of East Germany that reveal weaknesses and tensions in west-ern “modern” societies.

The synthetic element of his argument is twofold: first, in classical Weberian fashion, Maier attempts to provide a balanced account of the “long term” structural and cultural influences that made the decline of communism appear inevitable. In his first chapter “Losing Faith” Maier presents an engaging and convincing cultural analysis of the relationship between coercion, consent, loyalty, and legitimacy in the East German regime from its founding to its demise. Maier argues that the East German regime preserved the loyalty of its public through a precarious balancing of coercion, privilege, and utopian-idealism. This balancing-act, was according to Maier, also a source of the regime’s undoing. Maier cites Sir Lancelot’s revealing and fateful lines in East German playwright Christoph Hein’s 1989 work The Knights of the Round Table: “Arthur, do you know that the people outside don’t want to hear any more about the Grail and the round table? Before they respected us . . . today they only laugh if they see a knight of the round table” (p. 4). Indeed, as faith in the utopian “Holy Grail” of communism unraveled, the justification for brutal coercion weakened, and Maier asserts, the rulers “no longer believed in their own original political vision” (p. 57).

But the decline of the socialist vision’s legitimacy is only half the story. In Maier’s next chapter “Economic Collapse,” we are presented with the structural and apparently irreconcilable economic and political contradictions faced by the East German elite. During the 1980s East Germany sought to manage a rapidly growing international debt, and, since constrained by its trading obligations to their COMECON partners, it confronted increasing pressure to import Western goods and technology. Maier writes, “The GDR was caught between its need for Western goods and its dependence on the assured demand for its exports to the Soviet Union” (p. 66). Maier delves into recently-opened SED archives to arrive at his central contention that these economic problems were rooted in a more basic failure of political will. Maier asserts that while SED economic policy-makers had the opportunity to adapt to more “flexible-specialization” production in the 1960s—investing in computer technology, for example—the SED was constrained institutionally and ideologically. With a slow-down in capital investments, increasing debts, weakening trading partners in the east, and no capacity to compete with the West, the GDR elite faced a viscous circle of economic disintegration.

Maier’s well-balanced account points to both the cultural constraints of a waning commitment to the “idea” of a communist East Germany and the structural constraints of a decaying economic base—both of which led to a long-term process of decline. But Maier does not end his argument here. These impersonal cultural and structural “forces” neither made the collapse of East Germany inevitable nor necessarily logical. The collapse of communism was not an agentless “implosion.” It is here that Maier provides his second synthesis. While Maier emphasizes that certain structural and cultural constraints caused “strains” and “stresses” within East Germany, the second half of his work is focused entirely on the actions, events, and moments of indecision experienced by the East German protest
movement, scrambling eastern political
elites, and exultant western politicians.
The middle chapters of Maier’s work provide an engaging and insightful narrative of the events of 1989-1990—
tracing the actions of political elites, the
formation of new political parties and the
doomed fate of the short-lived, but
decisive, East German Bürgerbewegung. Maier focuses on
the strategies, tactics, and interactions of protesters and political elites, though
he grounds his account in the cultural and structural realities facing the GDR
regime. His work, in this sense, achieves what many scholars pronounce
as a goal but few accomplish: the balancing of agency and structure and the
avoidance of the excessive voluntarism of the “transitions” literature that dominates political science accounts of regime change.

Maier’s emphasis on the “protagonists” of political change within the context of the structural and cultural constraints imposed by Leninism provides the most comprehensive and insightful account to date of East Germany’s
decline. Nothing in Maier’s account is inevitable, yet Maier illuminates the
potential social tensions that undermined East German society in the late 1980s. But Maier’s exclusive focus on East Germany—with only unsystematic references to other East European cases—leaves several questions to be explored for students of post-communism: what is the actual relationship between cultural and structural features of decline (lack of legitimacy and economic decline) and the particular political choices pursued? Given these structural and cultural constraints, were there other outcomes really possible? If so, which? How do we determine the relative weight and relationship of these two “forces” of history? Only by working within a systematic comparative framework—that explores the fate of other communist states—can these questions
be addressed. Maier’s work simultaneously explains the dissolution of East Germany and points students of post-communism towards further research.

