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Letter from the Editors

Change and Appreciation

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With this first issue of 2004, we conspicuously depart from previous practice. As many long-time readers of the newsletter will have already noticed, the section president's opening letter is missing from the current edition. Breaking from tradition, the president's letter will appear but once annually during the next two years, specifically in the newsletter's 2005 and 2006 summer issues. In its stead we will invite one of our section's prominent members to contribute an essay to the first issue of 2005. Lengthier than the usual contributions to the symposium section of the newsletter, this essay will address a broad theme of general interest to the section's members. Readers are invited to submit their ideas to us about which topic or controversy they would like to see covered in this new space in 2005 and to suggest the names of persons who might be especially qualified to comment on it.

Also in this first issue of the New Year, we wish to thank the outgoing president, Evelyne Huber, for her outstanding and dedicated service to the Organized Section in Comparative Politics and to the newsletter. Following in the footsteps of the section's past presidents - Peter Lange, Ron Rogowski, David Laitin, Bob Bates, David Collier and Michael Wallerstein - Evelyne Huber has generously shared with us during the past two years, through her biannual Letters from the President in the newsletter's opening page, her insights into the current state of the study of comparative politics and the major substantive and methodological questions currently percolating within our subfield. Under her able leadership the section's paid membership surpassed 1600, thus consolidating the position of comparative politics as the largest organized section of the American Political Science Association.

As Evelyne Huber departs as Comparative Politics Section President, we welcome to the pages of future newsletters the pensive missives of her successor, Peter A. Hall, Krupp Foundation Professor of European Studies and current Director of the internationally renowned Minda de Gunzburg Center for European Studies at Harvard University. Peter Hall, of course, is no stranger to most comparativists and he is especially familiar to students of European politics. Author of more than 60 articles on European politics and policy-making in leading journals and collections, his most prominent

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contributions to comparative scholarship include *Governing the Economy: The Politics of State Intervention in Britain and France* (Oxford University Press, 1986), recipient of the Woodrow Wilson Foundation Award (1987); *The Political Power of Economic Ideas: Keynesianism across Nations* (Princeton University Press, 1989), edited for the Social Science Research Council; *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage* (Oxford University Press, 2001), edited with David Soskice; and "Mixed Signals: Central Bank Independence, Coordinated Wage-Bargaining, and European Monetary Union," *International Organization* (Summer 1998), co-authored with Robert J. Franzese, Jr. and winner of the 1999 Gregory Leubbert Award for the best article in comparative politics.

Less well known, but perhaps even more important for the comparative politics subfield, has been Peter's mentorship of dozens of graduate students at Harvard. Liberally distributed among many of the best political science departments across the United States, Canada, and Western Europe, Peter's former students comprise a small army of some of the best scholars the comparative politics subfield has to offer today. Like the many students and colleagues to whom he has been so generous over the years, we too are grateful to Peter for his past efforts on behalf of the Comparative Politics Section and for those he has committed himself to making as section president during the next two years.

Editor's Note

The editors welcome suggestions of important news or themes that should be covered or reviewed in APSA-CP.

The Confluence of American and Comparative Politics?

Introduction

Some years ago - people will disagree about exactly when - comparative politics and American politics were entirely distinct subfields of political science. Now, however, it is increasingly evident that the boundaries between American politics and comparative politics are eroding. Comparativists are more frequently including the United States as a case in cross-national research, and some Americanists are beginning to venture out into other countries, either theoretically or in their field research. At the same time, many research questions, frameworks, and methods that developed first in the study of American politics are filtering into research on other countries, and efforts to understand the U.S. in comparative perspective are becoming more common. Occasionally a concept developed for understanding another country has gained currency in the U.S. ("social capital," for example). Some applaud these trends and look forward to the day when the boundaries between the two subfields have vanished. Others are apprehensive.

We invited six prominent scholars to address one or more of the following questions that are inspired by this confluence of American and comparative politics:

- In what specific ways and to what degree has one subfield influenced the research agenda of the other in recent years? Has greater (intellectual, methodological, etc.) cross-fertilization produced stronger theory? Is there currently less cross-fertilization than there could or should be, or too much?
- Has the comparative subfield become intellectually or methodologically subordinate to the American subfield in recent years?
- Is the United States now but a single case to be studied by comparativists using comparative tools and methods? Is American politics the last case study?

Each symposium participant stakes out a different position. Robert Dahl celebrates the existing specialization and division of labor in which some scholars study the United States exclusively, others specialize in other countries, and still others situate countries (including the U.S.) in comparative perspective. If the comparative subfield were to decide that comparative research "must always or usually be 'comparative,'" he writes, "the result would be a net loss in our understanding." Paul Pierson laments the segmentation and over-specialization of the American subfield. Comparative politics should not emulate Americanist goals and organization that lead to arcane, blindered research, he argues; rather, the Americanists should emulate question-driven, more wholistic approaches in comparative politics. Gary Cox and Mathew McCubbins outline the major research agendas and subagendas in the area of legislative organization. In their view, American and comparative politics have converged so thoroughly in this area that examples of research on the U.S. and research on other countries are easily placed side-by-side under every outline heading. Because these agendas are all driven by rational-choice theories originating in the U.S., this area is the closest approximation of a kind of U.S. hegemony, albeit one that eagerly accommodates variations in other countries. In her contribution, Melissa Nobles portrays the confluence of American and comparative politics in the study of racial politics over the past three decades as a mutually fruitful commingling of ideas. Despite some grumbling about the inapplicability of the United States's black/white dichotomy to societies with more blurred racial boundaries, this field seems to have embraced happily both the need for comparison and the relevance of the U.S. as a case. Finally, Daniel Levine drawing on his experiences studying consciousness, organization, and conflict, privileges comparative over exclusively American perspectives. He argues that theories and methods originating in the U.S. - rational choice and quantitative analysis - must be used in moderation and with caution, for they are inherently too narrow and naïve to do justice to the rich histories, contexts and cultures of other countries. Like Pierson, he believes American politics would profit by importing non-U.S. ideas and approaches more often.

We have, then, four models of convergence: Dahl's cordial division of labor, the Gramscian-American near-hegemony that Cox and McCubbins describe in studies of legislative organization, Pierson's and Levine's wariness of exclusively U.S.-inspired approaches, and Nobles's portrait of harmoniously merging subfields in the study of racial politics. Collectively, these statements represent the diversity and range of positions in our subfield on these issues. Individually, their positions are representative of the approach that is dominant in

research in their areas of expertise. The contrast between the pluralism at the level of the subfield and the relative unity prevailing in more specific areas demonstrates that American and comparative politics can flow together in quite different ways. One cannot help but wonder whether these research areas might have managed, and might yet manage, the confluence of subfields differently than they have so far.

Comments on the Confluence of American and Comparative Politics

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A half century and more ago, "political science" was a highly parochial discipline centered on the United States and practiced predominantly by Americans and a small (but important) number of scholars in Britain and Europe. Outside the United States, scholars writing on the empirical and philosophical aspects of politics were typically attached to academic fields like philosophy and history, and separate departments of political science were almost nonexistent. With a few exceptions, the subfield of "Comparative Government" tended to be limited to studies of the United States, Britain, France, and Germany, with an occasional investigation of Fascist Italy and Soviet Communism. Although a few intrepid scholars and graduate students ventured further afield, in their teaching, research, and writing most American political scientists focused their attention on this limited group of countries - and mainly on the U.S.

This predominantly parochial view of politics was countered in part - perhaps mainly - by studies in the subfield of political philosophy (or political theory, as it was often called in political science departments), which brought students and scholars in contact with the rich variety of perspectives on political life from Socrates onward. "Political theory" in this sense was, I believe, a required subfield in some departments (as it was, if my memory serves me correctly, at Yale)

The change during the past half century or so has been enormous. From a parochial field occupied predominantly by American political scientists focused largely along the lines I've just described it has become a worldwide enterprise, with scholars spread throughout the globe. Studies of political systems around the world no longer depend on Western scholars: now, any country with an advanced educational system is likely to have some political scientists who study and write about it.

I might illustrate this change by drawing on a personal experience. My own work was centered on political theory and American politics until the late 1950s when, eager to learn about a broader range of countries, I began the collaboration that became *Political Oppositions in Western Europe* (1966). I found this collaboration enormously enriching, and it encouraged me to extend the scope of the project to the smaller European democracies, to developing democracies, and even to nondemocratic regimes. One product of this effort was *Regimes and Oppositions* (1973), which included chapters on oppositions in the Soviet Union by Frederick Barghoorn, Communist East Europe by Gordon Skilling, tropical Africa by William Foltz, Spain by Juan Linz, Latin America by Robert Dix, India by Rajni Kothari, and Japan by Michael Leiserson. Val Lorwin,

> "... the losses from largely replacing a 'country-centered' perspective by a comparative perspective would be much greater than the gains."

Hans Daalder, Stein Rokkan, and I planned to bring out a series of books on the smaller European countries that would survey comprehensively these under-studied political systems. Although only one volume of this series was published (Chubb 1970, on Ireland), this project stimulated research in, and about, these countries.

In addition to this global expansion in political science and political scientists, a huge increase also took place in the availability of cross-national data, which often helps to make the analysis of a number of countries both more rigorous and more truly comparative.

As a result, the field of comparative government is far richer than it once was. Far from having become "subordinate to the American subfield," comparative studies are, in my view, often independent of it - not only because many studies are now conducted by scholars outside the United States but also because a fair number of American political scientists are them-

selves deeply immersed in the language, culture, history, and institutions of a country or region on which they concentrate their research and writing.

Some cautionary words may now be in order.

Among the various domains of research and analysis that are viewed as single domains of study ("fields"), politics may well be the most complex. By comparison, the domain of physics, for example, is relatively simple, not - to be sure - in its methods, theories, and language, but in the physical world that it seeks to analyze. Since it would be inappropriate for me to pursue here my reasons for these conclusions, let me simply assert what surely most of us already believe: politics is an extraordinarily complex phenomenon - or, rather, set of phenomena. To see why, we need only consider the relevant types of units (individuals, groups, associations, territorial entities, states, international systems, etc.) and the variety of complex relations (of authority, power, influence, control, domination, etc) within types (among individuals, for example, or among states) and among different types (individuals, associations, and states, for example). A little calculation would produce thousands, many thousands, of possible types of relations. In addition, our possibilities for employing the rigorous methods of verification of, say, physics or chemistry, are severely circumscribed by limits on our capacities for direct observation and experimentation. After all, we can't observe a hundred countries under a microscope, or in a cage, as they run through transitions to, or breakdowns from, democracy.

