

The Organized Section in Comparative Politics of the American Political Science Association

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## Letter from the President

### The Role of Cross-regional Comparison\*

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Systematic cross-regional comparison is neither widely practiced nor written about from a methodological point of view, in contrast to case studies, small-N comparative historical studies within the same region or where region does not figure as a variable, and large-N quantitative studies. What I mean by systematic cross-regional comparison is a combination of within- and cross-region comparisons of several cases, which entails the identification of regional patterns and the comparison of these patterns to each other. To some extent, the reasons for the relative scarcity of such comparisons are obvious; analyzing several cases in two or more regions to establish regional patterns demands significantly increased investment in case knowledge. However, the potential payoffs are significant in terms of refinements of concepts and theories, and cross-regional comparison deserves a central place among our research designs. Cross-regional comparisons can do at least three things: (1) They can increase confidence in the usefulness of our concepts and theories, if we find similar processes in widely different contexts. (2) They can force us to modify concepts and better specify theories with regard to contextual variables. (3) They can highlight the existence of different paths to the same outcome

and thus the need to develop new theories.

To support both arguments, regarding the relative scarcity of systematic cross-regional comparison and the potential payoff of such comparison, it is useful to start with the list of most widely assigned books in comparative politics courses and comprehensive reading lists published in the last issue of this Newsletter. Of the 30 books on the list, only four engage in systematic cross-regional comparisons based on empirical evidence. Almost two thirds of the others, if we count individual essays in edited volumes, compare cases from different regions but without explicit reference to regional patterns. In part this is because they choose one case per region only, in part because they use examples from different regions to illustrate general theoretical arguments but don't treat these cases in a systematic fashion. Still others establish and compare patterns based on factors other than region, such as waves of democratization, or paths to modernization. I shall begin with a discussion of the four books from this list and then briefly review some additional contributions to knowledge that cross-regional comparisons have made.<sup>1</sup>

Sartori (1976: 266-7) makes the point that cross-regional comparisons can be powerful in negative terms, that is, he talks about the "boomerang effect" of the inappropriate application of

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concepts developed for societies with well-consolidated states and party systems to formless states and non-consolidated party systems. He argues that this "boomerang effect" undermines the concepts themselves and thus the discipline as a whole. However, we can equally well identify a positive "boomerang effect," that is, a refinement of concepts and a revision of theories on the basis of their confrontation with new cases in different regions. In fact, Sartori's own conceptual discussion of political parties and party systems in the African context of the first three decades after World War II sensitizes us to the problems in applying our conventional notions of parties and party systems to an analysis of the processes of democratization in Latin America and Eastern Europe in the 1980s and 1990s.

Our concepts and theoretical expectations of party behavior were largely developed on the basis of the European experience, where parties were formed in societies with clearly defined sociological constituencies and relatively dense civil societies, and in states that enforced the rule of law across these categories and throughout their territories. Organizations that perform the main function of parties, presenting candidates for election, but exist in societies with very large informal sectors, ill-defined sociological constituencies and weak civil societies, and in states unable to enforce the rule of law evenly, as in many Eastern European and Latin American countries, require different concepts and theoretical expectations (e.g. Roberts 2002). However, this does not mean that the original and the modified concepts and theoretical expectations are

unconnected or non-comparable. On the contrary, it is precisely these comparisons and modifications that help us specify the contextual variables that shape party structures and functions.

Anderson's (1983) comparison of the rise of nationalism in Europe, South America, and post-colonial Africa and Asia provides the second example of the insights gained from cross-regional comparisons. One crucial difference across regions, of course, is timing and thus the absorption of ideas and techniques from Europe and the Americas by nation-builders in Africa and Asia. An important difference between Europe and the Americas is the link to mass politicization in Europe, which was absent in the Americas, where nationalism was essentially an elite project. The crucial similarity between the Americas and Africa and Asia is the fact that the new states coincided with previous colonial administrative units, and that therefore an incipient "imagined community" had been formed among the local officials through the colonial pilgrimages to the colonial capitals. These comparisons give us a much more nuanced picture of nationalism than its study in one region alone, and an appreciation of partially different paths to the same endpoint. The ability to identify different paths to the same outcome, of course, is a point that Ragin (1987) emphasizes as one of the key strengths of case-oriented comparative research.

The third book on the list that offers some cross-regional comparisons is O'Donnell, Schmitter, and Whitehead (1986), specifically in O'Donnell and Schmitter's concluding volume, and in Whitehead's essay on international aspects. O'Donnell and Schmitter

note that structures of representation and more-or-less democratic decision-making were less thoroughly destroyed in Southern Europe than in South America (with the exception of Brazil), despite much longer periods of authoritarian rule. They attribute this at least in part to the greater resilience of civil society in Southern Europe. They also note that in Italy, Portugal, and Spain (at least by the 1960s and 1970s) the regimes were less militarized and repression less directly associated with military officers than in South America. The combination of these factors presented fewer difficulties for the transition, both because there was less military resistance due to fear of prosecution and because fewer institutions had to be invented from scratch in Southern Europe than in South America. Whitehead adds to this the much more favorable context in terms of external influences in Southern Europe. He draws a clear contrast between the consistent and impersonal pro-democratic pressures being exerted by Western European governments and the European Economic Community and the more arbitrary and inconsistent pro-democratic policies of the U.S. government. In the European case, the primary motive was consolidation of democracy in the region, and promotion of democracy elsewhere was seen as a related endeavor. In the U.S. case, promotion of democracy was a secondary goal to protection and extension of international hegemony, and during the Cold War the fight against communism often entailed a weakening of pro-democratic forces on the left and alliances with right-wing forces of dubious democratic commitment.

These comparisons firmly put two important variables on the agenda

that have proved important in the subsequent study of transitions and of the new democratic regimes: pre-authoritarian and authoritarian regime legacies on the one hand, and external influences on the other hand. Had these scholars studied either South American cases or Southern European cases exclusively, they would have held much of this variation constant, certainly with regard to the international context. Though most scholars continue to agree that democratization cannot be imposed from the outside and that the crucial determinants of democratic development are internal social and political forces, external incentives can clearly influence the internal balance of forces. Hardly anybody would deny the importance of the prospect of EU membership for the process of democratization in Eastern Europe (e.g. Vachudova 2001).

The fourth book on the list that engages in systematic cross-regional comparison is Rueschemeyer, Stephens and Stephens (1992). They show how different regional patterns of development shaped class structures and, thus, the balance of pro- and anti-democratic forces, and how external influences shaped the balance between state and society and thus the chances for democratization. In South America, expansion of the export economy led to the growth of urbanization and the state before industrialization, and thus to a growth of the middle classes and their organizations at an earlier point and to a greater degree than the working class and its organizations, if compared to Europe. Subsequent import substitution industrialization entailed the use of imported technology, so the working class never grew to account for as large a proportion of the population as in Europe. As a result, the middle

classes were thrust into the leading role in the struggle for democratization, but their commitment to democracy with universal suffrage remained contingent, likely to be abandoned in the face of lower class pressures for radical reforms. Pro-democratic alliances of the middle and working classes, and of small farmers and the working classes were weaker than in Europe or non-existent. Not only differences in class structure but also in the social construction of class interests were responsible for the lesser strength of pro-democratic alliances. These differences in turn were related to the different timing of industrialization and political incorporation of the working class in Europe and Latin America, which opened the way for political learning on the part of state elites. Mobilization of the working class by personalistic leaders and state incorporation into corporatist structures were Latin American elite responses to the perceived political potential of labor movements and much-preferred alternatives to the social democratic-led labor movements of Europe. The combination of all of these factors kept democracy weaker and less stable in South America than in Europe.

Like Whitehead, Rueschemeyer, Stephens and Stephens find the international system in the period after World War II to have had systematically different impacts on chances for democracy in Western and Southern Europe from Latin America. External support for the repressive apparatus of the state in Latin America, in the form of U.S. military aid, increased the autonomy of the military from civilian authorities and the military's capacity to repress pro-democratic forces or overthrow democratically elected governments. Subsequent research has confirmed the impor-

tance of the size of the military for the problems of democracy in Latin America (Bowman 2002). What Rueschemeyer, Stephens and Stephens show, then, is that the same key variables shaped the processes of democratization in Europe and Latin America, but that these variables assumed quite different values and interacted in different ways across the cases and regions. Thus, the cross-regional variations generated elaborations of the original theoretical framework that had been built mostly on work on Europe. However, by the same token, the fact that cross-regional variations were systematic and could be explained within the same basic theoretical framework strengthened the confidence in the explanatory power of this framework.

It is precisely in the area of the comparative study of democratization that the issue of usefulness of cross-regional comparisons has been debated most widely. Roughly a decade ago, a spirited exchange about the usefulness of comparing cases from different regions, specifically Southern Europe, Latin America, Eastern Europe, and the former Soviet Union emerged. The key issue in the debate was whether comparativists should use the same concepts and analytical frameworks to analyze these regions, in search of broad generalizations, or whether initial conditions and the processes of transformation themselves were so different that new concepts and analytical frameworks were called for. Schmitter and Karl (1994) persuasively argued that much was to be gained from such comparisons. Bunce (1995) took issue with many of their arguments but ultimately agreed that - with proper precautions - cross-regional comparisons could indeed be fruitful, and

she later wrote an article on comparative democratization, proving the point (Bunce 2003).

Linz and Stepan (1996) provide a particularly rich systematic comparative analysis of democratization in Southern Europe, South America, and

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“It is precisely in the area of the comparative study of democratization that the issue of usefulness of cross-regional comparisons has been debated most widely.”

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post-communist Europe. They demonstrate the importance of differences in the theoretically well-established variables of regime legacies and external context, and they add differences in institutions chosen (parliamentary or semi-presidential versus presidential systems) and in sequencing of economic and political reforms.<sup>2</sup> All four variables were comparatively favorable in Southern Europe, which accounts for the comparatively rapid and firm institutionalization of the democratic regimes. With regard to sequencing, the political transition came first, closely followed by the expansion of social welfare policies and only later by structural economic reforms.

In South America, the starting point of hierarchical military regimes, the absence of the incentive of EU membership, the absence of NATO membership and thus of the provision of external orientation and valuation of

the military, the continuity of presidential systems, and the simultaneity of economic and political transformations all combined to make the transition difficult and obstruct the firm institutionalization of democratic structures and processes. In the post-communist cases, they stress a difference between East Central Europe and the member countries of the former Soviet Union. In most of the latter, the difficulties have been compounded by a low degree of stateness and by the priority given to economic change, specifically privatization, over political reforms. The former have more consolidated states and had shorter experiences of communist rule and thus on average more favorable legacies for democratization, and they have the incentive of EU membership to stay on a democratic reform course, despite the difficulties of having to carry out economic and political reforms at the same time. Thus, Linz and Stepan also demonstrate the explanatory value of their basic theoretical framework; the same key variables shape democratization across regions, but they assume different values and configurations and thus result in different outcomes that can be explained by the theory.

To move away from the comparative study of democratization, we can look at the use of cross-regional comparisons in McAdam, Tarrow, and Tilly's (2001) study of contentious politics. They present a different version of cross-regional comparison from the one outlined above and used by the other authors discussed here. They do use several cases from different regions and engage in systematic comparisons, but they do not seek to establish regional patterns. Rather, they seek to establish maximum variation in cultural context, state capacity, and democracy. Their goal is to

discover similar mechanisms and processes in different forms of contentious politics, such as social movements, revolutions, strike waves, nationalism, and democratization, in widely different contexts. They analyze two sets of six cases from different regions in matched pair comparisons; one set of pair comparisons is used to identify similar mechanisms and processes in dissimilar varieties of contentious politics, a second set to identify such similarities in episodes belonging to the same conventional variety of contentious politics but having widely varying trajectories and outcomes.

They find that indeed in all these cases three crucial processes evolve in roughly the same form; constitution of new political actors and identities, polarization of political groups, and scale shift from local to translocal arenas of political contention. These processes, in turn, are comprised of the same linked mechanisms. Social appropriation, innovative action, certification/decertification, and category formation shape the constitution of new political actors. Opportunity/threat spirals, competition, category formation, and brokerage interact to produce polarization. Scale shift can follow two different paths, the diffusion/emulation or the brokerage/coalition formation pathway, both leading through the mechanisms of attribution of similarity. McAdam, Tarrow, and Tilly's ability to discover these similarities in these widely different contexts greatly strengthens the confidence in the theoretical explanatory power of their postulated mechanisms and processes.