Gatekeepers of Growth: The International Political Economy of Central Banks in Developing Countries
Sylvia Maxfield
Princeton University Press, 1997

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There has recently been a proliferation of works dealing with the relationship between the globalization of markets and the institutions of the nation-state. Sylvia Maxfield’s latest book, Gatekeepers of Growth approaches this issue by offering a compelling model to account for the degree of central bank independence found in a nation-state. She argues that the degree of independence enjoyed by the central bank is determined not only by the prevailing

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(continued from page 3)


The Sage Prize Committee has awarded the Sage Prize for best paper presented at last year’s APSA to Joel Hellman of the European Bank of Reconstruction and Development for “Winners Take All: The Politics of Partial Reform.”

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financial condition of a country, but also the political temper of the ruling elites, shaped by their outlook and preferences. According to Maxfield, central bank independence depends on four factors: it is most appealing to countries with 1) a larger need for balance of payments support; 2) a greater expected effectiveness of signaling of creditworthiness; 3) more secure tenure for their politicians; and 4) fewer restrictions on international financial transactions.

Maxfield focuses on the link between an interest in the independence of the central bank and the unfolding of certain historical events. For example, after the fixed exchange rate regime collapsed in 1971, bank independence was seen as the way to limit price instability. Subsequently, a decline in monetary policy effectiveness and the desire to reconstitute this effectiveness increased the attractiveness of bank independence. She contends that financial globalization raises the cost of poor monetary policy and increases the value of central bank independence as currency traders and investors observe central bank actions closely and take them as signals of future national economic policy and asset performance.

The author lists the different characteristics of asset types which would affect policy decisions. First, foreign direct investments, which cannot be quickly liquidated, are not very responsive to changes in the macro-economic environment and therefore are not greatly affected by central bank independence. Second, foreign equity shares are more liquid, responsive to changes, and more likely to choose exit rather than try to influence the course of change. Third, international bank loans involve few actors, have local presence, and can afford to be less responsive to the market environment. Finally, foreign government bonds are most affected by central bank independence. Therefore, the message sent to investors by central bank independence is least effective when foreign direct investment is predominant and most effective when foreign government bonds are held.

The substance of Maxfield’s argument for her model lies in four case studies, in which the complex interactions of the four factors, each exerting its influence to varying degrees, yield a net force toward or away from central bank independence. In the case of Thailand, it is very clear that changes in politicians’ security, objective indicators of balance of payments status and stock creditworthiness, and the composition of foreign investment contributed to the changes in the central bank’s independence. For example, Prime Minister Sriti increased central bank independence in 1957 because he was secure in his position and the other three major factors remained relatively unchanged. Later, in 1973, when democratization jeopardized governing politicians’ tenure security, international credit became easily available, and the country enjoyed good balance of payments, the independence of the central bank declined. Under Prime Minister Prem, political stability, deterioration of balance of payments, and contraction of international financial resources, once again led to independence for the central bank.

In the case of Mexico, the permissive international financial conditions in the 1970’s lowered the cost to political leaders for increasing government spending, and allowed them to nationalize the central bank. The rise in international portfolio investment, growing short-term debt, and financial deregulation around 1993, however, made President Salinas’s move towards central bank independence attractive.

Meanwhile, South Korea’s case shows that if objective conditions do not put pressure on political leaders to give priority to the competition for international financing, they may rely on the central bank for greater economic stability.
tional capital, the leaders are free to follow the dictates of domestic political economy. In the 1950's and 1960's, tenure insecurity and abundant international aid minimized the desire for central bank independence. In the 1970's and 1980's, it was heavy government regulation of the financial sector, strong export performance relative to debt, and favorable interest rate on foreign loans which kept the need to compete for creditworthiness low, and thus further postponed the independence of the central bank.

Finally, in Brazil, the relatively strict regulation of domestic and international financial transactions lessened the need to compete for international creditworthiness. The decline in foreign direct investment and foreign lending, and a military coup led to the creation of a central bank in 1964. In the 1970's and 1980's, however, the uncertainty of presidential succession, and the low need for international capital or creditworthiness served as a low incentive for central bank independence.

Maxfield claims that the cases represent a wide range of experiences and contexts, "chosen independently of any knowledge of variation in their need to compete for capital internationally or their level of central bank authority" (p. 71). Although such practices in case selection are valuable and encouraged, it is difficult to believe that a scholar with Maxfield's knowledgeable background would be able to choose her cases so randomly. But granting the methods by which the four case studies were selected, it would be instructive to test the robustness of this theory in cases chosen randomly by other scholars. Another question remains concerning the formulation of the author's model. She leaves unclear whether the theory is inductively derived from comparisons or is a deductive model justified by cases.