Having said all this, what is impressive to me is how much reasonably reliable knowledge we now possess about politics, political life, political systems, etc., that we lacked a half century or more ago - lacked, indeed, for the preceding two millennia and more of serious political analysis in one form or another.

Today, as a result, complexity is a fundamental characteristic not only of the human activity of politics. It is also characteristic of political science as an activity.

Now as we all know, one obvious consequence of this complexity is that, like other "fields," the study of politics requires specialization. We also know that one price for specialization is ignorance - ignorance of many of the contributions made in other specialized subfields. Another price we pay is the difficulty of achieving a synthesis: it becomes difficult for a scholar to incorporate the rich body of knowledge available in other subfields without producing work that may be somewhat shallow and superficial. Yet without synthesis, highly specialized knowledge may become simply irrelevant - irrelevant to human action, choice, decision, and in a sense, even to human understanding. But unless we believe that a better understanding of this complex human activity may contribute, if only indirectly, to human well - being, why should we study politics? Without that end, it seems to me, political science becomes no more than a game played for the benefit of the players.

As I reflect about our field from this perspective, I drift toward some general conclusions. Although I fear I can't fully support them, either here or perhaps at greater length elsewhere, here they are:

- If politics as a general field of human activity is extraordinarily complex, so too is any particular political system, such as the political system of a country. Among the political systems of different countries, I'm inclined to think that the American political system might just be the most complex.

- However that may be, to understand it requires specialization - not simply on "American government and politics" but on particular elements, aspects, institutions, behavior, and the like. I believe then that we require and will continue to require scholars who focus their inquiry on the basic features - the presidency, Congress, parties, courts, opinion, behavior, local governments, and so on.

- If we were to adopt the assumption that studies like these must always or usually be "comparative," the result would be a net loss in our understanding. I don't mean to say that there would be no gains, but only (in my view) that the losses from largely replacing a "country-centered" perspective by a comparative perspective would be much greater than the gains.

- Fortunately, however, comparative studies need not displace work that concentrates on aspects of the American political system (or, for that matter, aspects of other political systems). Comparative studies can, however, sometimes profitably supplement work that retains an exclusively American focus.

- In addition, the field of political science profits greatly from comparative studies. (Given the collaborate works I cited earlier, I could hardly conclude otherwise). Yet I certainly don't believe that all individual scholars should engage in comparative work. The field is richer now than it was a

half century ago because some individual scholars are engaged completely or primarily in comparative studies, while others are engaged completely or primarily in work on a particular country, a particular political system, or a particular institution in a particular country.

I hope, then, that individual scholars will continue to enrich the field of political science by their different approaches: some by deepening their knowledge, and ours, of particular political systems, including that of the United States, while others deepen their knowledge, and ours, by situating particular political systems, like that of the United States, in a comparative perspective.

Why Americanists Should be Buyers in the Markeplace of Ideas

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It is now common in the discipline to hear the confident assertion that the American politics subfield is the most sophisticated, advanced, and "scientific." There is much conversation about the prospects for a more unified and "modern" discipline. For many, the assumption is that this unification should and will be built on the recent achievements of the Americanist subfield. In short, in the marketplace of ideas Americanists are presumed to be the sellers.

This may yet come to pass, although I doubt it. If it does, however, it will be bad news for the systematic study of politics. The Americanist subfield has produced some very fine scholarship. Its dominant orientations reveal some admirable features. I could easily write about the considerable insights that comparativists could glean from Americanists. Given space constraints, however, I want to consider the less successful aspects of the subfield. These are equally deserving of notice, for it is a subfield that contains some stunning biases and limitations. The subfield's shortcomings are instructive, both for comparativists thinking about new research agendas and for Americanists thinking about the future. Americanists have much to sell, but it would be far healthier if they were also very active buyers in the intellectual marketplace.

A few observations about the subfield's organization may help to make its shortcomings more explicable. First and most obviously, Americanists focus their intellectual energies on a single political system. This feature leads to a second: the subfield can study that polity with an unmatched density of scholarly resources. Crucially, these two features have combined to produce a third: inquiries into American politics are typically organized around a "pizza-pie" approach. Highly institutionalized groupings focus on particular slices (Congress, the Presidency, Interest Groups, Parties, etc.) of the political system, with most of the major intellectual conversations occurring within rather than across groupings.

Now some would say that this highly specialized division of labor is to be applauded - part of a natural progression that we should associate with a mature field of science and something to be applauded and emulated. Yet while specialization is obviously necessary, comparativists prefer to specialize by organizing research around particular problems or substantive themes (like democratization or political violence). Such an organization of research permits sustained attention to the interconnected pieces of modern polities.

To an outsider, the "pizza-pie" organization of the Americanist subfield almost seems designed to root out interesting lines of inquiry. The effect of specialization and compartmentalization has been an intense concentration of intellectual resources on narrowly framed questions: When are Presidential vetoes successful? What are the sources of incumbency advantage? How do negative campaign ads influence elections? Because scholars operate in large but specialized communities focusing on a particular site of political activity, their questions tend to address what seems most prominent or tractable in that particular area, rather than more system-wide issues in the polity. Most notably, scholarship on Congress (which has been at the heart of the Americanist subfield) has been heavily oriented toward the investigation of the internal workings of that institution. Congress scholars have paid much less attention to considering systematically how changes in the broader political and social context influence what Congress does.

The absence of a comparative framework, which can help to generate more compelling puzzles by highlighting distinctive features of the American polity, reinforces this narrow focus. The marked advantages of situating the American polity in a comparative context can be seen in Jacob Hacker's recent book *The Divided Welfare State* (Hacker 2002). Unlike most contemporary studies of American politics, Hacker's approach is clearly comparative. He uses

detailed cross-national comparisons to reveal that the most distinctive feature of American social policy is not the *level* of spending but the heavy reliance on policy instruments that subsidize or regulate private provision rather than promoting public provision. Hacker uses comparative analysis to establish the central puzzle, providing an opening for a revealing exploration of central elements of American politics. In demonstrating how multiple features of the polity combine to promote this structure of policy development, Hacker shows the ways in which American political institutions channel government action rather than simply blocking it. By situating his analysis comparatively he brings a new level of sophistication to our understanding of how crucial actors, such as employers and conservative politicians, have shaped and reshaped American governance.

The compartmentalization that characterizes the American subfield has also led to a kind of methodological one-upmanship. Technical proficiency becomes the metric for evaluating quality. Statistical analysis of large data sets and the development of formal models of strategic interaction of small groups of actors are dominant. Despite the wealth of scholarly resources, research has become increasingly concentrated on that restricted subset of questions that lend themselves to the most "sophisticated" research techniques. There is no questioning the technical proficiency of much work in American politics. Yet far too much of that research reminds one of nothing more than muscled-up body-builders, whose arms are so bulky that they are almost useless for everyday tasks.

Comparativists are puzzled by the huge firepower that those who study contemporary American politics have directed on what often seem like trivial or at least relatively uninteresting puzzles. The trend culminated in the 1990s. This was a time of profound and often puzzling transformations of the American polity. Yet the most prominent debate, a magnet for the attention and skills of many of the most highly regarded students of American politics, centered on disputes about the functioning of Congressional committees. Did committees mostly facilitate "gains from trade" or expedite the flow of information among legislators? Debates generating this level of activity and attention should meet the test of providing something of real use beyond those most intensely engaged in the scholarly enterprise. To be blunt, I do not see how this (admittedly very sophisticated) debate on Congressional committees passes the "who cares" test.

Of course the flip-side of the narrowly focused energies of the subfield has been a range of striking silences. Too many subjects - including tremendously important ones - have simply dropped off the radar screen because they are not easily addressed (or even recognized) by those employing favored techniques. In canvassing the work of Americanists, I am often reminded of the famous *New Yorker* cartoon-map of the United States, as viewed from the perspective of a Manhattanite. Local landmarks like the Empire State Building and Central Park loom large, but most of the rest of the country disappears from view or appears in only distorted, fun-house mirror form. At least in the major mainstream journals, a map of the American polity as studied by contemporary Americanists would reveal a few skyscrapers, such as the study of public opinion, voting behavior, and Congress. Major parts of the landscape, however, receive little or no attention. To offer just one example, although courts play a more prominent role in the American polity than they do in any other contemporary democracy, and their significance is clearly on the rise, "law and courts" remains a backwater in the subfield. Urban politics, federalism, social movements, and many other areas of important political activity are similarly neglected.

"Comparativists are puzzled by the huge firepower that those who study contemporary American politics have directed on what often seem like trivial or at least relatively uninteresting puzzles."

Yet the biggest problem is not that some slices get more attention than others; it is that intensive specialization and the lack of comparative perspective blinds Americanists to many of the most interesting problems in politics, as well as to distinctive kinds of explanation. Again, comparativists are much more likely to organize their inquiries around distinctive substantive issues rather than particular sites of political activity. For example, one of the liveliest areas of inquiry in comparative politics over the past two decades has been the study of political economy. A large group of well-respected scholars has debated how the evolving structures of national economies and the coalitions of interests surrounding those economies influence, and are influenced by, political systems. By contrast, there is really nothing like a field of political economy in the American subfield. There are scattered studies that could be place under such a rubric. Yet

despite massive and growing economic and political inequalities, the interplay between the highly distinctive American economy and its peculiar political system has not generated a sustained or systematic research program.

The facts that comparativists are spread more thinly and are trained to contrast a particular polity with others almost dictate that analysts think in more "configurational" or relational terms. This turns out to be a tremendous advantage, since it compels systematic attention to both interaction effects among discrete sites of political activity, and to how changes in one site can modify the functioning of others. In the field of comparative political economy, for instance, this has spurred an appreciation of institutional complementarities - the ways in which multiple, interacting institutions and policies can come to constitute distinctive regimes that facilitate particular kinds of political action and generate divergent social outcomes. In comparative work on social movements, thinking about configurations of institutions and organizations has facilitated the identification of distinctive political opportunity structures that enable or discourage particular kinds of social movement strategies. In comparative politics, there has also been a strong tendency to think about politics dynamically - to recognize that the social world is marked by processes that unfold over time.