Another area in which cross-regional comparisons have led to major progress in our understanding is

strategies of economic development. Gereffi and Wyman's (1989) comparative study of Latin American and East Asian development strategies provided a much-needed corrective to simplistic analyses of the failure of the import substitution (ISI) strategy in Latin America. Such analyses had equated the failure of ISI with excessive state interventionism and advocated abandoning ISI along with state intervention, pointing to the success of the presumed free market model in East Asia. Gereffi and Wyman's study demonstrated that East Asian countries, specifically Korea and Taiwan, also pursued a decidedly state interventionist ISI model, but that the crucial differences were the pattern of intervention and the accompanying export base, as well as the international context. Governments in the East Asian countries had no raw materials to speak of and thus were constrained to build a manufacturing base for export earnings. Accordingly, they gradually phased out some of the protection they provided to manufacturing industry once a branch had become established and forced and helped companies to enter export markets. Governments in Latin America, in contrast, continued to rely on raw materials for their export earnings and extended protection for manufacturing industries indefinitely. Thus, the cross-regional comparison challenged the simplistic theoretical framework by forcefully drawing attention to a new variable, the nature of the export base, and by emphasizing the type rather than the degree of state intervention. The comparison further highlighted the importance of the geopolitical and international economic constraints and opportunities that were important for the developmental trajectories of the two sets of countries.

Another set of insights derived from cross-regional comparisons, which illustrates how regional patterns can constitute different paths to the same outcome, concerns determinants of social development. McGuire's (2001) comparison of the development of mortality decline in East Asia and Latin America demonstrates that the countries where these rates were lowered significantly showed different patterns of economic growth, poverty, inequality, and public health policy. In South Korea and Taiwan, rapid economic growth and comparatively low poverty and inequality made it possible for those at the bottom of the income distribution to purchase health care for themselves and their children, and infant mortality was lowered despite a minimal investment in public health care systems. In Costa Rica and Chile, in contrast, slow economic growth and comparatively high inequality kept large sectors of the population poor and unable to purchase health care, but in the 1960s and 1970s these countries invested in public health care systems accessible to the poor, which provided an alternative path to the reduction of infant mortality.

To conclude, then, let me reiterate the importance of cross-regional comparative analyses and the need for the inclusion of this particular research design in methodological writings on comparative social science. As the discussion should have made abundantly clear, I agree that we should not employ region as a variable in its own right but can and should follow Przeworski and Teune's (1970) prescription to replace names with variables. To speak in the language of statistical analysis, we should not use dummy variables for regions, but rather specify the variables that characterize regional patterns. In the

works discussed above, these variables include patterns of state formation, degree of stateness, regime legacies, role of the military, timing and sequencing of economic development, class structures, external economic and political influences, sequencing of reform efforts, and more. Differences across regions in values and configurations of these variables have gone a long way in expanding our knowledge about democratization, contention, economic development, and social outcomes.

### Notes

\* I would like to thank Indira Palacios for research assistance.

<sup>1</sup> It goes without saying that these brief reflections cannot provide a "state of the art" comprehensive review of all works that have done cross-regional comparisons and have done them excellently. I am constrained to select just a few additional and pioneering works to highlight findings on topics that are of great scholarly and/or practical interest.

<sup>2</sup> Differences in sequencing were also mentioned by Schmitter and Karl (1994).

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## The Relevance of Comparative Politics for Public Life

### Introduction

Engagement with public life was a salient theme within the discipline throughout 2002-2003. It was the theme of the 2002 Annual Meeting of the APSA, and the APSA has launched a new journal, *Perspectives on Politics*, that gives it prominence. As it has evolved thus far, however, the debate has been dominated by the relevance of American politics and international relations for public life. It is our view that greater attention must be paid to the relevance of comparative politics and to the special issues investigated by our subfield. No other subfield of political science, we believe, has nearly as much to say about state-building, truth commissions, religious fundamentalism, ethnic conflict and violence, constitutional design, or judicial and legislative reform. No other subfield can put social and economic policies in a comparative perspective, showing U.S. policymakers and journalists how our achievements and failures compare to those of other countries and, thus, enriching the pool of policy options from which they draw.

Although comparativists possess relevant expertise, we tend to be confronted with ethical concerns, professional tradeoffs, and practical obstacles that complicate our involvement in and engagement with public affairs. These include:

1. Our desire to have an impact on public life is sometimes in tension with the goal of building a "scientific," objective, unbiased social science. Does public relevance threaten our scientific aims? Must comparativists necessarily be cautious about sharing their expertise with governments,

political parties, or groups that may have partisan or particularistic political agendas?

2. There is a tendency within our profession to reward research that is technical or formalistic and therefore not obviously or immediately relevant to the practitioners and political leaders who might benefit from it. Can this gap be bridged? If so, how?

3. American journalists, legislative staffers, nongovernmental activists, political leaders, and the mass public are already overloaded with less than rigorous information about foreign countries and governments. How can we persuade a nonacademic, non-specialist audience to listen to what we have to say? Who beyond our narrow specialties is interested in our contributions? Who should be interested?

4. Part of our impact on public life is indirect, through our teaching and mentoring of students. Are we giving them the knowledge and tools that they will need to become more insightful journalists and staffers, effective activists, successful leaders, and better citizens? How might we do better?

The following symposium features the reflections of four distinguished comparativists who have had practical experience in public affairs: Andreas Schedler, Susan Woodward, Terry Lynn Karl and Brendan O'Leary. Our contributors were asked to address one or more of the above themes. We hope you will find their responses stimulating.

### Comparative Politics as a Resource

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How relevant is our comparative knowledge of the political world? Does it ever feed back into our sphere of study? Do politicians and citizens care to know what we know about them? And if so, what for and to what effect?

Concerns about the practical relevance of social science are as old as social science itself. As an institutional sphere of specialized knowledge, social science is shut off from pressures of practical utility at the same time that it confronts perennial demands of social relevance. The tension between institutional insulation and practical relevance is insoluble, except at the price of either abandoning scholarly autonomy or retreating into scientific autism. Any reflection about the relevance of comparative politics in public life has to recognize a structural paradox: comparative politics can be practically relevant only as long as it preserves its distance from practical imperatives.

The following glances from the ivory tower (a walled colonial house in rural Mexico) revolve around five short propositions. The first three concern the practical relevance of our explanations, our factual knowledge, and our conceptual tools. The remaining two address the methodological as

well as political problems that may arise from our eventual practical relevance.

### Explaining Politics

*What makes comparative politics relevant is less the provision of secure knowledge than the introduction of doubt.*

Both policymakers and grantmakers often demand political science to be useful. They want practical advice, they want to know what works, and what doesn't, to achieve their objectives. How well are students of comparative politics equipped to deliver knowledge of immediate practical application in policymaking and institutional design? How useful are our findings in terms of instrumental rationality, of identifying appropriate means for the achievement of given ends? I would say that comparative politics is almost never useful in this narrow sense. We are not meant to be the technical servicemen of Machiavellian princes.

We are unable (or, at the very least, should be hesitant) to issue straightforward practical recommendations since the causal knowledge we produce is usually less than straightforward. Most of our explanations are complex and context-sensitive. Rather than stipulating universal relations of linear causation we tend to formulate probabilistic explanations of restricted scope. We tend to find X does not always lead to Y. Whether it does or not depends on given configurations of variables, and often worse: it depends on the strategic interaction between actors who have to cope with uncertainty, time pressures, normative dilemmas, and conflicting criteria of rationality.

The conditional nature of the causal knowledge we produce conditions the nature of the policy advice we can give. Laypersons are used to thinking of science as a fountain of secure knowledge. Comparative politics, as perhaps the social sciences in general, cannot meet that illusionary expectation. If our causal findings come to be relevant for policymakers, it is less through the provision of instrumental certainties than through the introduction of doubt.

More important than our certainties are our uncertainties, more relevant than our tentative areas of knowledge are our vast fields of ignorance. Comparative politics, rather than providing secure foundations for political decision making, reveals its uncertain grounds. It works much better in unsettling common assumptions than in establishing common sense. Its role is not to resolve political debates, but to get them rolling, not to make public decisions easy, but to make them hard.

Take, for example, the dilemma of opposition parties in electoral autocracies. When authoritarian rulers hold flawed elections to legitimate their perpetuation in power, opposition actors have to decide whether to enter their manipulative game or to boo at the fences. What should they do? What can students of comparative politics tell them? We can tell them (without telling them anything new) that their dilemma is a common one as well as a real one. We can explain the potential costs of both abstention and participation, which are high and uncertain. We can also tell them that in making up their mind about participating or boycotting they should consider their own popularity, which, again, is uncertain. Authoritarian regimes often enjoy a

certain degree of popular legitimacy while, in any case, we never know for sure what people think in the absence of fear and credible opinion polls. The best we can give them are honest declarations of structural ignorance and informed estimates of uncertainty. Of course, comparative politics may generate productive irritations, rather than illusionary certainties, only if political actors are open to doubt. This implies, for instance, that under the current U.S. administration, comparative politics is condemned to irrelevance. President George W. Bush as well as key figures in his cabinet are reported to be persons free of doubt. Yet if you do not know doubt, you will not want to know about comparative politics.

### Describing Politics

*What renders comparative politics relevant are less our causal arguments than our descriptive inferences.*

Political science, at least in the U.S. tradition, privileges explanation over description. We are supposed to explain political phenomena, not just describe them. Description seems an honorable enterprise only as a prelude to explanation. The internal devaluation of description by its academic producers, however, stands in remarkable contrast to its external valuation by its non-academic consumers. It seems, at least in the sphere of comparative politics, that the public demand for descriptive knowledge exceeds the public demand for causal arguments.

In most countries, the subdiscipline of comparative politics does not possess a methodological core. It is circumscribed by context-dependent substantive boundaries: What is called "comparative politics" is the study of



politics in foreign countries. Often its purpose is fundamentally descriptive. It wants to know how politics is similar or different in other places. The consumers of comparative politics, politicians as well as citizens, often have similar factual interests. They wish to learn about other countries in order to know how their own country fares by comparison.

Over the past two decades or so, students of comparative politics - working hand in hand with international organizations, national governments, and non-governmental organizations - have been complementing the traditional socio-economic datasets provided by the IMF, the World Bank, and other UN organizations with comprehensive political datasets. Among the best known are the Freedom House annual reports on political rights and civil liberties, the Corruption Perception Index published by Transparency International (TI), and the regional public opinion Barometers.

It is not that shoe shiners and taxi drivers spend their days debating Afrobarometer, Transparency International, and Freedom House data - although they may occasionally do so, as Fredrik Galtung and Jeremy Pope (1999: 275) describe with reference to the TI index. Still, those thin quantifications of comparative knowledge sometimes achieve remarkable salience in domestic debates among political elites. Their comparative scales do not explain nor resolve any political problems; they define political problems. They allow people to appreciate the relative magnitude of national failures or achievements. They allow citizens to see how democratic, how corrupt, how civic, etc. their country looks in comparison to other countries, be they similar or dif-

ferent, close or distant.

Politicians and citizens also look to foreign experiences to widen their repertoire of action. When examining comparative experiences, they know they lack the comfort of scientific certainties. They know they have to rely on their own local knowledge and political judgment if they decide to adopt foreign ideas and translate them into local realities. But they wish to learn: What have others done to cope with problems that look similar to ours? They study comparative politics to learn what others did to combat poverty, improve higher education, control corruption, reduce environmental damages, and so forth. For instance, the recent IDB volume on reform initiatives in Latin America (Payne et al. 2002) is basically descriptive in purpose. Responding to a pervasive "lack of reliable information" it wants to take stock of the multifaceted institutional reforms the region's democracies have implemented over the past decade. Its explicit goal is "not to break new theoretical ground or to test hypotheses" but to lay out "a map of reform options available" (p. 2).

### Framing Politics

*Political actors often do not apply comparative politics; they talk comparative politics. Overlooking the pictures we draw, they use our language and conceptual frames.*

The relevance of comparative politics for public life does not lie primarily in policy advice. Its main role is to nurture public debate. We should not underestimate the "nutritional value" our descriptive and causal inferences may possess. Yet the basic foodstuff comparative political scientists feed into the public arena is our conceptu-

al offerings. The language of comparative politics provides political actors with conceptual tools and analytical frames to comprehend themselves and their adversaries, their problems and their solutions. The facts of political life do not speak for themselves. Comparative politics offers conceptual tools that enable them to speak.

The language we use, and the concepts we choose, are not innocent. Concepts are crucial pieces in the game of political argumentation. Different concepts carry different inferential roles. They differ in the empirical, normative, and practical claims and commitments they involve. It is not accidental that political struggles often are conceptual struggles. For example, it makes "a difference that makes a difference" (Gregory Bateson) whether we think of vote buying as electoral clientelism, electoral corruption, or electoral fraud; whether we describe acts of political violence as terrorism, war, or organized crime; or whether we conceive processes of regime change as modernization, revolution, or transition. And so forth.