Furthermore, Maxfield does not adequately assign a weight to each of the variables. According to her analysis, the balance of payments condition seems to precede every other variable in importance. Since the politician's decisions are irrelevant except when the need for capital grows, it is not clear why the politician's decisions are not treated as an intervening variable instead.

Maxfield assumes that politicians share a common set of tools and priorities under the global market, but there are other ways of governing the economy than the one espoused by liberal economic thinking. She leaves out the possibility that some countries would not even consider central bank independence a policy option. For example, communist countries had once believed in the virtue of a command economy, choosing to follow a different path from the rest of the world. In discussing the case studies, Maxfield too easily dismisses the role of ideology as a contributing factor since she assumes that everyone accepts or needs to heed the basic liberal economic ideals. If there indeed has been a fundamental change in the international system that forces states to conduct economic activity in one fashion, she fails to represent convincingly such a situation. Even if her assumptions are correct, the present situation may be reversible. One cannot overlook the possibility that politicians might oppose central bank independence because they simply believe in a different economic theory.

These problems not withstanding, Maxfield's book is an impressive achievement. Her analysis extends to the level of individual policy-makers taking into account their motives and concerns. Although her theory has a difficult time framing or weighting politicians' incentives vis-à-vis global economic forces, she deserves credit for her attempt to tackle the concerns of policy-makers (unlike many statist or

### CALL FOR PAPERS

The 1998 International Studies Association-Western Regional Conference will be held at the Claremont Graduate University's School of Politics and Economics on October 9-10, 1998. The theme of this year's conference is "Globalization and the State: Sovereignty, Security and Society." This year's program chair is Lewis Snider (Lewis.Snider@cgu.edu). The Conference coordinator is Ana-Mari M.Hamada (Ana-Mari.Hamada@cgu.edu). The keynote speaker is Dr. Pedro Aspe, former Minister of Finance under President Carlos Salinas De Gotari and currently
institutionalist arguments). Compared to legal and pure sectoral arguments, Maxfield's approach has much more to offer. Legal and pure sectoral arguments often explain broad cross-national differences in central bank authority and view them as relatively static. The problem with this approach is that they do not sufficiently consider the variation in explanatory factors to account for changes over time. In contrast, Maxfield is able to show very convincingly the particular changes in different countries over the span of history using her model.

The model Maxfield presents is very parsimonious. She limits the number of independent variables to just a few, and engages them in straightforward rational choice thinking in determining the significance of each. On the other hand, this same factor might make it an oversimplified model for capturing the complexities of central bank independence in countries other than the ones she analyzes. Nevertheless, the simplicity of the model helps the reader discern the major contributing factors in decision-making. The book is clear, engaging presentation of a usually technical and abstruse subject. *Gatekeepers of Growth* is a good comprehensive book for specialists as well as laymen who are interested in learning about the functions and consequences of central bank independence and the role of political actors in the decision-making process.

**Making Votes Count: Strategic Coordination in the World's Electoral Systems**

Gary W. Cox

*Cambridge University Press, 1997*

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Duverger's Law has been one of the most important and controversial tools used to understand electoral and party systems since his book *Political Parties* appeared in 1954. Since that time scholars—including Sartori, Riker and many others—have refined and/or discounted the hypotheses presented by Duverger. Gary Cox provides an important advance in the field of electoral studies by developing a unified game-theoretic model to explain strategic coordination in electoral systems and its effect on parties. This approach provides a formal basis for the conditions under which electoral systems will diverge from a Duvergerian outcome. Cox goes beyond theory and provides concrete empirical tests of the hypotheses he develops.

The dependent variable in this study is strategic coordination, including both strategic voting and strategic entry by parties. Strategic voting focuses on the incentives for individuals to vote sincerely at the district level. Strategic coordination by political parties determines whether or not a party will enter an election or join forces with other parties. Three main independent variables are used to explain strategic coordination: electoral institutions, political motivations and public expectations. The types of electoral systems studied in the analysis include: proportional representation (PR), single-member simple plurality (SMP), single non-transferable vote (SNTV). Each of these systems requires a different type of coordination on the part of voters and candidates.

In part I of the book, Cox develops a general theory of strategic coordination which can be used to describe a variety of electoral systems. The basic model which Cox uses is a coordination game, similar to the Battle of the Sexes game in which a couple would prefer to spend the evening together, but have different preferences as to where they should spend the evening. Voters often face a similar dilemma.

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when choosing between candidates. In an election with more than two candidates, if they choose their preferred candidate even when he has no chance of winning, their least preferred candidate may win. Therefore voters may choose to vote for the candidate closest to them who has a chance of winning. For parties, a dilemma is whether to enter a candidate at all or to coordinate with other parties.