Again, this contrasts sharply with an Americanist subfield in which "history" too is treated as a separate specialization, sliced off into a distinct category of "American political development" that is seen as remote from issues of contemporary politics. As I have argued elsewhere, static analyses, of "snapshots" of moments of time, can distort what we see and how we understand it in profound ways (Pierson forthcoming). They may, for example, lead us to ignore long-term processes that can quietly but profoundly transform causal relationships of interest. For instance, the massive expansion of federal policy in the period after 1960 has unquestionably altered the functioning of, and relationships among, key political institutions and organizations in the United States. Yet students of American political institutions have generally paid little attention to this expansion, which comparativists would easily recognize as a major episode of state-building.

The focus on method-driven research, the lack of recognition of the distinctiveness of the United States, the limited appreciation of the ways in which causal relationships in specific areas of a polity may be influenced by broader features of a particular social configuration, and inattentiveness to temporal context all produce a final shortcoming. Even as they operate on a far smaller empirical canvas, Americanists seem far more prone than comparativists to act as if they are producing generalizations about politics that will apply at widely different times and in widely different social contexts. There are very strong grounds for treating this confidence with skepticism.

Some might fairly say I have offered my own *New Yorker*-style distortions in this account. This is obviously a highly-stylized treatment of a rich and complicated subfield. Clearly, there is a lot of work that would not fit my depiction. And there are signs of a backlash against the restricted vision of much of the subfield. For instance, recent scholarship on income inequality and political influence (Bartels 2002), growing partisan polarization (McCarty, Poole and Rosenthal 1997), and the political impact of long-term change in the structure of the media (Prior 2002) combine technical sophistication with attentiveness to fundamental questions about the performance of American democracy. Yet the subfield's record remains decidedly mixed. Americanists have developed bodies of theory and methodological techniques that should be of considerable interest to comparativists - only, however, as additions to the broad and diverse portfolio needed to consider the wide range of issues of interest to comparativists. Despite the self-congratulatory tone that marks much mainstream work on American politics the debilitating consequences of organizing the subfield in a way that slices the polity are evident. Too often within the subfield, the universe of politics deemed as suitable for scrutiny has been redefined in ever more diminutive terms. The study of American politics becomes the study of Congress and voters. Big questions, larger contexts, and long-term transformations have receded ever farther from view - and political science risks cutting itself off from the concerns that justifiably engage the interest of broader audiences.

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Theories of Legislative Organization

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That the study of democratic legislatures is dominated by the study of the U.S. Congress has been the theme of two successive review articles in the APSA's *State of the Disci*pline series (Mezey 1993; Gamm and Huber 2002). In this limited sense, there has been a confluence of American and comparative scholarship in legislative studies for quite a while. In this essay, we survey recent theories of legislative organization, with an eye to selected features that resonate beyond the American Congress. We will not cover models that deal with intercameral (e.g., Tsebelis and Money 1997, Druckman and Thies 2002) or interbranch (e.g., Magar 2001, Cox and Morgenstern 2001, Diermeier and Feddersen 2000, Cameron 2000, Carey and Shugart 1998) relations, focusing instead on the three most-studied intra-cameral structures-parties, floors and committees.

We divide the literature into theories that de-emphasize parties (in favor either of committees or floors) and theories that emphasize parties as the main organizing feature of the legislature. Within the latter category, we focus on the distinction between parties as floor voting coalitions and parties as procedural (agenda-setting) coalitions.

I. Non-Partisan Theories

A. Legislatures are like town hall meetings

In some models, the legislature is no more than a group of equal legislators, none with resources or agenda-setting powers greater than others, who meet in plenary session to decide various matters. This idealized legislature is organized as if it were a town hall meeting, with no committees, no parties and a floor that is characterized only by the basic decision rule - whether a majority or a super-majority rules. Typically, it is assumed that policy choices and legislators' preferences can be arranged spatially on a left-right policy continuum. The implications of these theories-which include those by Downs (1957), Black (1958) and, more recently, Smith (1989) and Krehbiel (1998) - are well known. Everything hinges on the voting rule, with pure majority rule leading to the famous median-voter theorem and super-majority rules leading to a systematic role for "pivotal" legislators (e.g., those positioned spatially to be the crucial last-recruited members of a coalition to override a presidential veto).

These models do not imply that real legislatures in fact lack committees and parties, although they are sometimes used as a baseline against which committee and/or party treatments are tested (Krehbiel 1991, Cox and McCubbins 2002). Rather, their main purpose is to clarify the central effect of varying vote requirements on the floor. To this end, committees and parties are ignored.

B. Legislatures are like firms

Another group of models moves beyond considering floor voting rules, to bring finer details of legislative structure into analytical view. In these theories, the organization of the legislature is similar to the organization of the firm and the analogies used parallel those in the literature on industrial economics - that is, legislative structure differs from the disorganized "market" of the town hall meeting. The structure that these models typically try to explain is the existence and function of committees. The answers offered can be divided into those that hinge on internal processes and those that hinge on external processes.

Internal explanations of committees

There are currently two main internal explanations of creating a division of labor via committees. In one theory, dubbed "distributive," committees help to ensure that gains from legislative trade are accrued (Weingast and Marshall 1988). In another theory, dubbed "informational," committees help to ensure that gains from specialization of labor are accrued (Gilligan and Krehbiel 1990, Krehbiel 1991). For present purposes, we will only make three points about these well-known models. First, intra-legislative payoffs drive each of them. Second, committees' agenda-setting powers play a central role in both. Third, both suggest that committees produce outcomes that differ from the preferences of the median (or pivotal) floor legislator.

External explanations

Diermeier and Myerson (1999) have recently offered an external explanation of why committees exist. In their view, creating additional committees (so long as they remain faithful to their chamber principals) improves a given chamber's ability to bargain with other bodies in separation-of-powers systems (for much the same reason that the U.S. president's bargaining position with foreign powers may be enhanced by the necessity of treaty ratification by the Senate). In the same vein, Epstein and O'Halloran (1999) argue that committees are used to offset executive agencies in order to yield policy outcomes that favor the legislature as a whole. The use of legislative committees as a watchdog of executive agencies has been suggested for the Policy Affairs Research Council within the Japanese LDP (Rosenbluth 1989, Raymseyer and Rosenbluth 1993, McCubbins and Noll 1995) and elsewhere (Aberbach, Putnam and Rockman 1981)

C. Legislatures are like markets

Many explanations of legislative politics focus not on explaining the structure of legislative organization, but rather on the outcomes of the law-making process. A common view is that politics involves interest-group bargaining and interest-group logrolling (Schnattschneider 1960, Lowi 1969). In this view, committees are monopoly suppliers of policy change and they exchange policy favors or "rents" in return for cam

paign contributions. This view had its roots in theories of pluralism (Bentley 1949; Truman 1951, Dahl 1967). In essence, Congress is a marketplace and committees are like firms, auctioning off their wares to the highest-bidding interest groups (Buchanan and Tullock 1962, Landis and Posner 1976, Peltzman 1976, Posner 1974, Stigler 1971). Some have argued that committees, using their institutionally defined veto powers (both ex ante and ex post) exist to establish these monopoly bargaining rights and to secure vote trades (Shepsle and Weingast 1987).

A similar model has been offered by Laver and Shepsle (1996) regarding coalition governments in parliamentary democracies. The relatively small parties in multi-party governing coalitions play the role of the special interests; while the cabinet portfolios play the role of committees. The party holding any given portfolio cannot credibly commit to internalize the costs of its decisions to its partners. Thus, governments are constructed in light of this weakness. Policy is stable but potential gains from legislative trade within the coalition remain unrealized. Laver and Shepsle do not formally conclude that levels of government spending would be too high but their model is compatible with such a conclusion. Bawn and Rosenbluth (n.d.) push a similar line of analysis further in this direction, explicitly concluding that multiparty governments lead to greater government spending.

A third version of the legislature-as-market analogy, albeit one with much different policy implications, appears in some theories of democratic corporatism. In these models, "peak associations" with (a) long-standing ties with ministers and/or governing parties; and (b) encompassing interests (cf. Olson 1965), are firmly embedded in the policy-making process (Schmitter and Lehmbruch 1979, Almond 1983, Katzenstein 1984, Rosenbluth 1989). Because the groups that capture the policy process in these models have broader interests, the conclusion is that more externalities (of various sorts) are internalized-and the decentralization of policy-making gets generally higher marks (achieving specialization and gains from trade, without running as many risks of excessive spending).

II. Partisan Theories

In contrast to the theories of legislative organization just discussed, another strand of legislative research views political parties as the main organizational elements of legislatures. Many scholars envision parties as similar to firms (per the literature on industrial organization), in that they involve delegation to central agents (party leaders) in order to reduce transaction costs and ameliorate collective action problems. In this section, we first consider why parties might arise-again dividing the literature into internal and external expla-

nations - and then consider some of the issues involved in organizing parties and how parties 'seize' the authority resident in the legislature.

A. Why parties? Parties are created to solve internal collective action problems

One line of theorizing about why parties exist is similar to the distributive line of argument regarding committees. Absent any organization (other than a voting rule for floor decisions), legislators face a chaotic and unpredictable agenda. They cannot be sure that the legislature will not vote tomorrow to strip them of benefits conferred today. Nor is it clear how to ensure that the benefits are conferred to begin with, given a world where any legislator can move any amendment at any time.

In order to deal with the unpredictability - and unprofitability - of an unorganized legislature, legislators form political parties to bind themselves together in durable coalitions. Gains from legislative trade are thus accrued that could not be accrued without parties. Probably the clearest exponents of such a view of legislative parties are Schwartz (1977) and Aldrich (1995), but many others similarly stress the purely legislative payoffs to forming a party.

Parties are created to solve external collective action problems

An alternative branch of theory views legislative parties as formed primarily to accrue electoral gains. Modern political parties facing mass electorates have a strong incentive to fashion and maintain a brand name, just as do modern corporations facing mass markets. Such brand names are, however, public goods to all politicians running under the party's label. Thus, parties arise in order to ensure that the usual problems of providing and maintaining public goods are overcome - and in particular to internalize electoral externalities that would otherwise arise. Probably the clearest exponents of this view of legislative parties are Cox and McCubbins (1993) but others (e.g., Kiewiet and McCubbins 1991, Cox 1987, Evans and Oleszek 2000, Strøm 1990) similarly stress how legislative actions can foster valuable collective reputations (brand names) and how politicians take legislative action with an eye to such payoffs.