Like it or not, comparative politics forms part of political struggles over conceptual choices. In a very literal way, the discipline may even come to set the terms of political debate. Take the idea of democratic transition. The so-called third wave of democratization has led to a remarkable global extension of electoral democracy. But even more, it has brought the global diffusion of a certain language to conceptualize democracy and democratization. Contemporary democratization literature is anchored in the concepts of transition and consolidation. The notion of democratic transition differs from our earlier vocabulary of political change and crisis, of modernization

and revolution. It involves a couple of strong empirical as well as normative assumptions that guide political diagnoses as well as recipes. It contains the modest idea of change limited to the level of regimes; the instrumental idea of change brought about by non-violent means; and the teleological idea of change progressing from a known point of departure (authoritarianism) to a known point of arrival (democracy). These assumptions do not provide celestial certainties, but they do circumscribe political hopes and strategies, agendas of institutional reform, as well as permissible practices of contestation and participation.

All over the world, political actors have adopted and adapted the scholarly language of regime transitions. They have fought ardent debates over the starting and endpoints of democratic transitions, over whether their respective countries are already in transition or still in transition to democracy. The strong inferential implications the concept of democratic transition bears explain the importance and passionate imprint those debates have acquired in places like Chile and Mexico. If a country describes itself as being in the middle of a transition, it makes a declaration of faith: it reads the changes and uncertainties it faces as directed towards the goal of democracy. If a country declares its democratic transition to be over, it makes a strong empirical claim: it declares democracy not to be an aspiration anymore, but an achievement. Either description involves different ideas about the nature of politics. During transitions, actors struggle to redefine the rule of the political game. Afterwards, they are supposed to accept given rules and pursue their goals within their boundaries.

### Out of Control

**For political actors, comparative politics is a resource. Once our work enters the sphere of politics, we do not control the conditions of its use.**

Debates about the practical relevance of political science often pose a potential trade-off between methodological rigor and practical relevance. The more specialized our languages (jargon and mathematics) the less outsiders will understand us and the more we will have to invest in translation. Of course, there is nothing like a linear tradeoff between the rigor and relevance. All combinations are possible. We have seen rigorous research that remains confined to the ivory tower as well as lofty work that makes a big splash in the public sphere. Of course, our professional ethic excludes the latter. If we care about public relevance, we would not want to be relevant without being rigorous. Yet we have to accept one complication and one sad fact.

The complication lies in the fact that our practical relevance may change our notions of rigor. To the extent that our concepts and findings influence the public sphere, the products of our research alter our objects of research. By being relevant we "contaminate" our data sets. Sophisticated conceptions of methodological rigor have to be attentive to such interaction effects. Politics does not take place in a self-contained sphere of aseptic isolation. It is not in a hermetic experimental cage that political actors think, speak, and act. They may change their ways of thinking, speaking, and acting through the consumption of comparative work. The relevance of comparative politics may alter its units of research and their behavior. It may destroy the methodological core pre-

sumption that sustains experimental designs, namely, the strict separation between the scientist and her objects of research.

The somewhat depressing aspect resides in the fact that methodological rigor is no insurance against political misuse. For political actors, comparative politics is a resource they exploit for their own purposes, according to their own criteria. We, the producers of comparative politics, have no control over its application. Nothing we say is safe from distortion and misunderstanding, nor is it immune to political abuse and manipulation. There is no way we could prevent, say, authoritarians from applying Duverger's law as effectively as democrats do. That's the frequent irony of our public relevance. It is always an aspiration we harbor, but the moment we achieve it is often a moment of suffering, of personal alarm and affliction.

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## Crossing the Line

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The decade of the 1990s was a heady period for many comparativists, whether budding or fully matured. Opportunities to engage in real issues of the day, both in policy circles and abroad, were nearly endless, while the political and ethical risks that had so poisoned the profession in the 1960s appeared low. The Cold-War wraps were off, specialists in strategy could be sidelined just enough to give room to specialists in the domestic politics of states, and there was an explosion of democracy promotion programs, complex humanitarian emergencies, major-power attention to civil wars, United Nations peace-keeping and peacebuilding missions, newly democratizing states seeking advice, and policy makers asking for lessons learned.

The signs of climate change under current Administration policy thus make this symposium particularly timely for taking stock of the lessons some of us have drawn about the current relation between knowledge and power. My own sobering experience suggests that the dilemmas of engagement are not, as usually portrayed, an issue of "political will" - do we wish to speak to them and do they wish to listen to us? The question is whether as comparativists we have anything to offer.

The basic assumption underlying the question of relevance is that policy

would be better if it were better informed. Throughout the 1990s, for example, I was asked repeatedly why it was that Washington did not listen to me and other Yugoslav experts, on the assumption that policy would be different if Washington took advantage of the national resource of area experts it had created during the Cold War. While Americans bemoan the anti-intellectualism in their culture and particularly in government, colleagues throughout eastern Europe and parts of Asia and the Middle East were clamoring for advice on think-tanks, on the assumption that if they had them, too, policy would be better.

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### "Providing knowledge is not the same as having influence"

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The facts suggest otherwise. The literature in comparative politics is to a remarkable extent generated by real-world problems. The obsession in the 1990s with ethnic conflict and with consociationalism show how far this can go. Very large numbers of comparativists engage at one time or another, many regularly, in transmitting knowledge (in policy briefs, congressional testimony, op-eds and media interviews, consultancies with the World Bank, development agencies, or the UN, and seminars for State, the Pentagon, the CIA, and training academies for diplomats or uniformed officers). Most of this, by the way, is by invitation -- from senior officials, elected politicians, military officers, or journalists whose professional stakes generate huge incentives to seek our advice, making them far more eager students than most

undergraduates.

Providing knowledge is not the same, however, as having influence. If one believes that comparativists have something to say to policy makers and that sharing that knowledge will indeed improve policy, peoples' lives, or the world we live in, then one must be prepared to play by the rules of that world. We may well understand, as analysts, what it takes to have influence and how public life differs from scholarly life, but it is quite another thing to participate in it. Can one, pace Weber, be a scientist and a politician at the same time?

### Having Influence: Three Issues

My own experience highlights three issues in particular.

**(1)** Having influence means choosing one's time horizon and then accepting the consequences of that choice.

Does one want influence over issues of some immediacy, ones that propel one to act, or is one willing to do the "strong and slow boring of hard boards"<sup>1</sup> to change the agenda itself?

While the longer horizon appears less engaged and political, in the sense that it remains research-oriented and analytical, it actually requires far more commitment of time and focus, a recasting in fact of one's entire scholarship toward policy relevance and political strategy. It may even require an activist period to mobilize support.

Focus on the headlines of the day, most often in response to an invitation, by contrast, means working within an agenda and framework that has likely already been set by others. As any such frame is set by an intense political process, it also means entering a world where interested parties

are already mobilized and stakes are often very high. Should one be in the unfortunate intellectual position of finding that particular framing either factually wrong or dangerous in its policy implications, then one may well have to choose between one's integrity as a scholar and being ignored. Critics are not welcome in policy circles.

**(2)** The standards of evidence and inference in social science do not prepare oneself, moreover, for the competitive - and ruthless -- nature of public life. There are many sources of knowledge competing to be heard. Setting aside for the moment the cut-throat tactics and personalized stratagems that characterize a world on short-term contracts, political crusades, vulnerable promotion ladders, or deferential hierarchies, it is unclear how one can, as a scholar, claim greater authority than those who have substantial field knowledge as desk officers, hardened humanitarian workers, intelligence analysts, and seasoned diplomats or military officers.

Competition is not only individual but organizational, both in governments and in international operations. The more intense the competition between government agencies, national contingents, or international organizations for influence or scarce funds, the more the respective bureaucracies pursue power through social closure - favoring insiders who know "the way things work," "how things are done," and specialized jargon. One must in any case follow the SOPs (standard operating procedures) and customary norms of that organization, which requires special knowledge of its own (usually experiential, which takes time). If the implementation of one's policy recommendation requires a change in the way

things are done, one must be prepared, at the least, to deal with the threat one represents to that organization or staff. National origin also matters substantially -- for military units, NGOs, senior officials -- and the ease with which it generates loyalties, suspicions, and resources in

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### "Critics are not welcome in policy circles"

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this competition.

The most difficult competition, however, is that with one's own comparative colleagues. We do not ourselves agree on the most fundamental points of knowledge relevant to particular issues. Aside from the palpable frustration this causes policy makers and its effect on reducing our influence with some, confidence levels for different theories and interpretations are established by quite different criteria in public life than their presentation as academic knowledge would lead one to expect.

**(3)** Timing is everything when it comes to influence in public life. While this is also true in political science, the role played by time is fundamentally different and in basic confrontation with the world of scholarship. The two-page memo must convey all that is necessary, whether as analysis or prescription and its justification. All complexity must be reduced to a "bottom line." In public debate, only sound bites get reported or aired. Working within a policy or operational environment means running from one meeting or task to another, with little time to think, and frequently, insufficient sleep (with predictable consequences for quality).

Above all, there is the requirement of speed. When knowledge is sought, there is no time for research or reflection; it has to be ready then, that moment, that hour, that day. Increasingly, moreover, one's enemy is the "real time" transmission capacity of high-tech mass media. Facts take longer to emerge, and in the meantime a new reality or frame has been created, to which one inevitably has to adapt one's own analysis or advice.

**(4)** Packaging also matters greatly to influence. This may be the greatest obstacle to the relevance of comparative politics in public life. The fact that it has long been recognized but only minimally addressed by our field reinforces how difficult this problem may be.

On the one hand, relevance requires special knowledge in addition to the expertise of one's field. For example, policy assessments require deep contextual knowledge, not just credible inferences from theory. Ethical considerations alone should limit our tendency to reach for analogies, in theories or cases, when we do not know. Analysis also requires a credible theory of dynamics, for one has to apply one's knowledge in an environment that is changing rapidly, often highly uncertain and indeterminate, and a context where rhetorical frames and policy "goal posts" fluctuate beyond one's control. Any assessment also requires political sensitivity to the risks of decision-makers in capitals.

On the other hand, relevance requires willingness to accept imposed constraints on what one is willing to say and how. Diplomats think in terms of "leverage," that is, in terms only of those policy instruments they think

political traffic can bear, regardless of their effectiveness in relation to alternatives. They tend to seek greater leverage through psychological factors, desiring knowledge of the personalities of political actors with whom they will interact or of the likely popular reaction to some choice of policy. Even if they are open to alternatives, their instructions will make them autistic. They particularly do not want to be told that the external environment, such as the actions of major powers or the international economy, is implicated in a conflict, let alone that they might bear some responsibility and require changing behavior, too. Structural arguments, which comparativists make so well, are of no use at all unless they can be translated into short-term behavioral implications. Knowledge that contradicts conventional wisdom or dominant frameworks (however much they enjoy the intellectual exchange) is

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**“The most difficult  
competition is with one’s  
own colleagues”**

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most likely to be dismissed - and the advisor with it - as an existential threat or politically incorrect. Statistical inferences, too, must be stated in terms of scenarios with attached risks.

### **The Relevance of Comparative Politics to Relevance**

The relevance of comparative politics to public life thus depends, in my view, on how far we wish to tailor our own enterprise - the questions we research, the packaging we provide, the time horizon we accept, our

mechanisms of accountability for knowledge employed - to that goal. Crossing the line back to comparative politics from a policy experience is also difficult, judging by the articles I read by people in that transition. Being a scientist if one's goal is policy relevance is also not automatic. And the profession as a whole is no more receptive to such persons and perspectives than it has ever been.

Having returned for the freedom our world provides - on questions, frames, and time -- I have a wish list for increasing our relevance.

**(1)** First, our comparative advantage would improve greatly if we did serious research on the politics of governmental and international organizations. How do organizations adopt policy advice or learn and apply their own lessons? Why do we have so few mainstream studies of implementation? There is a yawning gap in the policy world, moreover, of systematic analysis of the actual effects of policies, aid programs, and other attempts at foreign influence. The political reasons are obvious. Being outsiders with methodological tools, we are well-suited to fill that vacuum. Greater efforts to communicate regularly with practitioners would also improve our relevance by making us more sensitive to operational and political constraints, the knowledge they consider lacking, and ways to package results more effectively.