Strategic coordination between parties and voters is not the only determinant of the number of parties in a system. Cox notes that sociological critiques of Duverger's work make the argument that social cleavages are more important than electoral systems in determining the number of parties in a system. Cox argues that social structure need not be ignored when examining party systems. He explains that the nature of electoral systems is not only influenced by the type of electoral system chosen, but also by the nature of the cleavage structure within the society. An electoral system provides an upper bound on the number of candidates which may compete in an electoral district in equilibrium. A society with few cleavages may have fewer candidates than expected in a PR system. In the following sections of the book, Cox goes on to show that a variety of other factors may have the effect of constraining or increasing the number of candidates in a district.

Cox begins Part II of the book by developing a general classification of party systems, focusing on the translation of votes into seats and the building of electoral coalitions. He goes on to analyze strategic voting in each type of system examined by Duverger, including single-member dual ballot systems. This section is useful in that it details the factors in each type of electoral system which will influence strategic voting. Cox's main finding is that the number of viable candidates or lists in an electoral district is \( M + 1 \) (\( M \) refers to district magnitude) in equilibrium.

In order to detect strategic voting, Cox has developed an empirical test. He begins with the hypothesis that there are two types of equilibria in the presence of strategic voting, Duvergerian equilibria in which trailing candidates will be deserted and non-Duvergerian equilibria in which the first and second losers will receive the same number of votes. In SMSP districts where there are three candidates instead of two, voters will either desert the third candidate, or the second and third place candidates will receive roughly the same percentage of the vote in equilibrium. Strategic voters will not want to waste their vote if they know that a candidate is in third place; however, if they aren't sure which candidate is in third place, they will vote for their preferred candidate. Using the ratio of the second to the first loser's vote total (the SF ratio) he finds evidence of strategic voting when an election is close in the SMSP, SNTV and PR systems he analyzes.

Cox's theory predicts that voters will strategically desert a trailing candidate; however, there are other factors which may confound strategic voting. As noted above, voters may not be sure who the trailing candidate is, or nomination rules may increase the blackmail power of small parties. Another important factor which affects the level of coordination is whether voters or parties are focused on short-term or long-term benefits. For example, a voter may be willing to vote for her most preferred party if she thinks the party has a chance to win a seat in the next election. A party may also be focused on the long-term, or it may want to show its strength in order to force cooperation with a stronger party, or exercise blackmail potential. Strategic coordination may fail in these situations. Coordination failures are examined in more detail in Part V of the book.
Cox continues his study of strategi-
cop霹inal in Part III of the book by analyzing parties at the district level. This section details how the different voting systems create coordination problems for parties, particularly in determining whether to enter a race or how many candidates to run. Using the Japanese case as an example, Cox provides support for Taagepera and Shugart’s Law of Conservation of Disproportionality. This law basically states that there is a negative relationship between proportionality in a system, and the number of parties. Although increasing district magnitude boosts proportionality, there is an indirect negative effect on proportionality. As the number of parties competing for seats increases, the share of seats for each of the smaller parties decreases. A party may decide to drop out of a race if it thinks that it does not have a chance of winning a seat due to an increase in competition. Parties may also exchange their votes in one district for influence or a better opportunity to win a seat in a different district. Cox finds that in the Japanese case, fewer parties competed in an election in districts where there was a large disproportion between vote and seat shares in the previous election.

Party coordination at the national level and Duverger’s law are the subjects of Part IV. Duverger’s law predicts bipartism at the district level in SMSP systems, but it is not clear how this law translates to bipartism at the national level. Cox argues that district-level legislative and national-level executive electoral rules need to be taken into account when studying national party systems. The types of electoral rules used at the executive level can increase the need for cross-district cooperation by parties. He concludes that Duverger’s law depends on the factors which drive linkage between district-level candidates and national parties. This linkage is stronger when executive power is concentrated, and legislative and executive elections are held concurrently.

Executive electoral rules also play a role in the next chapter where Cox shifts his focus to the interaction between social diversity and district magnitude. In the empirical portion of this section, he finds that the effective number of parties is a function of social heterogeneity (“the effective number of ethnic groups”) and electoral permissiveness (“the magnitude of the median legislator’s district”). Using evidence from 54 electoral systems, he is able to substantiate his claim that the interaction between social cleavages and electoral rules, both at the legislative and executive level, influences the number of parties in a system at the national level.