B. What do parties do?

Different partisan theories place varying emphasis on parties as floor voting coalitions (organized to influence their members' votes on key substantive floor votes) and parties as procedural coalitions (organized to influence their members' behavior in committee and other pre-floor stages, as well as their votes on key procedural floor votes).

Parties as floor voting coalitions
When focusing on the parties as floor voting coalitions, many argue that individual party members, recognizing their own incentives to "free ride" or the difficulties of coordinating action, empower a boss (party leader) to discipline and coordinate them, so that they all can achieve the benefits of fuller cooperation (e.g., Cooper and Brady 1981, Sinclair 1983, Stewart 1989, Rohde 1991, Maltzman and Smith 1994, Binder 1997, Cox and McCubbins 1993). The visions here are similar to the theory of the firm in the industrial organization literature (Alchian and Demsetz 1972), and floor leaders are explicitly agents of their parties (cf. Sinclair 1983, Cox and McCubbins 1993).

A central issue that arises when viewing leaders as agents is agency loss (Kiewiet and McCubbins 1991). The more leaders are independent actors who do not internalize costs across their parties' constituent parts, the less it makes sense to view them as agents of their parties. Hence, in the U.S. literature, one finds research on why leaders lose their posts (e.g., Jones 1968), on whether they are "middlemen" in their parties (e.g., Truman 1951), and so forth. Similarly, in the comparative literature, one finds concern for the rules of selection and accountability of leaders (Coppedge 1994, Strøm 1990).

The flip side of leadership fidelity is membership loyalty. But do members of a given party vote together on the floor solely because they are like-minded or also because they are subject to inducements and sanctions from their leaders? In the comparative literature, Ozbudun (1970) long ago noted the distinction between cohesion (voting together for whatever reason) and discipline (voting together due to leaders' influence). In the U.S. literature, Kingdon (1977) and more recently Krehbiel (2000) have elaborated a similar point. Several recent techniques, all based on the stochastic spatial model, have pushed the methodological frontier on this issue forward. Several (Groseclose and Snyder 2000, McCarty, Poole and Rosenthal 2001, Cox and Poole 2001) require only roll-call voting data to implement and are thus potentially applicable far beyond their U.S. origins.
Another issue addressed in the literature that views party leaders as agents is when leaders receive greater (lesser) delegations of power from their rank and file. The main answer offered in the literature (Rohde 1991, Aldrich 1995, and Aldrich and Rohde 2001), is that

majority-party backbenchers delegate more power to their party leaders when preferences vary less within each party and more between parties. Party government is thus conditional on there existing sufficient disagreement in preferences between parties (relative to their internal disagreements). Thus far, most of the empirical investigation of the conditional party government thesis has not focused on demonstrating that increased party differences (relative to internal differences) produce increased delegations (although see Rohde 1991 and, for contrary evidence, Schickler and Rich 1997); but rather has looked to more distal consequences (cf. e.g. Aldrich and Rohde 2001).

Parties as procedural coalitions

When focusing on parties as procedural coalitions, the issue of accountability arises vis-à-vis committee contingents and committee chairs. Parties can promote specialization and facilitate legislative compromises within their ranks by taking advantage of the division of labor created by a committee system. The governing party or parties in particular can do so. In order to benefit from committees, however, the ruling coalition must be able either to control committee outputs or to screen committee outputs (or a mixture of both). Control (or influence) over committee outputs can be exerted via the committee appointment process, or through the use of checks and balances exercised by "auditing" of "control" committees (cf., Cox and McCubbins 1993). Thus, a branch of both the U.S. and the comparative literatures deals with party control over committee appointments (e.g., Shepsle 1978, Smith and Deering 1984, Cox and McCubbins 1993, Damgaard 1995, Santos and Rennó 2003).

Committee outputs can be screened if the central party leadership has effective control over which bills reported from committee will actually receive a hearing and vote on the floor. Thus, another branch of both the U.S. and comparative literatures deals with the extent to which governing parties can control the floor agenda (e.g., Cox and McCubbins 2002, Sinclair 2002, Döring 1995, Cox, Masuyama and McCubbins 2001, Amorim Neto, Cox and McCubbins 2003).

How could one tell whether the set of bills in a given legislature is controlled to a significant extent by the government of the day, or whether any member who wishes can get a hearing and vote on any bill? One way to determine this would be to read the rules governing the legislative process in the given legislature. An alternative to reading the letter of the legislative law is to look at actual legislative outcomes - which bills in fact are voted and what do the various parties think of them?

In our recent work (Cox and McCubbins 2002, 2003), we have proposed two simple measures that (imperfectly) reflect the government's control of the agenda and are calculable in a wide range of democratic legislatures. The first measure, called the party roll rate, is the number or proportion of times the government is unable to prevent the passage of a bill its members oppose. If the government controls the agenda, then it should be able to "veto" bills it dislikes and should thus never (or rarely) suffer rolls (passage of bills that a majority of its members oppose). This measure contrasts with the more frequently encountered "government win rate" (e.g., Saiegh 2003, Gamm and Smith 2002) in that we do not count the failure to pass a bill the government does want as a roll. Such defeats do not suggest that the government cannot control the agenda, as what is being voted on on the floor is something that the government likes. Its failure reflects the inability to muster sufficient votes on the floor and points to insufficient discipline, not insufficient agenda control.

A second indication of who controls the agenda is the direction of policy movement (left or right) proposed by each bill that reaches a final passage vote. If the government controls the agenda, then final-passage bills should propose policy movements that please the government. In spatial terms, this means leftward policy movements for leftist governments and rightward policy movements for rightist governments. The direction of policy movement can be gauged from roll-call voting data and hypotheses about the proportion of bills moving policy leftward subjected to various tests.

III. Conclusion

Most theorists, American and comparative, see legislative organization as a means to solve collective action problems and reduce transaction costs, be it through committees or parties. Increasingly, too, theorists see agenda power as a key issue in understanding the structure of legislative power, in addition to the traditional concern for raw voting power. The debate is thus not just about what factors determine voting cohesion but also about who seizes agenda control and how they keep and exercise it.

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The Other Side of American Exceptionalism: The Comparative Study of Racial Politics

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The exceptionalism that has characterized the study of American politics - making the United States, arguably the last case study - has effectively not existed in one literature of comparative politics for approximately thirty years: racial politics. Far from being incomparable, the U.S. has been compared with other slaveholding societies and colonial regimes, most frequently Brazil and South Africa, and with other liberal democracies, most notably Britain (Katznelson 1973, Greenberg 1980, Marx 1998, Nobles 2000). Indeed, these studies most often take slavery, the underside of American democracy (exceptionalism's hallmark), as their starting point. As slavery, *de jure* and *de facto* segregation, and twentieth century social movements have received greater attention within the American politics subfield, so too have their comparative dimensions, to the theoretical and empirical enrichment of both subfields.

Renewed interest in American slavery, beginning in the 1970s, and in the similarities between South African Apartheid and Southern *de jure* ("Jim Crow") segregation account largely for the invigoration of comparative work (for example: Degler 1971, Fredrickson 1981, Cell 1982, Fogel 1989). For the most part, historians and economists wrote the earliest and most influential works. Greater appreciation of the transatlantic slave trade provided a much wider context within which American slavery could be placed and usefully compared and contrasted. That research underscored both the uniqueness and scope of the slave trade and the plantation economies that it created and sustained. With an estimated 12 million people captured between 1500 and 1870, it was one of the largest systems of slavery in human history. Just as importantly, the distinctiveness of New World slavery rested in the intensity and brutality of plantation life and permanence of slave status. Such status was inherited and usually life-long, corresponding closely (although not always) with racial identification. New World slavery was fundamentally, of course, a system of labor. However, insofar as states, colonial administrators, and slave owners developed racial identifications designed to protect, reinforce, and justify slave status, it was also a system of racial slavery, a fairly rare form of bondage. To be sure, there were important differences among and within slave-holding societies. For example, the Caribbean colonies produced sugar, received the most slaves, and were administered largely by absentee plantation owners and their colonial agents. The United States and Brazil were, in contrast, fully settler states, with more diversified slave-produced commodities, and significant regional variation in the use of slave labor. The point here, then, is that the wider context of New World slavery coupled with the evident commonalities between Jim Crow segregation and South African apartheid breathed new life into comparative study.

With this broader historical and geographical canvas, the comparative study of racial politics has been driven by two fundamental and interrelated questions. First, what effects did slavery have on political institutional and economic development in the U.S., Brazil, South Africa, Cuba, and Jamaica? Second, upon emancipation, what explains the emergence of and differences within systems of domination, along ascribed racial lines or in Donald Horowitz's term, "ranked systems?" What accounts for the endurance and alterations within, if not the complete demise of, these racial orders?

In answering the first question, scholars have sought to ascertain both the ways in which governance was organized to sustain and protect slavery and to measure the economic impact of slavery over time. In every case, it is generally agreed that slavery was an indispensable, if not the

> "... the cross-fertilization of ideas has strengthened theory-building across both subfields ..."

sole, contributor to private wealth accumulation and the overall economic development of the colonies and their ruling countries, as well as to newly independent states (i.e. the U.S. and Brazil). As obvious as this may seem, it is only recently, for instance, that the centrality of slavery to America's national economy, as opposed to the south's regional economy, has been openly and widely acknowledged (Fogel 1989). As for the political effects of slavery, in all cases, scholars agree that slave-holding interests were strongly protected through legislation and other political

mechanisms, the efforts of abolitionists notwithstanding. Of course, the political power of slaveholders varied across the cases, as did the implications for the quality and character of political life. In the sugar-producing plantations of the British and Spanish Caribbean, the absence of democracy mattered little. Nor was there a disjuncture between slavery and monarchical rule in Brazil. The great disjuncture, then, was between American democracy and slavery. The power of slave-holding interests in shaping antebellum politics is well documented and widely accepted by historians (Richards 2000, Brion Davis 2001). Recent research documents the reach of the 3/5 constitutional clause, which ensured that southern states disproportionately controlled the federal executive, legislative, and judicial branches for all of the antebellum period (1790 - 1865) (Wills 2003). Overall, this work is important because it has identified slavery as a fundamental variable for cross-national comparison in much the same way as colonial systems or party systems have been used in other seminal comparative scholarship.