**(2)** Second, we would also benefit in many ways by reaching out more systematically to colleagues outside the US and to knowledge generated elsewhere. Our own analyses increasingly emphasize international context and national variation, yet it seems to me that we remain surprisingly insular - others might say imperialistic - as a

field. Although being an American in a non-American policy context or international mission will continue to be problematic, our approach to knowledge and bibliography need not be so vulnerable.

**(3)** Third, we could dedicate attention to packaging. Our methodological standards for policy inference are woefully underdeveloped. Refereed journals in the profession could establish sections where policy inferences are debated, where scenarios and their probable consequences are analyzed, and where comparisons of alternative policy paths are presented. Policy makers frequently ask for an up-to-date summary of the literature on a particular issue. Such a resource should be quite easy to devise.

Whether regular opportunities to publicize such knowledge would overcome the problem of time is not clear. The striking lack of any sense of urgency in the academy when events require immediate action can be profoundly disturbing when one is in a policy environment. By having packages ready (and continually debated as knowledge evolves) in a transparent, public form, however, we might be able to get beyond the complacency, a polite indifference, that was my frequent experience when pleading for "state-of-our-knowledge" on X (such as lessons from the literature on democratic transition to give inquiring opposition activists, knowledge that might help devise immediate policy interventions to interrupt a situation of spiraling violence, or analyses of the real-world consequences of engineering different electoral systems, fiscal reforms, or property rights in land).

**(4)** Fourth, we can improve our stan-

dards of accountability in public life. Protection against discrimination within the academy, where frames set in public life can have immediate and harmful relevance to comparative politics, ranks rightly alongside issues of accountability of public officials, to electorates, courts, and, now, international norms.

But how much do we discuss professional accountability for our ideas in public life? Mary Anderson has had much influence in the aid and humanitarian communities with her suggested standard that aid "do no harm." The precautionary principle in environmental policy is spreading to other policy areas in the European Union. If members of our profession use their scholarly credentials to legitimize advice that proves harmful or was even politically or ideologically motivated from the start, do we have a parallel standard and means for enforcing it? It is here that the professional resistance to policy relevant research and activity could be seen as irresponsible. Our knowledge is used, by us and others. In my view we need to devise mechanisms of accountability for its use - not toward the persons who propagate them, which would be too vulnerable to abuse, but on the ideas and the policy inferences themselves.

### Notes

<sup>1</sup> Max Weber, "Politics as a Vocation" (1918) in *From Max Weber: Essays in Sociology*, translated and edited by Hans Gerth and C. Wright Mills (New York: Oxford University Press, 1946): 128.

## Not on Your Vita

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*Caring about what is happening in the world won't get you tenure. We are a theoretical department. If you get involved in public life, make sure it is not on your vita.*

*Advice from a senior colleague in my pre-tenure days.*

The days and nights waiting for a South Florida federal jury to return a verdict in Romagoza et al. versus García et al.-- the trial of two former Ministers of Defense from El Salvador living in the United States -- were some of the longest I have ever spent. The June 2002 case against Generals José Guillermo García and Carlos Eugenio Vides Casanova marked the first time in United States history that a jury would rule about the role of former high-level military officers, who personally appeared to defend themselves against claims that they had failed in their responsibility as commanders to prevent egregious human rights violations against their own people.<sup>1</sup> Much was at stake. Politically, a verdict in favor of the plaintiffs would be a clear statement of accountability for the more than 70,000 murders and countless incidences of torture and rape of civilians in El Salvador's state repression and subsequent civil war. A favorable judgment would be a significant step towards ending the impunity of human rights abusers. Legally, it would have far-reaching implications

for firmly anchoring the doctrine of "command responsibility" in U.S. law and establishing a precedent that murderers and torturers could not safely retire to the United States. Morally, it would help to vindicate the thousands of Salvadorans who never had the opportunity to seek justice, and it would bring some degree of solace to Neris González, Carlos Mauricio, and Juan Romagoza, respectively the eight-months-pregnant church worker, professor and doctor, who were raped, beaten, tortured and forced to witness the torture and murder of others.

My role as expert witness in the trial was to interpret the outbreak of civil war in El Salvador and then argue several key elements of command responsibility: that the military and security forces were the main perpetrators of murder and torture; that Ministers of Defense García and Vides Casanova knew (or should have known) about the conduct of their troops and had the capacity as top commanders to curb these violations; and that these generals failed to take all reasonable measures to prevent their subordinates from committing human rights abuses or punish them if they had done so. This testimony was crucial to the outcome of the trial. While no one who had met the plaintiffs doubted that a jury would believe their stories, direct testimony alone would not be enough to return a positive verdict. Under the legal theory of command responsibility, it was imperative to connect the torture of these three ordinary Salvadorans directly through a chain of command to two of the most powerful people in the country at the time -- ruling generals who were not present during the actual violations, who had been awarded Legion of Merit medals by the U.S. Secretary of

Defense, and whose attorney portrayed them as "the John Adams and Thomas Jeffersons" of their country. Because the generals had previously been acquitted in the murder trial of four American churchwomen, they seemed very confident that the analysis and information necessary to establish command responsibility could not be known and certainly could not be proven - least of all to citizens in the United States who could not even begin to fathom the history of this tiny country.

But the generals were wrong. On July 23, 2002, as a tense silence reigned in the courtroom, the verdict was read. "Is General José García legally responsible for the torture of Juan Romagoza?" "Yes." And, as spectators, jurors, lawyers, plaintiffs, and even court officials began to cry, the bailiff continued to read: "Is General Eugenio Vides Casanova legally responsible for the torture of Juan Romagoza?" "Yes." A jury of ordinary Florida citizens showed that it had learned about this horrific period of Salvadoran history, grasped the doctrine of command responsibility, and held two generals accountable for their actions and inactions. For the first time, responsibility for El Salvador's reign of terror was laid at the feet of these generals. While this happened in Florida and not in El Salvador, where torturers have yet to be punished, the verdict is one step in ending Latin America's long history of impunity.

Which brings me to the relevance of comparative politics for public life...

Why was a comparativist scholar (granted one with an academic background in Latin America) giving expert testimony in this trial rather than, say, a knowledgeable reporter? Would not

the journalists who trekked through dangerous territory to the massacre site of El Mozote, photographed the bones, interviewed the sole survivor, and later documented the murders of almost 700 people, including 131 children under the age of twelve, be more effective on the witness stand? Why use a comparativist that the defense attorney would surely malign as an "ivory tower egghead" in order to undermine the damage of the testimony itself and enhance the arguments of former U.S. policymakers who would testify on behalf of the generals?<sup>2</sup> What does this choice of witnesses say about what our expertise has to offer the public realm?

Not much, some would argue. There are those who claim that when we "go public," we merely state opinions and "opinion is not the same as knowledge." The latter is "scientific" and "disinterested" scholarship, while the political scientist who writes, speaks or acts in public is just a citizen with an opinion - just like any other citizen (albeit likely to be a bit more educated.)<sup>3</sup> This view is often accompanied by a preference for "scientific" approaches and methodologies: rational choice models or statistical analyses that, of course, do not lie or suffer from intrinsic biases. The flip side of this view is a prejudice, frequently articulated by these "scientists," against much of the work of comparativists, exemplified by the claim that there is little difference between an area specialist and an investigative journalist (a charge I have too often seen as part of tenure reviews). The conceptual framework, operational design, case studies, and thick description resulting from difficult and sometimes dangerous research in the field or painstaking inquiry into historical materials, which is the heart of much of the best com-

parative work, is reduced to "just" good investigative journalism. Hence, those who bring their area-based scholarship to the public realm are, at best, competent reporters or, at worst, opinionated descriptors.

Certainly there are cases of both, but theoretically grounded and empirically rich comparisons, at their best, provide scholars with the best means for formulating general understandings about how political and developmental processes have occurred, and allow us to explore how these same processes may have produced similar or different effects across time and space - a skill that is *not* part of the training of a journalist or accessible to the citizen observer. Comparativists postulate, discover and test for the existence of *types and patterns* in the realm of politics, for example, in transitions to democracy, civil-military relations, authoritarian regimes, state- and nation-building processes, impacts of public policies, and instances of violent conflict. This, in turn, permits us to assess the experiences of particular countries, not only with regard to their uniqueness, but also in terms of the common outcomes that they share with other countries of a similar pattern or type. This sensitivity to what is common or unique across political time and space is what we bring to our engagement in public life, and it is what sets us apart from other experts.

Return for a moment to the trial of the Salvadoran generals. The jury's determination hinged, in no small part, on their interpretation of whether El Salvador in 1979-1983 (the period of worst state repression) was undergoing a transition to democracy or an attempted reversion to hard-line military rule based on state terror. Buffered by the rhetoric of the U.S.

government at the time, the defense claimed that the generals were leading a transition to democracy, that they were being assaulted by authoritarian and uncontrollable forces from both the right and left, and that they were just following the orders of a civilian president anyway. Thus how could they be held responsible for the military's murder and torture, even if they were, respectively, Minister of Defense and head of the National Guard? But this did not prove convincing to a jury presented with specific indicators showing the power and control of the military led by the High Command, types of authoritarian rule that included civil-military juntas, *comparative data* on repression showing that El Salvador far outdistanced even Pinochet's Chile and Argentina during the "dirty war," empirical evidence about *patterns* of state repression and government responses, and *conceptualizations* of civilian control of the military and democratic transition that were not limited to the mere presence of elections. Moreover, the jury learned, comparatively, about *patterns* of deniability - the words and actions authoritarians use to disguise terror and confuse observers over where responsibility for state terror lies. In the end, they were convinced that the generals were not the powerless democrats they portrayed themselves to be, but rather the instigators of state repression and the protectors of torturers, and El Salvador during this early period experienced an attempt at authoritarian reversion, not a transition to democracy. In effect, the middle-range theorizing developed by comparativists and the solid empirical research of a number of Latin Americanist specialists helped to win the day -- not the fallible opinion of a citizen.<sup>4</sup>

Comparative politics does not lack

relevance for public life, as some seem to think but, rather, pervades the world of practitioners. Examples abound - even if we confine the definition of public life to mean direct influence on policymakers. What Thomas Carothers has called "the seminal work of Guillermo O'Donnell and Philippe Schmitter"<sup>5</sup> has shaped the way policymakers understand transitions from authoritarian rule, not only in the United States, but all over the globe - a fact repeatedly attested to by Albanians, Bulgarians, Hungarians, Russians, Czechs, Chileans, Argentineans, Uruguayans, South Africans and others who have paid tribute to the usefulness of "the little green book" for their own transitions. Robert Putnam's *Bowling Alone* and *Making Democracy Work* has helped to put the issue of civil society on the agenda of U.S. policy practitioners and foundations, leading to a renewal in efforts to support non-governmental organizations.

If we broaden the definition of public life to go beyond directly influencing politicians (as we certainly should), the impact is even clearer. Margaret Keck and Kathryn Sikkink in their *Activists Beyond Borders* have taught a new generation of NGOs about the implications of different strategies for change and has helped them see the historical impact of their work. At a time when "lootable assets" such as "blood oil" and "conflict diamonds" dominate the headlines, the arguments by comparativists, including this writer, concerning the detrimental impact of petroleum and mineral wealth on the political and economic institutions of the countries that export them, once greeted as excessively "determinist" and "unscientific" by some political scientists, is now ensconced in the lexicon of practitioners ranging from Oxfam and Catholic

Relief Services to the World Bank and Exxon.

That so many in the political science profession do not see the extent of this impact may be a question of measurement. For some, influence in the public realm is gauged exclusively by their ability to "whisper directly in the ear of power" or by the number and notoriety of articles published in the *New York Review of Books*, *New York Times*, or *New Republic*. However important these forums may be, real clout is often more indirect, subtle, and diffuse, and, therefore, much more difficult to measure. How can the number of journalists whose articles are shaped by the work of comparativists be counted? How does one know when policymakers understand a particular country or political process differently as the result of some scholarly work he or she has read (for example "regime change" in Iraq)? How can we assess the impact of reports produced for Oxfam, Human Rights Watch, or Catholic Relief Services that circulate widely and educate numerous people around the world? And what about the religious leaders who extend their beliefs to include new factors based on the work of comparativists like the recent statement of African Catholic bishops calling for the just use of petroleum revenues, business leaders who call for new codes of corporate responsibility based on some of our findings, or students who change their life plans because of the types of polities or patterns of policy that comparativists have discovered and confirmed? Clearly, this impact cannot be easily measured.