Coordination not only influences the number of parties in a system, it can also affect the representativeness and stability of a government. Coordination failures are an important aspect of this analysis and in Part V Cox describes how coordination failures affect democratic performance. He begins by addressing the issue of electoral engineering and the effect of strong systems on representation. Failure to coordinate can affect the performance of strong systems if center candidates cannot coordinate and extreme candidates win in constituencies. Weak systems may not be representative if parties are unable to coordinate at the government formation stage.

The failure of the opposition to coordinate at the electoral stage can lead to one-party domination and a corrupt party system. Cox tests the effect of electoral systems on dominant parties, using Japan and Taiwan as examples. He argues that governing parties in an SNTV system are more able to coordinate due to their access to “pork” and money. He concludes that an SNTV

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system is more likely to produce one-party dominance than a pure PR system.

Cox’s work formally defines the
ways in which electoral systems may deviate from the democratic ideal. It improves upon previous studies by developing empirical tests of propositions derived from game-theoretic models. Cox not only addresses weak links in theories related to electoral systems, he also helps to build a bridge between those who focus on institutions and those who argue that cleavage structures determine the number of parties. This work has two other features which distinguish it from previous studies; first that it relies on constituency rather than national level data, and second that it not only describes when coordination succeeds, but also when it fails. This book is an important step forward in the study of electoral systems and is must reading for researchers and students of parties and electoral systems.

Time and Revolution: Marxism and the Design of Soviet Institutions
Stephen E. Hanson
University of North Carolina Press, 1997

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Given the immense body of literature on the development of Soviet political and socioeconomic institutions, Stephen Hanson’s unique analysis of the effect of ideology on Soviet institutional formation is quite an achievement. Focusing on the evolution of time as a theoretical concept, Hanson traces conceptual debates beginning with Kant and Hegel and ending with Gorbachev. Hanson’s argument, that a distinctive Marxist view of time, labeled “charismatic-rational,” had a strong influence on Soviet institutional development, emerges from this analysis. While the explicit argument addresses Lenin’s political institutionalization, Stalin’s socioeconomic institutionalization, and Gorbachev’s unsuccessful attempt at cultural institutionalization, there is also an implicit argument that the collapse of the Soviet Union may be understood in terms of the inability to sustain a charismatic-rational conception of time. Although not the main focus of Hanson’s work, this underlying argument only serves to enhance the overall originality and creativity of his analysis. Hanson’s argument stems directly from Ken Jowitt’s characterization of Leninism as an original fusion of “charismatic” and “impersonal” elements. Based on Weber’s argument that variation in types of political order can be understood as three types of legitimate domination, Hanson’s originality lies in his ability to insightfully apply Weber’s ideal-types of political order to Marx’s understanding of time and to the development of Soviet institutions. Hanson’s contribution is the attention he pays specifically to the charismatic form of authority, which he views as necessary to comprehend Marx’s view of time. Based on conflicting Kantian and Hegelian theories of time, Marx’s conception, “an amalgam of charismatic and rational elements” (p. 19), was the core shared agreement that allowed Soviet leaders to institutionalize time use. Hanson clearly demonstrates that although Marx failed to unambiguously theorize about rational and charismatic time conceptions, he did provide a synthesis which can be seen in his perception of communism as a social movement “within time that liberates humanity from time” (p. 42), and his perception of a cultural ideal as a “vision of human subjects who continually engage in fulfilling, time-transcending activity within the temporal world” (p. 53). According to Hanson, these perceptions, based on Marx’s charismatic-rational time orientation and articulated during the “theoretical cycle” of Soviet institutionalization, formed the backbone of policies initiated by all Soviet leaders from Lenin to Gorbachev. During the “political cycle” of Soviet institutionalization, Lenin adopted a moderate centrist position between the leftist desire for spontaneous revolutionary activity (charisma), and the rightist desire for gradual revolutionary activity (rational-legal). In a successful attempt to reconcile these objectives, Lenin developed and organized the “party of professional revolutionaries,” guided by the principle of party discipline. Hanson cleverly notes that the term “professional revolutionary” itself indicates the need for both discipline (rational time) and revolution (charismatic time). While Hanson sheds much light on the development of the Leninist party as the “collective charismatic agent that would master time within the boundaries of time” (p. 79)—in other words the political institutionalization of Marx’s synthesis—he also illustrates Lenin’s confusion regarding the practical applica-