The second question, which seeks to explain the emergence and softening (if not demise) of racial orders, is motivated by the recognition that slavery's abolition marked a fundamental rupture in economic and political arrangements. Abolition required, at the very least, that the boundaries of citizenship and economic arrangements be reconstituted, turning slaves into wage laborers, and non-citizens into citizens. Why and how did race then continue to qualify citizenship and largely determine class and social standing? The literature offers explanations that emphasize one of three interacting variables: class, ideas, and political elite design. On the class view, well exemplified in Stanley's Greenberg's dated comparison of the American South and Apartheid South Africa, class formation and racial orders are inextricably connected. Responding directly to the modernization literature's assumption that capitalist development would eventually abolish racial and ethnic identities, Greenberg argued the opposite, for the short term at least: that the demise of racial orders ultimately depended on the actions of white economic elites, or in his words, the "business challenge." According to scholarship that emphasizes ideas, such as that of George Fredrickson and my own work, racial politics in the United States, Brazil, and South Africa, depends crucially on the idea of race itself and on "white supremacy." These ideas, which provide the justifications for inclusion and exclusion and/or subordination within the body politic, are not entirely reducible to underlying political and economic motivations. For both Fredrickson and me, the dismantling of racial orders depends inescapably on attitudinal and ideational changes. Finally, Anthony Marx argues that racial orders in the U.S., Brazil, and South Africa are the products of elite design, with whites excluding blacks from full citizenship in the United States and South Africa because reconciliation after war required it. Brazilian elites developed no such exclusionary ideology because, without war or serious division among whites, reconciliation at the expense of blacks was unnecessary. Just as Greenberg argues that capitalism's dependence on racial barriers sows the seeds of discrimination's demise, Marx argues that exclusionary racial orders create their own opposition in the emergence of mass social movements. In my view, the cross-fertilization of ideas has strengthened theory-building across both subfields, most notably by directing the discussion away from "race relations" as an exclusively sociological phenomenon, toward the examination of the historical origins of racial categories and their impact on participation and distribution of political, economic, and social goods. Racial categories are derived from ideas (religious, scientific, cultural), political institutions (public laws and policies) and economic arrangements. This is not to suggest, however, that all political, social, and economic processes intentionally reinforce racial orders, although that is most often the case. It is also true that there are unintended consequences of both racially conscious and racially neutral institutional processes and political action. In sum, as scholars have come to view race as political in origin and consequence, and not only as a social problem or moral dilemma, comparative study, in which the U.S. case figures prominently, has been enriched and advanced.

However, it is the very prominence of the American case that has sparked criticism. As detractors see it, the comparative study of racial politics is a thinly veiled attempt to see racial subordination and attitudes where none exist. Brazilian and other Latin American scholars (and, to a lesser extent, French scholars) have voiced this view most forcefully. They object to what they see as the implicit assumption that American ways of viewing and treating race are (or should be) operative everywhere. Comparisons are, on their face, faulty because scholars use units of analysis that are incomparable both in definition and significance. Brazilian scholars argue that talking about "whites" and "blacks" within Brazil and further comparing their positions to American "whites" and "blacks" simply makes no sense. While it is

tempting to dismiss their objections as defensive and evasive, such a reaction misses the point. Recent scholarship that takes Brazilian views of race or color seriously also shows convincingly that race/color identifications are politically, economically, and socially consequential, in ways both similar to and different from other countries.

Yet, on the other hand, it is unlikely that the prevailing methods of the American politics subfield will come to dominate the comparative study of racial politics for two related reasons. First, the subject matter lends itself most readily to comparative historical analyses. Like other big topics, racial politics requires methods that can produce historically grounded explanations, which serve, in turn, as indispensable starting points for further inquiry. Second, insofar as quantitative analyses rely on statistical data, the data themselves must be reliable or, at the very least, intelligible. This is true of all statistical categories, but it is especially true in the study of racial politics, precisely because the categories - their content and boundaries - are contingent and require the nuanced treatment that qualitative methods provide.

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Comparative Politics and American Politics

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The United States may indeed be "the last case" in the sense that students of American politics are increasingly broadening their horizons, but the more apt point may be that it is not the last case, nor the first case, but simply put, just a case. As a practical matter, so many scholars here and abroad work with models and perspectives spawned in the study of American politics that the temptation is always there to treat United States as "the first case." I still remember teaching in Guatemala more than twenty years ago (on a Fulbright Fellowship) and encountering students brought up on a steady diet of translated American textbooks. They began most inquiries by searching for counterparts to US institutions and experiences: a particularly fruitless effort in Guatemala, and unlikely to be much more productive elsewhere.

My response to the invitation to comment on the questions posed by the organizers of this symposium is from the point of view of someone who came to political science out of a long fascination with consciousness, organization, and conflict: with politics that is, and not political science as such. I came to comparative politics out of a fascination with the realities of the "two-thirds world" (I use this term rather than the more common third world to underscore the geographical reality of the matter). A guiding principle of my own work has

been to pay attention to what is believed, thought, and said "out there": to learn, not just to study. This is, to be sure, a cliché, but what saves it from pure banality is the reality of ideas and concepts and aspirations that come to us from that world of lived, meaningful experience. They give us concepts and tools with which we struggle to make sense not only of their world, but of the world in general.

As social scientists we should strive to generalize: recognizing and respecting particularities of place and time and searching for principles of variation, but working to identify and account for some common underlying principles. My long interest in consciousness, conflict, and organization conditions my search for generalization not simply to observed regularities along some smallest common denominator, but rather to observed patterns in the elective affinity of ideas and audience, consciousness and context. To paraphrase Max Weber, my goal is not merely to identify a unit of behavior or the empirical regularity or "rule" that produces it, but to get at what "following a rule" means to those who organize their lives to follow such rules.

What does all this have to do with the questions that frame this symposium? For one, it has to do with the nature of theory, or what we accept as a theory. Theory, and by extension a "theoretical account of things," ought to explain and order reality. A theory should help distinguish relationships and provide a frame for bringing levels of analysis together in a coherent way. Theory is not a cookie cutter, but a template. A good theory should be "portable," able to do its work in distinct settings, but not in a mechanical way. In this light, theory is not exclusively causal, but more generally connective. The organizers ask us to consider whether greater cross-fertilization among our discipline's subfields has produced "stronger theory." Thinking about this question from my corner of comparative politics, what I note first of all is a series of retreats from grand theory and from theorizing on a grand scale, which may be one and the same. Nothing guts a theory like sustained empirical failure. In my own experience as a working scholar, I have witnessed at least four such failures: functionalism in the social sciences, functionalist theories of modernization and development, theories of secularization, and theories of dependency. All brought important elements to the table, all gained an enduring place in academic discourse because they were better than what came before, or perhaps because in some way they reflected some element of the Zeitgeist. Despite occasional efforts at a comeback (the current vogue for revivals of modernization theories comes to mind) such grand schemes failed because too much reality was excluded from the world to which they pointed scholars, and eventually those realities burst in to make the shortcomings of the theory all too painfully evident.

Following on the failings of grand theory, or at least of the grand theories noted above, and of a style of work that searches for a single organizing principle for everything, scholars in our discipline have moved in several directions. There has been concern for conceptual precision, with much effort devoted to creating categories and typologies, and then fitting phenomena into the categories these new classifications provide. Although much of this work is helpful, classification as such cannot be our goal. One lesson that has remained with me from graduate school is that typologies are subordinate to theory. When we identify important phenomena and sort them into categories we do so in service to a theory that tells us why they are important and how to analyze their workings alone or in combination with others. Creating typologies and pursuing conceptual clarification are by themselves a dead end: they must be subordinate to a theoretical enterprise.

Not all scholars have turned to classification as a solution. There has also been a search for "better" general theories, a concentration on greater empirical precision, and a growing focus on the construction of mathematical models of politics. It is evident that our discipline has been strongly influenced in recent years by the evolution of quantitative analysis toward an increasing focus on the construction of mathematical and (hopefully) portable models of politics. This has been accompanied by a growing body of scholarship and discourse centered around themes derived from rational choice theory, to the point that one commonly finds work self-described as attending to the "micro foundations" of institutions, as if the only possible "foundation" were one rooted in an individual calculus of utility and marginal benefit. This is methodological individualism at industrial strength concentration. A spinoff of this perspective is the regular and repeated reference to the "collective action problem" to which many of us who have studied actual social movements respond: if it is such a problem, why is there so much sustained collective action?

The unstated premise of this symposium is that the alternatives just mentioned have gained greater prominence in comparative politics as a result of the borrowing and "cross-fertilization" of this field with American politics and the increasingly quantitative "mainstream" of political science as a whole. A common complaint of many students of comparative politics is that these approaches leach out

much of the variation whose analysis is at the center of what passes for comparative research, however defined. Elements of history, specific historical configurations, and of course cultural variation, are lost in the very definition of the problems. I share much of this critique, although I acknowledge that elements derived from rational choice models can put evidence in a new and fruitful light. To cite an instance from my own area of interest, analysis of the dynamics and consequences of religious pluralism in Latin America is enriched by an understanding of scholarship working with data from the US and Europe, and inspired by economic theories of competition, that offer coherent models of growth and competition. Where rational choice and game-theoretic models fail here (and I suspect elsewhere as well) is in failing to explore the reasons why groups compete for members (it really is not just a numbers game) or to contextualize the phenomena in a convincing way. They narrow motive excessively, and all too often dismiss concern with meaning as, well, meaningless.

The drive for greater empirical precision and by extension, mathematical models of politics, is surely long overdue in comparative politics. Done well, these endeavors enhance the ability of comparative scholars to interact with and be heard by the rest of the discipline. Where they arise, difficulties stem not so much from the goal, as from insufficient acknowledgement of the realities of data quality and even data availability. Anyone who has worked outside the so called 'developed world'' has innumerable stories of the fruitless search for reliable information, of holes in the data, of the unavailable or grossly unreal census. It is common to pose this problem in terms of area studies vs. disciplinary concerns, but this is surely a false dichotomy. The goal of having comparable data need not conflict with attending to cultural or contextual variation in meaningful ways. The issue is not whether, but how we do it.