The problem of relevance, in my view, lies not so much with the production of good work in comparative politics



as with its consumption by policymakers. The latter much too frequently embrace this scholarship, not as warnings about what they should not do (which may be what we do best), but rather as recipes for doing what they already plan to do. Policy-makers scour academic writings for theoretical buttresses to support positions they have already taken or to use as ideological coverings to justify what they have already done. This is most evident in Washington's embrace of the work of Samuel Huntington - from *Political Order in Changing Societies* (which was used to justify U.S. support for authoritarian rule during the Cold War) to his *Foreign Affairs* argument for "forced-draft urbanization" in Vietnam (which provided a much-needed modernization rationale for the bombing of peasant communities) to *The Clash of Civilizations* (which buttresses claims that Islam poses an intrinsic threat to the West). Not surprisingly, the academic work that deeply questions dominant assumptions and policies gets no such play in the policy world, a reality that damages the process of political learning and leads to the repetition of mistakes.

This consumption problem becomes even more complicated when the interpretation by practitioners distorts the empirical findings or normative implications of the work in question. Thus, Lipset's thesis in *Political Man* concerning the relationship between economic development and democracy was widely cited in order to rationalize the decision to support transitions to capitalism prior to transitions to democracy in Eastern Europe, the former Soviet Union and Mexico - a policy stance with profound and long-term consequences for both the quality of democracy and the concentration of wealth - neither of which were

intended by Lipset himself. O'Donnell and Schmitter's *Transitions from Authoritarian Rule* has been "read" in order to justify armed intervention and foreign occupation in Iraq - a misreading of their argument and the evidence they present concerning the role of outside powers in democratic transitions.<sup>6</sup>

Another problem lies not with practitioners but within the discipline itself, where the pervasive self-image of American political science as a form

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**“The road towards better social science and more engagement is the same: it lies in the internationalization of American political science”**

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of pure science detached from particular values and ideals biases some scholars against the search for relevance. When combined with structural problems in political science alluded to elsewhere in this forum, this creates the notion that the desire to have an impact on public life must be in tension with the goal of building an objective and unbiased social science, and it therefore is always suspect. But American political science, in the words of Ido Oren, "is a historically and nationally rooted ideology as much as an objective science," with "an enduring undercurrent: *America*."<sup>7</sup> Thus to claim that this discipline is disengaged from the state (after all, where do much of our research monies and consultancies originate?), that our theoretical imagination and selection of topics are not

linked to domestic and foreign policies (not to mention war efforts), that our most important and (allegedly) objective concepts do not reflect and even embody normative visions, that the substance of political science scholarship is not deeply rooted in the politics of the United States, or that the welfare of America is not "the master value of the discipline" is to ignore the history and development of the profession itself.

Theory without empirical research and sensitivity to the practices of politicians will always be flawed, but so will the practice of politics be flawed without the findings and insights that only systematic and sensible comparison can generate. This is not to say that combining public engagement and scholarly inquiry into politics in the same person is not fraught with perils. It is. But intellectually honest and well-trained scholars have tools to help correct this problem. If we are taught (and then teach) the notion of rival hypotheses, and if we actively seek out the information that might prove our least preferable hypotheses right, we can mitigate and confine preconceptions and prejudices.

Take El Salvador once again. Virtually all observers in the 1980s believed that a figure like Roberto D'Aubuisson, the reputed founder of death squads, ruled only from terror and could not have any mass political base. But a rival hypothesis would claim that terror alone could not explain his staying power, that he could (and did) have some sort of mass following. Being open to this possibility in turn produced evidence leading to a different understanding of the peasantry (and eventually a different analysis of the nature of the civil war). Working with biases on the

table in a testable fashion instead of veiled under the rhetoric of objectivity makes it possible to engage in public life and be a social scientist at the same time.

That we are even asking the question about the relevance of comparative politics for public life is an illustration of how American our discipline really is. Scholarship without engagement is a luxury of very rich countries. There are few places in the world that can afford to pursue scholarship for its own sake and for its formal elegance without concern for its impact or relevance.<sup>8</sup> Yet even rich and very dominant countries cannot bear the long-run costs of becoming so scholastic. It is supremely ironic that, at a time of heightened international threats and accelerated globalization with their concomitant need to know about other countries and cultures, many of the sub-fields of political science in the United States are becoming increasingly "Americanized," that is, they are too often over-theorized, narrowly specified, non-prescriptive, pre-occupied with method over substance, and rooted solely in deductive presumption to the detriment of empirical inference. In this respect, comparativists in this country, despite their marginalization within the profession, have an advantage when it comes to making an impact on the real world. Because our expertise lies outside the United States, we are literally pushed into forms of theory-building and fact-gathering that have to be specified more broadly, that take what our American colleagues regard as given and make it problematic. This tends to chip away at ethnocentrism: when one travels around the world, becomes a specialist in a particular region, learns other languages and cultures, and interacts with internationally diverse col-

leagues, it quickly becomes clear that scholarship about politics is never disinterested and the presumption of objectivity is simply hubris.

The road towards better social science and more engagement is the same: it lies in the internationalization of American political science, and not in the Americanization of comparative politics. We can learn from all methods and tools, and we can and should produce more accomplished modelers and quantifiers, but only if we also learn more languages, immerse ourselves in more cultures, do more field research, and develop stronger comparative skills. Better social science means more engagement, and engagement in the end produces better scholarship. To separate theory and practice (and disparage the latter) while insisting on the "science" of politics to the exclusion of its passions, is to set aside the hard questions of public life that do not lend themselves to parsimony and to define away the problems that do not already have a pre-existing data base. This net result is to reinforce existing hegemonies, neglect the consequences of altruistic behavior, and ultimately produce cynical students who do not believe in the power of scholarship to help make the world a better place. The alternative is to rebalance our own skewed discipline by learning from different scholarly experiences around the world that may be less "professional" but have inherited and preserved a tradition of greater intellectual engagement based on the notion that scholars have both the capacity and the duty to improve their societies. If we do so, to quote the great social scientist Albert Hirschman:

*It is then possible to visualize a kind of social science that would be very*

*different from the one most of us have been practicing: a moral social science where moral considerations are not repressed or kept apart, but are systematically commingled with analytic argument ... Such would be, in part, my dream for a "social science for our grandchildren."*<sup>9</sup>

### Notes

<sup>1</sup> Default suits had been won against the former Bosnian leader Radovan Karadzic and former Guatemalan defense minister Héctor Gramajo, among others, but these suits were won because the defendants failed to appear, and not as the result of a jury's judgment.

<sup>2</sup> The suspicion that a professor would be attacked in order to play upon anti-intellectual sentiment turned out to be correct. The defense attorney focused his rebuttal and closing argument on the academic testimony, in an attempt to discredit the analysis and the knowledge upon which it was based. He claimed that evidence from the generals' main expert witness, a former U.S. ambassador "who needed to give accurate accounts" was far superior to that of a "professor writing a thesis, needing to come up with a thesis on history" in order to promote her career. In a part of his argument that should cause great hilarity among those who understand the big power and methodological biases of American political science (or even the dynamics of my own career path), the defense attorney told the jury that producing such an analysis had led to my tenure and would make me famous. See the trial transcript of Romagoza et al. versus García et al. on the web at [www.cja.org](http://www.cja.org).

<sup>3</sup> See, for example, the letter from Heinz Eulau putting forth this position

in *Political Science and Politics*, Vol. XXXV, No. 4, December 2002.

<sup>4</sup> William Stanley's *The Protection Racket State: Elite Politics, Military Extortion, and Civil War in El Salvador* (Temple University Press, 1966) proved to be especially helpful here.

<sup>5</sup> Thomas Carothers refers to the volume *Transitions from Authoritarian Rule* in "The End of the Transition Paradigm," *Journal of Democracy*, Vol. 13, No. 1, January 2002.

<sup>6</sup> See Daniel W. Drezner, "Chicago School Democracy by America," *The New Republic*, March 12, 2003, then compare this to the evidence in the O'Donnell, Schmitter, Whitehead volumes, works by Thomas Carothers, and the recent study of Minxin Pei and Sara Kasper, "Lessons from the Past: The American Record in Nation-Building," forthcoming.

<sup>7</sup> Ido Oren, *Our Enemies and Us: America's Rivalries and the Making of Political Science*, (Ithaca and London: Cornell University Press, 2003).

<sup>8</sup> Thus, it is no accident that Latin America has lately been characterized by presidents and ministers who are well-known and well-trained academic social scientists; countries that are not rich do not have the resources to support a political science that is not relevant for public affairs.

<sup>9</sup> Albert Hirschman, "Morality and the Social Sciences: A Durable Tension," in *Essays in Trespassing: Economics to Politics and Beyond* (Cambridge University Press: 1981), p. 306.

## Engagements in Comparative Politics: Kant, Machiavelli, the Webbs & Us.



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These three individuals [the man of affairs, the statesman, and the man of the world or cosmopolitan] are united in attacking the academic, who works for them all, for their own good, on matters of theory. Since they fancy that they understand this better than he does, they seek to relegate him to his classroom (*illa se iactet in aula!*) as a pedant who, unfitted for practical affairs, merely stands in the way of their experienced wisdom. <sup>1</sup>

Kant was the academic's academic. Wordsworth's concluding couplet in an "Ode to a Skylark" was dedicated to him, at least according to my Irish grammar school teacher: 'Type of the wise who soar, but never roam/True to the kindred points of Heaven and home!' If my teacher was right these lines stereotype the academic, the philosopher of the Enlightenment, the theorist and stargazer who, by repute, never traveled more than 30 miles from Königsberg. Kant may have been a dull and asexual bourgeois bachelor, but he vigorously argued against leaving experience and practice to the anti-academics, insisting that everything 'in morals which is true in theory must also be valid in practice'.<sup>2</sup>

It is a curious feature of American

political science that one of its recent reigning prejudices, now undergoing a well-deserved assault from many angles of vision, embraced the stereotype which Kant himself rejected. That is, some of its leading exponents divorced theory from practice, and wished solely to lord over the confines of the department, the sub-field journal, and the occasional conference. For them life within the campus - and its inter-networked extensions - is professionalism. Professing to the rest of the world is, if not condemned, condoned only among the lesser-ranked IQs. Politics in this vision is, at best, data to be collected or explained; at worst, it is corruption; to participate is to be partisan, and lost to the higher calls of reason. These prejudices, of course, never stopped its exponents from intra-mural political conduct.

There may be institutional reasons why the inward, retreatist, quietist and pseudo-Kantian aspiration has recently been so vigorous in American political science. The European immediately sees the repercussions of leaving to lawyers most of public law (and of a thriving legal profession which, at its best, embraces the best social science); and of the less-noticed, but equally curious, American divorce between organized political science and public administration. And, since behaviorists and survey specialists may have been paid off with their own centers and consultancies, many political science departments may be left with cores comprised of rational choice theorists with non-empirical and non-prescriptive ambitions, or of political theorists who glory in scepticism about reason, science and enlightenment. (Two cores in some cases live together in an undeclared state of divorce). These rivals for powerlessness, be they 'rat-

choosers' or an internally divided coalition of post-structuralists, post-modernists, postcolonialists, and hermeneuticists, usually make no splash beyond the academy - fortunately, say some. Instead, it is that professionally doubted (though sometimes envied) figure, the public intellectual, who makes waves, talks on TV or radio, writes op-eds, and who spends too much time in Washington - according to the chair. The reasons why that figure is doubted within the profession are clear: s/he cannot have omnicompetence; s/he necessarily speaks mostly as a citizen rather than as an expert.

There are, indeed, good reasons why much political science research, theory-driven or evidence-based for policy-making, has less impact than it might. It may be bureaucratically and politically marginalized: politicians may want policies or pork that are hostile to research. The relevance of political science to politicians may neither be obvious nor accepted (especially since we have a healthy scepticism about the virtues of the political class). Cultural norms beyond the academy may be vulgarly empiricist: political science may be understood as mere data-gathering. These obstacles to social science in policy-making, implementation and evaluation have been spelled out by Martin Bulmer,<sup>3</sup> and the limits to professional social inquiry were articulated by Charles Lindblom nearly a quarter of a century ago,<sup>4</sup> in a still telling and astringent review of social scientific imperialism.

But we in comparative politics, wherever we happen to be, and wherever we happen to have come from (by origin or higher education), have always had two comparative advantages amongst our peers when we

profess beyond the academy. First, knowledge of other countries is usually valued inside the country within which one works. This expertise may be low down the food-chain of theory, but it provides exponents of comparative politics with a steady supply of

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“... it is one of our professional tasks ... to show the merits and defects of the dogmatic positions taken by some of our own enthusiasts for certain political remedies.”