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tion of the concept of rational-charismatic time to socioeconomic institutions, a confusion left to be resolved by his successors. From this reviewer’s perspective, Hanson’s analysis of the Stalin era is by far the most interesting and most controversial chapter of his book. Hanson argues that Stalin solved Lenin’s dilemma during the “socioeconomic cycle” of development with a logical “response” (p. 148) consisting of the application of Lenin’s understanding of party discipline to society as a whole. Stalin’s socioeconomic policies, including the implementation of Five Year Plans, collectivization, industrialization, and even mass terror, were rooted in two concepts: planned heroism and discipline. The Stalinist economic system, embracing these concepts, sought to achieve Five Year Plans in four years and to overfulfill additional quotas established by the state through the incalculation of work norms designed to stimulate human productivity beyond human capacity. Hanson certainly captures the reader’s attention with his perceptive discussion of Stalin’s campaign for “socialist emulation.” While Hanson’s argument highlights the ruthless perseverance of Five Year Plan quota achievement, and the era’s rapid, brutal collectivization and industrialization policies, the analysis is taken one step further when Hanson turns to Stalin’s policy of mass terror. It is here that the argument becomes somewhat polemical and perhaps therefore in need of stronger evidence than that which is provided. Hanson claims that successful institutionalization of the rational-charismatic time synthesis did not guarantee the creation of a “disciplined revolutionary culture” (p. 162). Accordingly, as Soviet workers increasingly failed to live up to Stalin’s heroic expectations and became more and more unproductive, Stalin launched a campaign, in the form of mass terror, to solve this problem. In Hanson’s own words, “Stalin’s policies from 1936 to 1938, despite their horrifying consequences, can be seen as a logical response to these cultural dilemmas of the charismatic-rational conception of time” (p. 166). Hanson does periodically assert that his explanation in no way diminishes or excuses the horrific consequences of Stalin’s actions. However, his structural argument downplays the role of agency; if this was a “logical” response, then we should assume that any individual in Stalin’s position might have adopted the very same extreme policy. Given that Hanson de-emphasizes leadership as an independent variable, more direct evidence reflecting Stalin’s thinking about the purposes of mass terror is needed if we are to accept Hanson’s debatable explanation of Stalinist terror. Following Stalin’s failure to create a disciplined revolutionary culture, Krushchev’s un-successful hair-brained schemes, Brezhnev’s era of stagnation and corruption, and the brief reigns of Andropov and Chernenko, Gorbachev attempted to revamp the Soviet socialist system. Hanson takes a unique position regarding Gorbachev’s objectives when he argues that perestroika was designed to transform Soviet culture “in a charismatic direction—to produce a culture, and not merely a socioeconomic structure, based on a mass internalization of norms of disciplined time transcendence in everyday life” (p. 182). Gorbachev’s objective, then, was to restore socialism entirely within the existing framework of the charismatic-rational time conception of his predecessors through a “practical and disciplined, yet evolutionary movement” (p. 195) to create a mass Soviet culture based on norms of revolutionary discipline. Ultimately Gorbachev failed to create such a culture through perestroika, as his fundamental aims were based on false assumptions outlined by Hanson. Finally, in a remarkable, though again highly controversial passage, Hanson introduces his implicit argument about the collapse of the Soviet Union by linking Marx’s cultural ideal with the use of mass terror: this ideal “inspired a standard of proper revolutionary behavior that very few people could live up to in practice, and only then for very short periods of time” (p. 205). Stalin failed to overcome this obstacle with mass terror. Gorbachev failed to overcome it with perestroika. The implication is that in the final analysis the Soviet Union dissolved into fifteen independent states in part because Marx’s synthetic conception of communist time, although institutionalized politically through Lenin’s conception of the Bolshevik party, proved too difficult to institutionalize in large-scale socioeconomic institutions. Marx’s ideal conception of communism and its accompanying cultural ideal were indeed just that: ideal. In short, the Soviet system, because it was founded on these fundamental ideas, was doomed to failure. This implicit argument illustrates Hanson’s creative and refreshing thinking about the breakdown of the Soviet Union. Hanson’s analysis connects the role of ideas to institutional outcomes and as such is an important contribution to the field of comparative politics. This reviewer would caution against the pursuit of parsimony at all costs, however. While Hanson’s focus on one independent variable is laudable, it should also be noted that he is analyzing complex phenomena (the development of Soviet political and socioeconomic institutions, and the collapse of the Soviet Union) with only one causal variable. Consequently, some of the claims made in this book are less compelling than they might be if additional variables were included. Despite this criticism, Hanson’s arguments are innovative, interesting, and plausible and therefore deserve our full attention.
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