It is evident to me that there are important lessons to be learned for understanding the politics of any particular place or time from the politics of any other. A librarian asked me once why someone interested in Latin America and in Catholicism and politics was borrowing books on Islam in Indonesia. It is the same planet, I said, and so it is: in a profound sense, it is about us. We do, after all, live on the same planet and work with the same human tools of reason, language, emotion, and memory, albeit put together in distinct configurations. With appropriate modifications and adaptations empirical models can travel well. Methods can also travel well, as long as one is willing to accept a range of data and understand that the data common to students of American politics is often simply not there in other parts of the world. The point I want to make is that influences and models do not travel in one direction only. My own research and thinking on religion and politics has been strongly shaped by perspectives and experiences created in Latin America and other parts of what I have referred to here as the "two-thirds world." The point holds for studies of democracy, democratization, social movements, to name only a few areas of concern.

Central to any definition of the enterprise of comparative analysis is attention to the sources of change and variation across cases, and more broadly, across categories of cases. The effort can be framed as a move from one steady-state equilibrium to another or, hopefully, in ways that take change as normal and continuous. However the question is framed, there is an alternative here to the failure of grand theory that involves neither a retreat from theory nor a turn to abstraction. This alternative draws on traditions of comparative historical analysis, enriched with attention to cultural issues. Such work is nourished less by economics and psychology (the wellspring of rational choice and cross-national survey research) than by sociology, anthropology, and sociologically informed history. These fight an uphill battle in our discipline but there is evidence that they are percolating into the study of American politics, for example in the exciting work underway on American political development, which infuses the understanding of themes as diverse as the development of a national bureaucracy, the emergence of a given policy consensus, the transformation of political parties or social movements in the United States with insights from comparative research. Whatever the influences, they surely run in more than one direction.

Technology, Freedom, and Democracy: an Evolving Debate

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Ithiel de Sola Pool (1983). *Technologies of Freedom* (Cambridge, MA: Belknap/Harvard).

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In his 1983 book *Technologies of Freedom*, Ithiel de Sola Pool wrote that "freedom is fostered when the means of communication are dispersed, decentralized and easily available, as are printing presses and microcomputers" (5). Writing well before the Web was invented, and when email was in its infancy, Pool saw emerging electronic media as potentially heralding a new era of free speech in the United States. Far from considering this outcome an inevitable product of the features of new technology, however, Pool stressed the political nature of the regulation of technology, the persistence of older regulatory frameworks despite technological change, and the real possibility that government control would squelch the freedom-enhancing potential of computer-based communication.

In the twenty-one years since his volume was published, Pool's incipient "technologies of freedom" have evolved into a commercial, multimedia network connecting nearly every country, including many where the same normative and legal protections for free speech do not apply. As these changes have unfolded, scholars have sought to analyze both sides of the reciprocal relationship between governments and their citizens' use of new communication technologies - the impact of state regulation on the freedom of communication, and the impact of this communication on the nature of governments and political regimes. With the Internet's initial growth occurring primarily in the advanced democracies, a first wave of research examined the questions of state regulation and individual liberties. As the Internet began diffusing to developing countries (including a number of authoritarian regimes) in the mid- to late-1990s, scholars also began to address the second question, focusing on democracy and democratization. Throughout the ongoing discussion of these issues, a central focus of the debate has been the relative weight of technological characteristics in determining political outcomes.

In this essay I review Pool's book along with several more recent contributions that represent different stages in the debate over technology, freedom, and democracy. Given the interdisciplinary nature of this research question, scholars of law, communications, and public policy have made important contributions to the debate; the books reviewed here reflect that diversity, though all should be accessible to those with only a political science background. In the conclusion I suggest several opportunities for further research in this emerging field of inquiry.

Media Convergence and the Threat of Government Regulation

In a book that was both unusually prescient for its time and also particularly cognizant of historical context, Pool surveyed the evolution of regulatory frameworks for different media in the United States and outlined the challenges for those who wish to see the First Amendment protection of print media extended to electronic speech via computer networks. The history of strong legal protections for publishing contrasts sharply with the regulatory regime for broadcast media, where authorities have imposed much greater restrictions on who can communicate and what content is allowed on the air. Both of these approaches were initially justi-

fied on the basis of technological characteristics - the widespread diffusion of the printing press versus the scarcity of frequencies in the broadcast spectrum. Nonetheless, each regime persisted through subsequent technological changes-the development of capital-intensive printing technologies that favored large newspaper publishers over small print shops, and the invention of cable television where the spectrum scarcities of early radio did not apply.

While each regulatory regime evolved separately, Pool argued that they were intersecting with the development of new electronic communications because of the "convergence of modes" - the ability of computer networks to carry print, voice, and video traffic. Given the history of treating new technologies as analogous to old ones, this development raised the question of which regulatory regime would prevail. Pool saw computer communication as similar to the printing press in its minimal expense and ease of accessibility, but he also recognized that the same First Amendment protections would not necessarily be extended. *Technologies of Freedom* ends on an optimistic note about the American commitment to pluralism as well as the "pliancy and profusion of electronic technology" (251), but one gets the sense that this was more hope than prediction. In the struggle between technologies that may favor freedom and governments that seek to control them, Pool certainly saw the potential for regulators to gain the upper hand.

Pool's perspective was unabashedly libertarian, and many will disagree with his assertion that free markets are more conducive than government regulation to free media, especially in an era where deregulation has allowed for such concentration in the ownership (and arguably, the content) of the means of communication. On the whole, however, the book was a valuable early contribution, both for its historical perspective and for insisting that the relationship between technology and freedom is determined by both technological characteristics and institutional contexts.

The Libertarian "Gotcha": The Impossibility of State Control

A libertarian perspective on the regulation of communications media has been a common one among American scholars, and similar sentiments prevailed during the mid-1990s as the incipient electronic media of *Technologies of Freedom* gave way to the global Internet. In contrast to Pool's emphasis on the threat of government regulation, however, much of the early analysis of the Internet argued for the impossibility of effective government control - a position James Boyle once described as the "libertarian gotcha" (Lessig 1999: 5). This position, expressed in the 1997 edited volume *Borders in Cyberspace*, derives from two key characteristics of the Internet: its global, transborder nature and the particular technological tools it offers for concealing communication. In assessing the relative freedom of technology use, *Borders in Cyberspace* thus takes a step away from Pool's emphasis on the influence of regulatory institutions and places greater weight on the nature of the technology.

The contributors to *Borders in Cyberspace*, who are mostly legal scholars, emphasize the difficulty of territorially-based regulation of the Internet by nation-states. Post and Johnson's opening essay is perhaps the best statement of the legal issues involved. Noting several prominent lawsuits of the mid-1990s, such as Germany's effort to stop U.S.-based

"any government that allows its citizens to become a part of the global electronic network will be forced to live with a freedom of speech even greater than that contemplated by the authors of the First Amendment" (Froomkin, 148).

Compuserve from allowing German residents to access online pornography, Post and Johnson argue that the rise of the global Internet poses complications for legal systems built on territorial jurisdiction, and that cyberspace will only be effectively governed if it is considered a separate "place" with its own set of rules. Michael Froomkin's essay adds additional perspective on the Internet's technological characteristics, showing how the network was designed to route around impediments to the free flow of information and how cryptography and anonymizing technologies can make it difficult to identify the sender of a message or its contents. In contrast to Pool's concerns about the challenges to free speech online, Froomkin argues that "any government that allows its citizens to become a part of the global electronic network will be forced to live with a freedom of speech even greater than that contemplated by the authors of the First Amendment" (148).

While most of the chapters in *Borders* focus on how well-established

democracies can regulate use of the Internet, Kedzie's chapter takes an explicitly global approach and examines the other side of this reciprocal relationship: the effect of computer-based communication on governments and regime type. In doing so, Kedzie takes a step beyond Pool's argument, hypothesizing that multidirectional, reciprocal communication technologies like email are conducive to democracy in a way that broadcast, print, and bidirectional media like the telephone are not. To test this hypothesis, Kedzie estimates a series of statistical models of the cross-national relationship between connectivity to email networks and the Freedom House scores for civil and political liberties. He finds a persistent correlation between these two variables across different models and functional forms, though he is appropriately cautious about interpreting causality and suggests that the most likely relationship between the two is a virtuous circle. One may quibble with Kedzie's vagueness about causal mechanisms or certain elements of his operationalization and statistical analysis,[1] but his results do suggest an interesting empirical relationship that is worthy of further testing.

The Sources of Effective Regulation: Flexible Technology and Institutional Constraints

On the whole, *Borders in Cyberspace* is a useful statement of the optimistic view of communication technology, freedom, and democracy, but its perspective draws heavily on technological determinism and there are limitations to its conception of the nature of the Internet. Lawrence Lessig's *Code and Other Laws of Cyberspace* responds to these drawbacks, seeking both to reinterpret the nature of Internet technology and to bring law and politics back into the discussion of Internet regulation. Lessig's first critique is that the control-frustrating technological characteristics touted by the Internet's libertarian boosters are not necessarily static or characteristic of the Internet as a whole. Encryption technologies may facilitate anonymity, but other features that allow for identification may be added to the Internet, such as the "cookies" that identify repeat visitors to a web site. Different component networks of the Internet can have different technological features, some facilitating greater surveillance and control - America Online, for instance, allows for much greater oversight of users' activity than most university networks.

Lessig's second critique of the libertarian position concerns the difference between perfect and effective control. Limiting every individual's online behavior is not necessary, he argues; strong but imperfect deterrents can be very effective. Technological features are one type of constraint on Internet use, but they can be supplemented by institutional constraints: law (which governments manipulate directly), as well as social norms and market incentives (which can be used as indirect forms of regulation).

According to Lessig, the combination of technological and non-technological constraints can act as an effective deterrent even when it is possible to circumvent these restrictions. Lessig does not fully shift the balance away from the influence of technological characteristics; ultimately he believes that the architecture (or "code") of the Internet is paramount. But by showing that this architecture is flexible, can be directly regulated, and can be supplemented by other forms of regulation, Lessig outlines a complex (if not always clear) interplay that brings non-technological variables back into the argument.