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resources in the form of students, readers and external consumers (be they the CIA, the State Department or NGOs). Second, since part of our business is to conquer ethnocentrism in explanation (and prescription) we may be useful both to the domestic political class where we work, and to outsiders who may wish to avail of our services. We have two additional (perhaps temporary) advantages. At our best we can explain why and when economists are wrong in their universalist prescriptions. And, in my own sub-field, we have some, albeit limited, usable knowledge of the workings and malfunctioning of ethnically, communally and nationally divided territories - knowledge more usable than that of our siblings in international relations, and some of our other cousins in other social sciences.

We have pathologies, of course. We have missionaries - though very few (successful) mercenaries. The mis-

sionaries are programmatically committed to certain packages of institutional solutions, our equivalent of IMF economists; and our missionaries can be just as dangerous as economists. (It is an interesting paradox that economists, who probably cause more harm than any other social scientists, have the most public and intra-academic prestige). So, it is one of our professional tasks - inside and outside the academy - to show the merits and defects of the dogmatic positions taken by some of our own enthusiasts for certain political remedies. Consider how much better the knowledge base is now on controversies over presidentialism and parliamentarism than it was twenty years ago - partly because of engagements with the latest waves of democratization and re-democratization.

I have done some political advisory work in three locales - in Northern Ireland, in Somalia, and in Kwa-Zulu Natal, South Africa. (I have also worked elsewhere, confidentially - and working confidentially may, reasonably, be as much part of our professional domain as the lawyer's or the doctor's). I make no claims to having been a key player in assisting the varied political and constitutional reconstructions that have and may be taking place in these three regions, but I do claim that comparative politics, as a discipline, has mattered in all three locales. It has mattered both in peace processes - in mutual learning (and mis-readings) about negotiations - and in the actual and attempted political settlements made by multiple parties. Comparative politics, sometimes spoken by political scientists, sometimes by constitutional lawyers, has helped structure useful debates, and improved arguments about political institutions - by expanding choices and sometimes by

inhibiting some infeasible preferences from being pursued. In all three regions there has been interest in the debate between exponents of integration or of temporary or durable consociation, proponents of centralism or autonomy, champions of different electoral systems, and of rival approaches to organizing the judiciary and the police. When politicians themselves talk comparative politics - as they have done in Northern Ireland and South Africa<sup>5</sup> - comparative political science matters. And political scientists can make it matter. How? In at least three ways:

**(i)** *By placing sleeper-ideas into the political domain*, i.e. stretching the idea of what is possible - not the same as the cliché about thinking the unthinkable. With my regular co-author John McGarry and others I participated in debates about how to structure power-sharing arrangements in and over Northern Ireland.<sup>6</sup> In 1993, adapting ideas from others, we proposed a power-sharing executive that never came into being, but perhaps had a sleeper effect. We tried to design an executive that could be formed without too much difficulty, but not easily be brought down by a legislature. We also applied thinking about the allocation of committee places in the European parliament - commending the d'Hondt or Sainte-Laguë rules (or the Jefferson and Webster rules as they are known here) for any new Northern Ireland assembly. Though we claim no direct responsibility, and regard our own contributions as one part of a marketplace of ideas, versions of these arguments were used by politicians and civil servants in making the Good Friday Agreement - generating a cabinet in which both the numbers and the choices of ministerial portfolios amongst political parties were deter-

mined by an algorithm (d'Hondt) that inhibited protracted bargaining. Time will tell whether this device - and the numerous variants on it that are possible - is desirable, durable and exportable. No doubt it has its own defects,<sup>7</sup> but it shows that institutional innovations are not solely the product of clever politicians or of Kant's *men of affairs*: they may be partly inspired by academics. This is, of course, the public enlightenment role: it may not happen very often, or very successfully, but it is surely part of our profession.

**(ii)** *By direct submissions to commissions, committees and executives, in which proposals are framed, in the light of comparative experience, for institutional change.* In Northern Ireland academics informed by comparative politics, especially political scientists and lawyers, partly shaped debates on restructuring the police, constructing a new human rights regime, and proposals for a new administration of justice. Effective submissions avoid straying outside specified terms of reference; they, of course, explore the full possibilities within such terms of reference; and they draw upon field experience, interviews and comparative data analysis. That does not mean there is no place for the dissenting submission, counter-proposals, or rebuttals of a newly emerging conventional wisdom - all of which reject the given terms of reference. To the contrary. But, dissenters are akin to the planters of sleeper-ideas; they do not and cannot expect immediate impact - though a well-timed rebuttal can occasionally be devastating.

**(iii)** *By working with other internationally diverse social scientists, learning from them, and disseminating and debating proposals jointly.* My work

for the European Union and the United Nations in Somalia involved teamwork, based at the LSE, with an anthropologist, a lawyer, a development economist and international relations specialists, of multiple nationalities.<sup>8</sup> The project, inspired by an EU official, helped structure local debates about constitutional reconstruction in Somalia, and led me to be involved with three constitutional lawyers in assisting some Somalis in situ, in the hottest place on earth, in drafting a constitutional charter for the region of Puntland.<sup>9</sup> No one can claim that project has been a great success beyond the paper it produced, but we avoided doing harm, and arguably marginally improved the local political environment: it is just too early to tell. The internationality of the team was important (two Italians, an American, and an Irishman). The Somalis, in seminars and outside them, did not see us as homogeneous: and they saw me, rightly, as the least imperial! We were 'resource-persons' in the language of the NGOs, and learned to be just that. We saw our cultural biases better by being an international team, and checked and balanced them: the Italian constitutional lawyers wanted to solve the Italian constitution's problems abroad; they thought I saw Northern Ireland everywhere; we all ganged up on the American.

Whether my own contributions have mattered much is not for me to say - but they have made me a better comparative political scientist. Working in strongly antagonistic political environments is worth many monographs. Working with others on political projects expands your range, and makes you ransack the thoughts of your peers with greater urgency. I may, I hope, be able to develop refinements of consociational theory that might

otherwise not have occurred to me. And, I think it has made me a better teacher - at least I have a wider repertoire of telling stories.

These three sketches of how comparative politics may usefully matter are deliberately low key. They do not envision political science as a master

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“Comparative political scientists are, of course, no more virtuous than other academics or citizens.”

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science, or as a simple and unreflective repertoire of technologies. They should cause no terror about scientism or loss of scientific standards. Comparative political scientists are, of course, no more virtuous than other academics or citizens. We may abuse our skills and roles. That is why professional evaluation of our contributions is highly desirable. Trying to build evaluation into our interventions - however piecemeal or grandiose they may be - should become a professional norm. But what makes political scientists everywhere, and not just in the United States, worry about direct engagements are two related dangers that I shall stereotype as the individual Machiavelli and the institutional Webb.

Machiavelli's *The Prince* was a handbook for gangsters; and no matter how many times one may re-read the text through Quentin Skinner's or Isaiah Berlin's mesmerizing words, most of us still recoil from the role of 'realist' advisor to princes. But we are unlikely to have that unpleasant task given to us by fortune. In contempo-

rary democracies at least we occasionally choose our prince; we need not be servile towards aspirant despots; and, importantly, those of us who have tenure need not be sycophants. The other stereotype, illustrated by the Webbs, is a more pertinent portent. The co-founders of the LSE, Sidney and Beatrice, betrayed the mission of their institution in their appallingly naïve assessment of the Soviet Union as a new civilization - at the very peak of the great purges.<sup>10</sup> The Webbs remind us how bad social scientists can be --- especially those who are least accountable --- at the top of our own autonomous hierarchies. But that some may want to be Machiavelli, and that others may corrupt themselves as the Webbs did, is not an argument for keeping comparative politics in purdah. We cannot, in any case, be pure, either inside or outside; and without external engagements we are unlikely to keep our field either intellectually or morally fascinating.

#### Notes

<sup>1</sup> Kant, Immanuel. "On the Common Saying: 'This May Be True in Theory, but It Does Not Apply in Practice'." In *Political Writings*, edited by Hans Reiss. Cambridge: Cambridge University Press, 1991, p. 63.

<sup>2</sup> *Ibid.*, p. 72.

<sup>3</sup> *Social Science Research and Government: Comparative Essays on Britain and the United States* (Cambridge: Cambridge University Press, 1987).

<sup>4</sup> Lindblom, Charles E, and David K Cohen. *Usable Knowledge: Social Science and Social Problem Solving*. New Haven: Yale University Press, 1979.

<sup>5</sup> McGarry, John. "Political Settlements in Northern Ireland and South Africa." *Political Studies* 46, no. 5 (1998): 853-70.

<sup>6</sup> O'Leary, Brendan, Tom Lyne, Jim Marshall, and Bob Rowthorn. *Northern Ireland: Sharing Authority*. London: Institute of Public Policy Research, 1993; O'Leary, Brendan, and John McGarry. *The Politics of Antagonism: Understanding Northern Ireland*. London & Atlantic Heights, N.J.: Athlone, 1993.

<sup>7</sup> For an initial exploration see O'Leary, Brendan, Bernard Grofman, and Jorgen Elklit. "The Use of Divisor Mechanisms to Allocate and Sequence Ministerial Portfolio Allocations: Theory and Evidence from Northern Ireland." Paper presented at the European Public Choice conference in Paris, April 2001, presently being re-worked for publication.

<sup>8</sup> Lewis, Ioan, James Mayall, John Barker, Teddy Brett, Peter Dawson, Patrick McAuslan, Brendan O'Leary, and Karin Von Hippel. "Decentralized Political Structures for Somalia: A Menu of Options." (London: London School of Economics, 1995).

<sup>9</sup> Guadani, Marco, Ugo Matteo, John Murray, and Brendan O'Leary (eds.). "A Draft Constitutional Charter for the State of Puntland." (Garowe, Somalia: UNDOCS Consultants' Report, 1998).

<sup>10</sup> See Gellner, Ernest A. "'No School for Scandal', A Review Essay on Ralf Dahrendorf, LSE: A History of the London School of Economics and Political Science." *The Times Literary Supplement*, 26 May 1995, 3-4.

## Seeing Like a State: Two Views

### Editors' Note

Two distinguished scholars review James Scott's book *Seeing Like a State* (New Haven and London: Yale University Press, 1998, pp. 357, notes 73 pp., index 13 pp.) from different perspectives.



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The late Isaiah Berlin popularized a famous distinction between the hedgehog, who knows one thing quite well, and the fox, who knows quite a few.<sup>1</sup> With very little adaptation, the distinction can be applied to our profession.

There are some scholars who spend their entire lifetime working on one given topic/subfield/theme/theory. They adopt a cumulative research program, seeking to solve an unresolved puzzle with each advance in their work. Imagine, for instance, a scholar of ethnicity and nationalism. She may begin by studying ethnic identities in Somalia, move on to research a related problem in Nigeria, migrate to sorting out similar issues in the former Soviet Union, and then try and figure out why ethnic civil wars break out. This strategy has a potential pay off: over the course of a lifetime, the hedgehog may well end up knowing one subject inside out and, more importantly, may also contribute

some big ideas to our scholarly life. This kind of research, however, may sometimes be accompanied by a certain lack of intellectual boldness, as each new step is taken in relatively familiar territory and the ease of sure-footedness replaces the joy of an initial fumbling and groping and the subsequent gathering of confidence and speed. It may also not lead to any big ideas, even though that is the ultimate purpose of such a research strategy. Following the natural sciences, the hedgehog (cumulative) strategy of research is often deployed in our profession.

James C. Scott has acquired fame pursuing the opposite strategy - that of the fox. Finding a lifelong pursuit of the same subfield/ideas/theories intellectually unrewarding, producing a certain narrowing of vision if not boredom, he has nimbly moved from one big topic to another, displaying remarkable intellectual breadth in the process, each topic keeping him engaged for a few years but not more. He started his academic life with a study of corruption and patron-client linkages; moved on to some truly seminal and influential work on why peasants rebel, and what form peasant protest takes in the absence of revolutions and why; and in this book, he sets his analytic gaze on a third big topic - namely, why so many apparently well-meaning schemes in the world, seeking higher mass welfare and increased human happiness, end up failing or achieving the opposite of what they intended. The detailed examples are drawn from an amazingly wide range of human activities: forestry, agriculture, urban planning, the birth of "surnames," the logic of revolutionary political parties, the emergence of universal scales of measurement. Consider some examples, of necessity briefly.

Scientific forestry emerged in the late 18th century Prussia and Saxony, seeking to replace the chaos of the traditional, "naturalist" view of the "forest as a habitat" with the "forest as an economic resource." Under this latter, "scientific," dispensation,

Plants that are valued become "crops," the species that compete with them are stigmatized as "weeds," and the insects that ingest them ... as "pests." ... the trees that are valued become "timber," while species that compete with them become "trash" or "underbrush." ... Highly valued animals become "game" or "livestock," while those animals that ... prey upon them become "predators." (p. 13)

What might be wrong with this? Forest biodiversity, as is well understood now, was "an insurance policy." Compared to polycultures, monocultures were simply more vulnerable to disease and breakdown, leading often to ecological damage.