Lessig's main concern is with the commercial control of the Internet in the U.S., and despite the arguments summarized above, he sometimes appears skeptical of governments controlling the Internet elsewhere. In a frequently cited statement, Lessig argues that the United States has "exported to the world, through the architecture of the Internet, a First Amendment in *code* more extreme than our own First Amendment in *law*" (167). But this property can be altered by those who construct the architecture of the Internet. In the U.S., this job is generally done by privately-employed hardware and software engineers; thus Lessig envisions them instituting elements of control that serve primarily commercial purposes. However, when the engineers developing the technological characteristics of computer networks are state employees in authoritarian regimes, the elements of control they implement may serve the interests, for example, of the Chinese government rather than Microsoft. Thus, while Lessig does not spell it out himself, his perspective suggests the possibility of authoritarian regimes exerting control over portions of the global Internet within their purview.

Generalizing the Argument: Internet Regulation in Developing Countries

Marcus Franda's *Launching into Cyberspace* is motivated by this basic question of whether countries outside of the advanced democracies have accepted or resisted the free flow of

information on the Internet. Franda positions himself somewhere between the libertarian perspective of *Borders in Cyberspace* and the image of pervasive control in Lessig's *Code*, stressing political culture and regime-related variables in explaining how and why developing country governments seek to regulate the Internet. While the new democracies of Eastern Europe have sought to satisfy the freedom of information requirements necessary for joining the European Union, Arab governments of the Middle East have taken a more cautious approach because of their sensitivity to political dissent and pornography, and Saudi Arabia and the United Arab Emirates have implemented extensive mechanisms for Internet censorship. Likewise, China seeks technological control of the Internet in part by channeling international traffic through a gateway largely controlled by state-owned China Telecom.

"The principal value of *Launching into Cyberspace* lies in its bringing empirical evidence to bear on the question of internet development outside of the advance democracies."

Franda views the question of Internet regulation in developing countries through the lens of the international regime for Internet governance, and his analysis asks whether states that oppose the principles of this regime seek to engage with and reshape it or simply pursue their own national policies. While this angle is intriguing, its application in *Launching into Cyberspace* is problematic for several reasons. First, it is questionable whether governance of the Internet actually does promote the free flow of content, as Franda assumes; most states have agreed on other elements of openness such as non-proprietary technological standards, but they have widely contrasting preferences about content regulation (Drezner forthcoming). To the extent that states implement national policies restricting free information flow, Franda interprets this behavior as deviant and "isolationist," and he is skeptical of its sustainability without offering a compelling explanation. But this focus on Internet regulation as a knee-jerk reaction to supposedly global norms de-emphasizes the reasons why authoritarian regimes proactively seek to guide Internet development according to a national plan. Top-down control of network infrastructure facilitates not only the censorship of public Internet use, but also improving information flow between different ministries to boost governmental efficiency, or prioritizing Internet development in certain key industries when international bandwidth is limited.

The principal value of *Launching into Cyberspace* lies in its bringing empirical evidence to bear on the question of Internet development outside of the advanced democracies. Franda's book is one of the first to gather cross-national qualitative data on this question and analyze it within a single theoretical framework, and his global focus is welcome given that many studies look within one region only. Unfortunately, the evidence from different cases is presented in a somewhat uneven and ad-hoc fashion, complicating systematic comparisons between them. To some extent this characteristic may spring from Franda's rather unwieldy focus on 42 countries. Nonetheless, *Launching into Cyberspace* is an important step away from the preponderance of anecdotal evidence about the Internet in the developing world, much of which reached the same sort of conclusions as early libertarian statements about the Internet in the advanced democracies.

Shifting the Focus: Internet Use and Democratization

While dealing centrally with the question of whether developing countries resist the free flow of information online, Franda does not directly address the question posed by Kedzie - whether the global diffusion of the Internet has implications for democracy. This question is the one taken up in the recent edited volume *Rhetoric and Reality*, which looks at the political impact of the Internet in nine Asian countries ranging from Singapore to Japan. In the introduction to the book, Indrajit Banerjee accepts that the Internet's architecture may make it difficult to control, but he pays greater attention to the social, political, and economic conditions prevailing in different countries and how these act as moderating variables between the Internet and any political impact. Since the Internet is an active medium, he argues, its impact depends on how users employ it. This explicit focus on Internet users is a productive one, moving beyond Franda's more top-down focus on government regulation.

Several contributions to *Rhetoric and Reality* offer particularly useful case studies. Kluver and Qiu's chapter on China is probably the strongest in the volume. While they cover the standard topic of online dissent by the Falun Gong and pro-democracy activists, they argue that the more

significant political impact of Internet use lies in two less-heralded areas: e-government (which facilitates surveillance but may also reduce corruption) and online chat rooms (which permit criticism of government policies, but also serve as a useful barometer of public opinion). Banerjee and Yeo's chapter on Singapore emphasizes citizens' political apathy in the context of successful economic development and the government's efforts to encourage self-censorship among a population that is aware of being frequently monitored. The authors also raise a theme first touched upon by Pool-the government's efforts to regulate the Internet by bringing it under the regime for broadcast media where mechanisms of control are already well-established.

Despite a number of useful insights, *Rhetoric and Reality* generally falls short of its potential as a comparative endeavor. Several other chapters are weaker in their analysis, consisting of somewhat ad-hoc surveys of Internet use in each country without a central argument. The book's introduction anticipates this criticism and appropriately points out the exploratory nature of the research, but more could arguably have been done even in a preliminary study. The volume lacks a concluding chapter, for instance, that could have drawn the disparate evidence from each case into a common comparative perspective. Another problem lies in the lack of a shared conceptualization of the study's main dependent variable, democracy. The concept is alternatively treated as a Western notion concerning procedures for electing representatives; a diminished subtype ("contextualized democracy") involving restrictions on the media; a normative concept where "the people" have equal voice in government, and a situation of good governance and Weberian bureaucracy. The introduction notes the contested nature of the concept and argues that "there can be no single conception and experience of democracy in Asia" (8). But establishing common definitions for the purpose of analytic clarity is different from assuming that citizens conceive of or experience democracy similarly in each country.

Conclusion: Research Opportunities for Comparative Politics

What can we say about the study of technology, freedom, and democracy more than twenty years after the publication of Pool's pioneering volume? Clearly, while technology itself advances rapidly, the comparative study of its social and political impacts is still in its infancy. Problems of data availability persist, especially when studying the Internet in authoritarian regimes. Nonetheless, there is potential for scholars of comparative politics to make a significant contribution to this stream of research in the future.

As research begins to examine the impact of Internet use on authoritarian rule, a number of insights from the field of comparative regime analysis can contribute to our understanding of this dynamic. While the mainstream literature on democratization has generally been silent on the role of communication technologies (Kalathil and Boas 2003: 3-4), scholarship in this field has carefully examined the concept of democracy and the types of processes that result in transitions from authoritarian rule. Since communication technologies are ultimately tools employed by political actors, their role in democratization is likely to come in areas already identified, such as the organization of popular protest. At the same time, an appreciation of the complex set of processes involved in democratization helps us realize that Internet-facilitated protest does not equal incipient regime change-an assumption of many anecdotal statements about the Internet in authoritarian regimes. It is also important to recognize that if we are interested in the political impact of the Internet in authoritarian regimes, democratization is not the only relevant dependent variable. As Kluver and Qiu note in their chapter in *Rhetoric and Reality*, state use of the Internet for the reform of public administration in China may be more politically salient than online dissent, even if its implications for democracy are uncertain or mixed.

In contrast to the burgeoning research on state control of the Internet in authoritarian regimes, much of the early research on government regulation of the Internet in the advanced democracies has given way to concerns over commercial control. Nonetheless, the issue of state control of the Internet and its implications for civil liberties in the advanced democracies is an increasingly important question in the current international security environment. Here, there is potential for comparison between democracies and authoritarian or semi-authoritarian regimes. For example, there are striking similarities between Russia's System for Operational-Investigative Activities (SORM) and the United States' Carnivore program, both of which require Internet service providers to install hardware and software that facilitate monitoring by domestic intelligence agencies. The major difference may be the degree

of judicial oversight of this exercise of executive power.

In pursuing these and other research questions related to technology, freedom, and democracy, a number of methodologies and approaches will prove useful. Pool's analysis highlights the value of historical institutionalism: regulatory regimes do not spring out of nowhere when new technologies appear. The Internet may seem like a fundamentally new phenomenon, but the ability of many governments to regulate Internet use and development depends upon the capacity of previously established institutions to control other media and to promote the development of science and technology.

"... it is even more important to remember Pool's argument that technology only sets the stage for what are ultimately political struggles with political outcomes."

Kedzie's initial quantitative analysis into communication technology and democracy also highlights the potential contribution of this relatively unexplored avenue. Data on relevant indicators are plentiful: the World Bank's World Development Indicators include annual statistics on Internet hosts and number of users, and cross-national data on democracy and its more traditional covariates are available from a variety of sources. A time-series cross-sectional analysis from the mid-1990s to the present would update Kedzie's initial inquiry and could address some of the methodological doubts that remain in his study. Further statistical analysis of this relationship would still leave unresolved questions of the nature of causality, but combined with small-N qualitative studies that probe causal mechanisms, quantitative analysis could form a productive research program.

Most significantly, political scientists can bring their understanding of politics to bear on a question that has often been addressed by people with a stronger understanding of technology. Many concepts that are central to political science remain under-analyzed in current research on technology, freedom, and democracy. The question of state capacity is central to the efforts of any government to either control the Internet or promote its development. Likewise, the problematic assumption that unrestricted data flow is necessary for economic prosperity in an interconnected world could be refined through the better application of insights from political economy. An understanding of technology is undoubtedly important for research in this field. However, it is even more important to remember Pool's argument that technology only sets the stage for what are ultimately political struggles with political outcomes.

Notes

[1] In terms of Kedzie's operationalization, the number of email users in a country would better fit with his theory than the number of hosts, and using data more recent than 1993 would be useful given that the global diffusion of the Internet was only incipient at that point. As for Kedzie's statistical analysis, the model estimating change in democracy over time does not appear to control for non-technological predictors (as his static models do).

A Review of Kaufman and Segura-Ubiergo's dataset on *Globalization, Domestic Politics, and Social Spending in Latin America*

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The interconnections among domestic politics, social spending and economic internationalization in Latin America are the focus of the dataset compiled by Robert Kaufman and Alex Segura-Ubiergo. Although similar datasets have been compiled for larger global samples, the focus on this particular region represents an important step towards a greater understanding of the impact of globalization in less-developed countries.