If that is so, why was scientific forestry so widely embraced? Scott's answer is unconventional. It was not simply a result of corporate profit-seeking, as the standard explanation held. Rather, it was also the project of "legibility" that the modern state needed, and deployed, for administrative convenience. The conventional forest was simply too chaotic and "illegible." The modern state needed control over its universe, requiring schematization and simplification. Commercial and bureaucratic interests thus went together.

A fascinating discussion of modern and traditional cities, parallel to that of forestry, follows. "An aerial view of a town" built during the Middle Ages had a "look of disorder" (P. 53). Its chaos, however, was perfectly under-

standable to its local inhabitants, who knew the logic and purpose of lanes, bylanes and walled cities. The very "illegibility" of such cities to outsiders was a form of defense against them. In contrast, modern city, *à la* Le Corbusier, the high saint of architectural modernism, is a "grid city." Because it is laid out in a geometric, repetitive logic, it is easily understandable to outsiders.

Is there anything the matter with such geometric simplification? Such designs may be easier to police and administer for the state, but they are not necessarily better for the inhabitants. Following the famous arguments of Jane Jacobs, Scott contends that a multi-purpose traditional urban design - a design that spatially mixes up living, shopping, working -- may be infinitely better than a geometrically designed city with each quarter specializing in one urban function - living, shopping, or working. Greenwich Village and Boston's North End, despite their apparent chaos, are much safer - and perhaps happier -- neighborhoods than upper Manhattan or many grid-like parts of Boston.

There are also some remarkable discussions in the book about when and why human beings came to have surnames. Until as late as the 14th century, ordinary Europeans generally did not have surnames, and it was more common to come across a "Luigi, son of Giovanni, son of Paolo" (p. 66); "Filipinos were instructed by the (Spanish) decree of November 21, 1849, to take on permanent Hispanic surnames" (p. 69). Scott also goes on to investigate why revolutionary political parties tended to have a vanguard and, despite swearing to mass welfare, distrusted mass initiatives, as also why collectivization of agriculture

was such a passion for so many revolutionary leaders. A search for "thin simplifications" and "legibility" marked all of these projects.

These examples generate two larger questions. Why were such schemes and ideas embraced, and why did so many of them - Le Corbusier's cities, Communist collectivization, a scientific Prussian-like forestry, Julius Nyerere's ujamaa village campaigns in Tanzania- fail?

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“It is a trenchant and, in my view, unexceptionable critique of the hubris of deductive and universalist knowledge”

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Scott furnishes two basic answers. The first has already been briefly mentioned: the necessity felt by the modern, or proto-modern, state for simplifications. The second is elaborated in a truly scintillating discussion of the difference between scientific knowledge and practical knowledge, which Scott, borrowing from the Greeks, calls *metis*. The utopian - or "high-modernist" - schemes, argues Scott, have failed because they privileged abstract, universalist, scientific knowledge over practical, contextual, local knowledge. The claim is not that scientific knowledge is something we should ignore, for much of it is demonstrably good for human welfare. Rather, it is the "imperialism" and hubris of scientific knowledge "that is troubling" (p. 340). Left entirely to itself, *without the aid of practical knowledge*, scientific knowl-

edge can have fatal flaws, undermining human welfare, instead of enhancing it. Collectivization of agriculture, monoculture forestry and agriculture, grid-like cities have all been defended most of all in the name of science.

Scientific knowledge works best in those "spheres of human endeavor that are freest of contingency, guesswork, context, desire and personal experience" (p. 321). And "*metis* is most valuable in settings that are mutable, indeterminant ... and particular" (p. 316). In a remarkable passage, Scott shows how even in the domains often thought of as belonging to science, there is an inescapable role that *metis* has to play.

Firefighters, rescue squads, paramedics, mine disaster teams, doctors in hospital emergency rooms, crews that repair downed electrical lines, teams that extinguish fires in oil fields, and ...farmers and pastoralists in precarious environments must respond quickly and decisively to limit damage and save lives. Although there are rules of thumb that can be and are taught, each fire or accident is unique, and half of the battle is knowing which rules of thumb to apply to which order and when to throw the book away and improvise. (p. 314)

Scott goes on to add war, diplomacy, and politics to the list of activities inescapably requiring a large role for *metis*.

In the end, thus, Scott's argument is not the sort of wholesale denunciation of science and modernity so common today in postmodern circles, nor is it an unqualified defense of traditions, which is also rather common.



Instead, it sets out domains where scientific knowledge and metis are respectively applicable. It is a trenchant and, in my view, unexceptionable critique of the hubris of deductive and universalist knowledge: "The single metric is an invaluable tool. Problems arise only when it becomes hegemonic" (p. 346).

Scott's argument against the imperialism of universal knowledge and his defense of practical, context-bound and particular knowledge is of great use to the field of comparative politics today. At least one of the dominant trends in comparative politics today is towards universalism, which has led to a devaluation of field research and to deduction at the cost of what used to be called "soaking and poking" in particularistic materials. For understanding why this is a self-defeating trend, which will lead to "thin simplifications" and grossly inadequate knowledge, this book as a whole, especially its chapter 9 ("Thin Simplifications and Practical Knowledge: *Metis*"), should become must reading for all graduate courses on comparative politics. If comparative politics in the past went too far towards establishing what was unique in each country, the trend in recent years has been towards uncritically applying universal modes of reasoning. Both ideas are equally flawed.

### Notes

<sup>1</sup> Berlin, Isaiah, "The Hedgehog and the Fox," first published in 1951, reprinted in Berlin, Isaiah, *Russian Thinkers*, Penguin Books, New York, 1978 and other dates, edited by Henry Hardy, page 22 (22-81) of the 1978 reprint; the original reference is to the Greek poet Archilochus, quoted by Berlin, fragment 201: "The fox knows many things, but the hedgehog

knows one big thing."

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In seeing like a state, James Scott refreshes us with his joyful curiosity and love of learning. His intellectual ambition both beckons and projects, inviting us to the feast he sets before us while regaling us with his interpretation of its significance. When launching into the history of Ujamaa villages or Chicago, studies of Lenin or le Corbusier, or discussions of forestry or town planning, Scott energetically exposit, describes, recounts, and interprets, all the time teaching us to see - whether like a state, as the title of this work implies, or like James Scott.

There are few facts in Scott's world; he problematizes what others treat as objective. Frequencies, means, expected values: these measures that describe the "reality" that other social scientists see become, in Scott's hands, royal roads to the consciousness of the observer - an observer that has herself been shaped by structures of power and significance that she seeks to describe and to explain. Look not only at this world, Scott is telling us; look also at how you have been conditioned to view it.

The force that is shaping our world is, for Scott, high modernism. In flights of hubris, the high moderns aspire to free themselves from the bonds that attach them to nature. They seek to shape the natural and transform it from master or neighbor to servant or slave. Geometry replaces chaos, will

supplants humility, and action marginalizes reflection, as people transform the natural into a source of utility and a means of satisfying human wants. What makes high modernism possible, Scott argues, is the state, which privileges its values against those of competing world-views. The alliance of high modernism with political domination represents a defining attribute of our age. The alliance is not typical; but when it occurs, be it when a "slum" is "redeveloped" or a village collectivized, the result is an unnatural act: a moment of pain that must be seen, noted, and communicated, so that it can be avoided by others in the future. Insofar as high humanism constitutes a vision of progress, the way in which we see must be rede-

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**“Scott energetically exposit, describes, recounts, and interprets, all the time teaching us to see - whether like a state ... or like James Scott.”**

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finer, so that we see the result as a triumph of a particular vision armed with the power to coerce.

Should this be the essence of Scott's argument, then there is much with which to agree. But there are also points of disagreement, and some of these run deep.

Scott is in favor of "the small" and sees "the big" as dangerous: scale leads to homogeneity and standardization, he argues. And yet there are many, myself among them, who would call for clarification. In many

respects, larger scale societies are more diverse than are ones that are small. Thus it was that Durkheim differentiated between mechanical and organic solidarity, linking the latter with large-scale societies, marked by the division of labor, rather than with face-to-face societies, populated by self-sufficient communities. As Scott states, to organize in large-scale societies, one must be guided by expected, not idiosyncratic, values. But the truth of this argument implies little about other moments of distributions, and it is the second moment, the variance, that seems the more relevant to Scott's argument.

When the central state displaces the local community, Scott argues, the result is a loss in social welfare. Scott grounds his argument on both consequentialist and intrinsic foundations. Local knowledge incorporates powerful truths, particularly about the relationship between human beings and nature; and with its loss, human societies can make costly mistakes. Scott offers numerous illustrations, some grounded on the inappropriate choice of technology and others on the dangers arising from the loss of eco-diversity. The intrinsic grounds for Scott's argument arise from political values. When local voices are silenced, then the collective outcomes will be unjust; they will not be responsive to the full range of preferences and so will violate the welfare of those too marginal to govern. The result is then either a form of totalitarianism, clothed in an ideology of modernization and progress, or the outbreak of local forms of resistance - another subject on which Scott has famously written.

But there is, of course, a third possible outcome - and one to which Scott pays too little attention, in the judg-

ment of this reviewer. Agency in Scott's argument arises from the state; it is the state that privileges the modernist program. As used by Scott, the state is a unitary actor. But what if the impetus for transformation arose not from a unitary actor, but rather a multitude of agents; and what if they do not collude but rather compete? Economic competition would result in the market; political competition, in democracy. Introducing this possibility muddies Scott's argument. Much of what Scott has to say about *metis* features strongly in the argu-

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“Might not the re-scaling of society, via the market and the state, thus perhaps preserve and privilege diversity, and therefore ... enhance social welfare?”

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ments of Adam Smith; and Smith notes their impact on the organization of production and the formation of markets - markets that draw on diversity to create, rather than to extinguish, value. And, as Scott himself admits, democracy can overthrow centralized and narrow visions of the good, forcing "the state" to accommodate the messy diversity of local visions and particular interests in ways the high modernists deplore. The untidy chaos of urban landscapes underscores the power of private rights in property; and the plurality of private voices in the chambers of our government has been bemoaned by many a critic of the democratic state in America. Might not the re-scaling of society, via the market and the state, thus perhaps preserve and

privilege diversity, and therefore, given Scott's position, enhance social welfare?

This reader obviously enjoyed Scott's work, finding it both learned and playful, entertaining but deadly serious, and at all times provocative. But I am also left with the feeling that the argument flows too easily. Scott breaks from the starting gate, exulting in the chance to learn and to teach. More deliberation, reflection, and caution might have generated a tighter line of argument, one more nuanced, more subtle, and more ironic. It might also have generated an argument susceptible to testing. As it is, the foundations for the argument remain to be established, and the argument is illustrated but not tested.

What might provide such foundations? One would be the logic of scaling; a second, the theory of information. A third would be the logic of strategic behavior. Use of these concepts, both severally and in combination, could provide insights into the conditions under which diversity is replaced by uniformity, the conditions under which people choose forms of local resistance, the use of pooling and separating strategies, the manipulation of signs and symbols, the exploitation of common knowledge, and the conditions under which incorporation will produce welfare gain or welfare loss. More closely reasoned arguments would bequeath to others not only inspiration - there is a lot of that in this book - but also lines of reasoning that could be applied to other subjects and extended to other settings.

## A Review of the ACLP Dataset

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The dataset compiled by Michael Alvarez, José Antonio Cheibub, Fernando Limongi, and Adam Przeworski (ACLP for short) is the culmination of ambitious efforts to collect a wide variety of variables to test the hypotheses put forth in the landmark book *Democracy and Development: Political Institutions and Well-Being in the World 1950-1990* (2000). It includes a sample of 135 states observed between 1950 (or the year of independence or the first year for which data on economic growth are available) and 1990 (or the last year for which economic growth data are available), totaling 4,126 observations. The unit of analysis is a given country during a particular year, and the 104 variables (including lagged variants) represent one of the most comprehensive sources of post-World War II indicators for "large-N" cross-sectional time-series studies.

This is not a trivial accomplishment. Given the well-known difficulties in gathering comparable cross-country indicators, the dataset's impressive empirical scope is a major contribution in the search for social, economic, and political measures in comparative politics. The endeavor of combining data from 27 countries from Latin America and the Caribbean, 10 from the Middle East, 8 from Eastern Europe, 44 from Africa, 3 from Southern Asia, 18 from Eastern Asia,

and 25 from the OECD group certainly deserves praise.

ACLP include a broad range of variables. The core of the dataset is composed of seven political regime indicators, which attempt to distinguish various characteristics of democracies and dictatorships. There are 23 economic indices (e.g., annual economic growth, rate of inflation, and central government expenditures), 21 social variables (e.g., birth and death rates, the number of riots in a given year, ethno-linguistic fractionalization, and the level of education of the labor force), and 4 religious measures (e.g., indicators of the percentage of Catholics, Moslems, and Protestants in a population). Finally, there are other political variables and a number of path-dependence indicators, which try to capture different aspects of the institutional history of each political regime. The type of civil-military relations, the mode of executive and legislative selection, and the percentage of other democracies in the region/world are examples of the other political variables, which serve as good complements to the path dependence indicators.

Beyond this extended coverage, ACLP's principal contribution is their innovative classification of political regimes. Instead of measuring democracy with graded scales, the ACLP's dataset provides scholars with a dichotomy: the "REG" variable, coded 1 for "dictatorships" (non-democratic regimes) and 0 for democracies. Like previous democracy indices (such as the Polity data and Coppedge & Reinicke 1991), the creators of the dataset prefer to adopt a minimalist definition of democracy. According to ACLP, the essential feature of a democracy is "contestation," defined as the existence of "regular-

ized competition among conflicting visions and interests" (Alvarez et al. 1996, p. 4). Unlike other democracy measures (such as the Freedom House Scores), however, ACLP do not base their indicator on the subjective interpretation of surveys, which makes their index more reliable. To their credit, ACLP run several correlation tests between their measure and other indices of democracy, finding some significant positive results.

A dichotomous variable is a good way of identifying big rare events (e.g., political transitions). Given the state of the current democratization research agenda, this variable is helpful in the study of the survivability of political regimes. For scholars who think that democracy is primarily a matter of kind rather than degree, this is a readily available measure for use in logistic regressions and hazard analyses.

Yet it is worth mentioning that operationalizing democracy as a dummy variable can be criticized on two distinct but related fronts. First, in terms of conceptualization, a dichotomous index based on the existence of contested elections omits a number of attributes that are not necessarily related to the electoral process but that may be equally relevant. By excluding the degree of "participation" (or inclusiveness) and other features (e.g., accountability, responsiveness, representation, equality, security, and freedom) from the operationalization of democracy, ACLP's REG is insensitive to the characteristics of partial transition processes. Second, a dichotomy may introduce certain biases into the measurement of the concept because it makes type II errors (i.e., classifying as democracies regimes that are not in fact democratic) unavoidable. ACLP themselves

recognize such a problem and they attempt to mitigate it by including a "Type II Error" rule in their coding. Their remedy makes the bias systematic, which they claim does not affect causal inferences. But should solutions to hypothesis testing overshadow concerns with conceptualization? The fact that ACLP's variable REG identifies Brazil as democratic between 1979 and 1985 when it was still under a military regime should at the very least raise questions of face validity.

The rest of the indicators in the dataset are less susceptible to challenge. The economic variables were gathered from widely-used sources such as the World Bank, the IMF, and the Penn World Tables. If these indicators suffer from some measurement error or bias, they are rather pervasive and not specific to the ACLP dataset. Similarly, the sources of most social, political, and path dependence variables (e.g., the Banks *Handbook of Political Indicators*, Singer and Small's *Correlates of War* dataset, and Keesing's *Contemporary Archives*) have been used for the construction of other datasets. Although not a guarantee, the use of these well-known sources gives ACLP's measures a badge of credibility.

This is not to say that data consumers should not question the elaboration of widely-used indicators. Extensive usage is certainly not a synonym for validity or reliability. For instance, the ACLP's Ethno-Linguistic Fractionalization index (ELF60) collected from Easterly and Levine (1997) is a widely employed measure in research on identity because it covers 129 countries. However, as discussed in the winter 2001 edition of the APSA Comparative Politics

newsletter, these quantitative measures do not incorporate the theoretical advances of the constructivist paradigm - a fact that questions the validity of such a widely used index. The point here is that although concern for overall quality should be a constant for users of any dataset, one cannot claim that the ACLP dataset is any less reliable than other widely-used sources.

My only caveat relates to the coding of those variables compiled by ACLP themselves. Besides the variable REG and those indicators derived from it, the creators of the dataset build four main regime measures that differentiate among various types of democracies and dictatorships: "INST" (classification of democracies by type of executive), "DIVIDED" (classification of dictatorships by the number of formal powers), "MOBILIZE" (classification of dictatorships by the presence of political parties), and "LAWS" (classification of dictatorships by the existence of a legislature). Given that systematic institutional descriptions of democracies or authoritarian regimes are scarce these indicators constitute an important improvement for studies in comparative politics.

Notwithstanding the benefits of having these systematic institutional measures of dictatorships and democracies, it is worth noting that ACLP are unclear about the coding process of these variables. First, ACLP do not provide the sources that they consulted in constructing these particular variables. In addition, the creators of the dataset failed to publicize who coded these variables, whether multiple coders were used, or whether tests of inter-coder reliability were conducted. When trying to replicate these measures, independent

researchers should keep these observations in mind.

Finally, data consumers should note that about 23 percent of the observations (mostly from less developed countries) are lacking information in the ACLP dataset. Unless there is no relationship between the data matrix (if no data were missing) and the missing-data indicator matrix, missing values will produce certain biases that are harmful to hypothesis testing. At the extreme, sub-samples of the ACLP dataset that are severely incomplete limit the range of statistical tools at the researcher's disposal. Thus, before using the ACLP dataset, researchers should check both the pattern (i.e., which values are missing) and the mechanism (i.e., why certain observations are missing) of missing data in order to make sure that the missing information can be either safely ignored or that appropriate methods of imputation, interpolation or weighting can be applied.

Ultimately, the value of a dataset has to be assessed in terms of its ability to offer both extended coverage and refined measures of theoretical concepts. Despite a few problems, ACLP have done exactly that. Although the dataset might not be useful for scholars interested in testing hypotheses pertaining to the 1990s, it is certainly an improvement over other datasets, which do not cover nearly as many countries or time periods. The ACLP dataset and codebook are available at:

<http://www.ssc.upenn.edu/~cheibub/dataset/>.

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## Other Datasets

### World Bank Governance Dataset

Governance Matters III: Governance Indicators for 1996-2002

Daniel Kaufmann, Aart Kraay, and Massimo Mastruzzi, The World Bank

This dataset contains estimates of six dimensions of governance covering almost 200 countries for four time periods: 1996, 1998, 2000, and 2002. These indicators are based on several hundred individual variables measuring perceptions of governance, drawn from 25 separate data sources and constructed by 18 different organizations. The authors assign these individual measures of governance to categories capturing key dimensions of governance, and use an unobserved components model to construct six aggregate governance indicators in each of the four periods. A paper explaining the methodology, as well as the dataset itself and a web-based graphical interface, are at

<http://www.worldbank.org/wbi/governance/pubs/govmatters3.html>

### 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 Mariana Sousa's review of the ACLP dataset, should contact Michael Coppedge at [coppedge.1@nd.edu](mailto:coppedge.1@nd.edu).

### Database on Political Institutions

Thorsten Beck, George Clarke, Philip Keefer, and Patrick Walsh, Development Research Group, The World Bank; and Alberto Groff, Federal Department of Foreign Affairs (Switzerland).

The Database on Political Institutions (DPI) was compiled for research in comparative political economy. It covers 177 countries over 21 years, 1975 - 1995. Included are indicators of the type of political system (parliamentary or presidential, legislative powers); degree of electoral competitiveness; party orientations on 5 dimensions; tenure, turnover, and popular vote share of the chief executive and the chief executive's party; legislative fragmentation (overall and by governing coalition and opposition); electoral rules; checks and balances; and federalism.

The data (in Excel format), a variable list, and a background paper are available at

[http://www.worldbank.org/research/growth/political\\_data1.htm](http://www.worldbank.org/research/growth/political_data1.htm)

## APSA Executive Committee Nominations

The Comparative Politics Section Nomination Committee has announced the nominees for the period 2003-05:

Vice-President and President-Elect:  
Sidney Tarrow, Cornell University

At-large member of the Executive Committee: Susan Stokes, University of Chicago, and Torben Iversen, Harvard University

These nominations will be presented and voted upon at the Section Business Meeting at this year's APSA Meetings.

The members of the nominations committee were T.J. Pempel (chair), University of California, Berkeley; Valerie Bunce, Cornell University; David Cameron, Yale University; John Cary, Washington University, St. Louis; Jeff Herbst, Princeton University.

Section President Evelyne Huber has appointed Nicolas van de Walle, Michigan State University, to serve as Comparative Politics Program Coordinator for APSA 2004.

## Contemporary European Politics and Society

The University of Notre Dame Press and the Helen Kellogg Institute of International Studies—a center of comparative social science research at the University of Notre Dame—are pleased to announce the launch of a new book series, *Contemporary European Politics and Society* under the general editorship of Anthony M. Messina. The first work to appear in the series in late summer, 2003, will be a reprint of the classic work on European integration, *The Uniting of Europe: Political, Social and Economic Forces*, by Ernst B. Haas with a new chapter by the author and a preface by Desmond Dinan.

Contemporary European Politics and Society welcomes proposals for single-authored books, edited volumes and reprints dealing with the many facets of contemporary Europe and European integration. Proposal and series correspondence may be addressed to:

Professor Anthony M. Messina  
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or

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Notre Dame, IN 46556  
[hanrahan.4@nd.edu](mailto:hanrahan.4@nd.edu)

## CP Section Business Meeting

The annual business meeting of the Comparative Politics Section will be held on Friday, August 29, 6:15-7 p.m.; the room will be listed in the APSA program.

## Sage Paper Award

Best 2002 APSA paper in comparative politics: "The Politics of Low Fertility: Global Markets, Women's Employment and Birth Rates in Four Industrialized Democracies" by Frances Rosenbluth, Matthew Light and Claudia Schrag (Department of Political Science, Yale University).

Honorable mention: "Government Formation in Corporatist Countries: An Application of the Portfolio Allocation Model to the Dutch Case" by Anne Wren (Department of Political Science, Stanford University).

The members of the award committee were Michael Wallerstein, chair (Northwestern University), Sofia Perez (Boston University), Matthew Shugart (University of California, San Diego).

## Luebbert Book Award

This was a boom year in great books. It was extremely difficult narrowing down the entrants to a short-list of ten, and frankly any of those ten could have been a winner in previous years.

Despite the remarkable quality of those top books, three quickly and consensually rose above all the others. Each of your books is superb and each will make a long-term mark on the practice of political science and the understanding of politics. Each, however, is quite different from the others in question, method, and general approach. Finally, after several rereadings and considerable deliberation among us, we made the hard, very hard choice and determined that there are co-winners and an honorable mention:

Winners:

Ashutosh Varshney, *Ethnic Conflict and Civic Life* (Yale)

John Huber and Charles Shipan, *Deliberate Discretion?* (Cambridge)

Honorable mention:

Peter Swenson, *Capitalists Against Markets* (Oxford)

The members of the award committee were Margaret Levi, chair (Cornell University), Gretchen Casper (Penn State University), and Richard Snyder (University of Illinois).

## Dataset Award

The Data Set Award Committee has completed its deliberations and has agreed to give the award for this year to the Comparative Manifestos Project which contains coded data on party election manifestos for 20 countries from 1945-1988. This project has been jointly administered by A. Volkens, I. Budge, D.R. Robertson, D.J. Hearl, H.D. Klingemann, and Judith Bara (founding Principal Investigator) and funded by numerous sources. Descriptions of the project and instructions for access to the data can be found at [www.data-archive.ac.uk/findingData/snDescription.asp?sn=3427](http://www.data-archive.ac.uk/findingData/snDescription.asp?sn=3427).

The entire data set 1945-1998 was published by Oxford University press in 2001 under the title: *Mapping Policy Preferences Estimates for Parties, Electors and Governments 1945-1998*. Editors are: Ian Budge, H-D Klingemann, Andrea Volkens, Judith Bara, Eric Tannenbaum. A disc is sold with (physically in) the book, containing all the data and information about it. The printed text describes the data and the uses made of them and considers the reliability and validity of the estimates and uses that can be made of them. There are 25 'maps' of party Left-Right movement for each country over the period.

The members of the award committee were Gary Freeman (University of Texas, at Austin), Ron Inglehart (University of Michigan), and Michael Alvarez (De Paul University).

# In this Newsletter:

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