This dataset includes information drawn from various sources for fourteen Latin American countries from 1973 to 1997, covering 25 variables and 351 observations. The countries are Argentina, Bolivia, Brazil, Chile, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Mexico, Paraguay, Peru, Uruguay, and Venezuela. This represents a quite comprehensive coverage of the region, even though six nations of the region are omitted due to missing or noncomparable data.

As the authors point out in their article in *World Politics*, "Globalization, Domestic Politics, and Social Spending in Latin America" (which was one of the runners-up for our section's Sage award), the effort to construct this dataset was justified by the need to understand the relationship among several critical trends in Latin America. The region is deeply embedded in the global trade and investment network of the last decades. There have been both positive and negative results from such integration: on the one hand, greater access to foreign markets; on the other, increased inequality. The region has also been undergoing significant political and institutional changes since the 1980s as a result of democratizion. The effects of these factors on the spending decisions of countries that constructed welfare systems with strong popular appeal in the first half of the past century suggest an interesting area of investigation, as social expenditures are the main resources that governments can utilize to respond to popular demands. The dataset usefully covers a 25-year period that reflects these critical economic and political trends-- debt crisis and recovery, the restructuring of economies along neoliberal lines, and democratic transition and consolidation.

It is in the social expenditures variables that Kaufman and Segura-Ubiergo greatly innovate, going beyond previous studies (Garrett 1999 and Rodrik 1997) that use aggregate measures of government spending that do not accurately reflect social spending per se. This dataset contains a variety of measures of social expenditures taken from the IMF's *Government Finance Statistics*, which comprise nine of the twenty-five variables. Social security, health care and education spending are included in aggregate form, and also broken down into two separate variables so as to distinguish between pension-related payments and human capital expenditures. This allows investigators to explore the reasoning and target population behind different government expenditures. This is critical for Latin America because of the widespread differences in benefits accruing to the formal and informal labor forces. Formal sector workers contribute to and thus receive social security resources, while informal sector workers are more likely to receive a piece of the health and education than social security expenditures. At least one later study takes account of this and has disaggregated these categories into their three components for use as dependent variables (Avelino, Brown and Hunter 2002).

In addition, these different categorizations of spending are measured in three different ways that allow for a broad perception of their impact. For instance, social security, health and education expenditures as a percentage of GDP and as a share of central government spending yield indicators of how much social spending represents within the larger national and sectoral expenditures. Furthermore, social spending is measured in per capita 1995 dollars, which supposedly indicates the proportion of social resources that are being distributed to every recipient. However, the authors are well aware (and future users of the data should take note) of the validity problems with these indicators, as they do not account for maladministration of the social security, health and educational systems of each nation.

Neither do they include state government disbursals, which may be significant considering the increased fiscal

decentralization that has characterized many Latin American countries. It is harder to come across these figures consistently in a cross-regional context, and compensatory mechanisms (such as excluding strongly decentralized countries) have resulted in insignificant changes in several studies (Kaufman and Segura-Ubiergo 2001; Brown and Hunter 1999). Nevertheless, these figures may represent an important complement to this dataset. Another problem that is worth mentioning is that there are several missing values for these variables, some of which for prolonged periods for countries such as Peru and Venezuela (1987-1997), Brazil and Ecuador (1995-1997), and Paraguay (1994-1997). Just why these are missing is not clear, especially since these represent later democratic periods for which IMF data should be available. These caveats aside, the three-way strategy and the differentiation between types of social spending represent an improvement over previous aggregate indicators.

Other domestic economic, political and socio-demographic variables make up the bulk of this dataset. Economic indices include GDP growth, GDP per capita, inflation rate, value added in the manufacturing sector as percentage of GDP, GDP per capita in purchasing power parity (PPP), and the real exchange rate. This represents a large portion of World Bank (*World Development Indicators*) figures that summarize the strength of the economy. However, indices of inequality and unemployment are excluded which may strongly affect the types and amounts of social expenditures by governments, especially if significant sectors of the population link inequality or unemployment with openness to international markets (Avelino, Hunter and Brown 2002).

One weakness of this dataset is its lack of valid measures of the strength of interest groups with respect to governmental decisions on social expenditures. Although it includes variables that are commonly taken into account in specifying social spending equations, such as the ratio of dependents to the working-age population, the percentage of the population over age 65, and the percentage of the population that lives in urban areas, it lacks direct indicators of civil society pressures on government. The proxy used to get at the political power balance is simply a dummy variable for popularly based presidents who were in office during the first six months of the year. It is coded as 1 when the president's party is historically linked and/or has programmatic ties to labor unions or to the popular sector, and 0 otherwise. Although this indicator agglomerates democratic and autocratic regimes with a popular base, the coding seems to make sense in the majority of cases. Nevertheless, there are cases that raise reliability issues. One example is Chile's Concertación period from 1990-1997, which is also a period in which labor struggled to maintain its social benefits. Another issue is that this variable does not take into account whether governments made spending decisions that were favorable to popular sectors. Perhaps the inclusion of variables measuring degree of popular or labor mobilization and unity would increase the validity of the political variables in this dataset. This may be one of the limitations of the heavy reliance on international sources of data such as the IMF and World Bank.

Also among the domestic political variables are the 10-point democracy and autocracy scales of Jaggers and Gurr's Polity III/IV, which reflect institutional characteristics of these regime types and have been widely used and tested for validity. The fact that the authors leave the variables in continuous form in the dataset despite their use of democracy as a dichotomous variable (a democracy is coded as 1 if it scores 6 or more after autocracy is subtracted from democracy) in their article is a plus in that it allows future users the flexibility to use the variables in their preferred way.

The final set of variables encompasses the extent of penetration by and in international economic markets. Here the dataset creators incorporate existing indicators of the degree of trade integration (imports plus exports/GDP) and openness to capital mobility using widely-used World Bank and Economic Commission for Latin America and the Carribean-ECLAC data respectively as a sources.

Given the scarcity of reliable economic, political and social indicators for Latin America, this effort at constructing a large-N dataset specific to the region must be considered a very positive contribution, especially since it contains concrete measurements of some concepts and themes that have been largely untapped in scholarly work. Although there are several areas in which it could be improved, this dataset marks a solid beginning. The dataset is available on the APSA-CP website *http://www.nd.edu/~apsacp/SeguraKaufmanDataSet3.xls* and explanations of its variables are included in the *World Politics* article.

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Editor's Note

The editors welcome suggestions of other relatively new and potentially useful datasets that should be announced or reviewed in APSA-CP. Anyone interested in reviewing a dataset for the newsletter, along the lines of Patricia Rodriguez's review of the Kaufman and Segura-Ubiergo dataset, should contact Michael Coppedge at coppedge.1@nd.edu.

Other Datasets

District-Level Electoral Data from around the World

Scott Morgenstern (Duke University)

This compilation of data brings together district-level electoral data for 18 countries across the world for different time periods, with more data available soon. The data is availble in Excel format and sources are noted on each worksheet. Each worksheet is organized around regions, districts, and party results for each election.

The dataset is available at:

http://www.duke.edu/~smorgens/componentsdata.html

Nationmaster.com

Nationmaster.com is a relatively new website that makes it easy for anyone to access a wealth of cross-national data. It claims to have compiled 921 variables from publicly available sources. A variety of variables is available on democracy (20 variables, including some recent election results), economics (99 variables), education, energy, the environment, "government" (36 variables), labor, media, militaries, "people," religion, and ten other categories that are less relevant for comparative politics. Sample size varies, depending on the sample size in each source. A few variables are time-series. Drop-down menus allow users to produce an HTML list of the value of a selected variable for all countries or for selected countries. The site has two highly distinctive features. One is its graphical interface. Users can easily produce color-coded maps showing the worldwide distribution of values on any of the values in the database, and even zoom in on major world regions. The second feature is that by clicking on any country, users access a more detailed political map of the country and an encyclopedia entry about it. For a small fee, users can also generate printable maps and graphs, bivariate scatterplots, and three-variable bubbleplots; the other features are free. This is a valuable reference resource that could be especially useful for undergraduate teaching.

http://www.nationmaster.com/index.php

The Center on Democratic Performance at Binghamton University

The Center on Democratic erformance at Binghamton University has launched the Election Results Archive (ERA), a collection of electronic files containing data on election results from around the world. This is an online database with global coverage provides researchers, policy-makers, scholars, and others interested in elections with information on over 900 elections from around the world. It includes information on presidential and national legislative elections in 134 countries that have met a minimum threshold of democratic performance for the year in which the elections took place. The ERA contains results back to 1974. This date was selected because it is frequently cited as a beginning point of the recent phase of democratic expansion (democratic elections in Greece and Portugal). More election data will be added to this Archive as time and resources permit.

The archive can be searched by country, region, or year and type of election.

http://cdp.binghamton.edu/era/index.html

APSA Centennial Center for Political Science & Public Affair: Visiting Scholars Program

The American Political Science Association recently opened the Centennial Center for Political Science & Public Affairs in its headquarters building in Washington. As part of its programs, the Centennial Center assists scholars from the United States and abroad whose research and teaching would benefit from a stay in and access to the incomparable resources available in the nation's capital. The Center provides Visiting Scholars the infrastructure needed to conduct their work, including furnished work space with computer, phone, fax, conference space, and library access.

The Center has space to host 10 scholars for extended periods of time, ranging from weeks to months. Space for shorter "drop-in" stays is also available. Scholars are expected to pursue their own research and teaching projects and contribute to the intellectual life of the residential community by sharing their work with Center colleagues in occasional informal seminars.

Eligibility is limited to APSA members. Senior or junior faculty members, postdoctoral fellows, and advanced graduate students are strongly encouraged to apply. A short application form is required and submissions will be reviewed on a rolling basis. Positions are awarded based on space availability and relevant Center programming.

For more information and an application please visit the Centennial Center web site www.apsanet.org/centennial-center or call Sean Twombly at 202.483.2512.

Gregory Luebbert Article Award

Best article in the field of comparative politics published in 2002:

Pamela Johnston Conover, Ivor Crewe, and Donald D. Searing, “The Deliberative Potential of Political Discussion,” *British Journal of Political Science* 32: 1 (January 2002): 21-62.

Honorable mention:

Gary Cox and Scott Morgenstern, "Latin America's Reactive Assemblies and Proactive Presidents," *Comparative Politics* 33: 2 (January 2001): 171-89.

The members of the award committee were Gary Marks, chair (UNC-Chapel Hill), Gerardo Munck (University of Southern California), and Deborah Yashar (Princeton University).

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