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Letter from the President

Building a Disciplined, Rigorous Center in Comparative Politics

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In the lead article of an important symposium on the future of comparative politics (*World Politics*, October 1995, p. 4), Peter Evans offered a strong defense of what he calls the "eclectic, messy center" in our field, located between the alternatives of general theory and deep immersion in specific cases. I wish to take his idea a step further by arguing that new developments in comparative politics challenge us to build a "disciplined, rigorous center." This center should emerge from the interaction between, on the one hand, recent innovations in theory and method, and, on the other hand, approaches and tools that have traditionally been the distinctive strengths of comparative politics scholars.

My previous letters discussed three building blocks for constructing this center position: the dialogue between quantitative and qualitative methods, innovation in the tradition of comparative-historical analysis, and the interaction between theory-driven research and inductive learning from cases that can grow out of field research. First, regarding the methodological dialogue, I reported the view held by many scholars that the evolving tools for analyzing a small number of cases (small-*n*) that constitute comparative method are not simply a way station on

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the road to advanced quantitative techniques. Rather, in substantive terms, we find in some literatures a sequence of learning in which scholars move from statistical studies to small-*n* studies, and not the other way around. Further, in methodological terms, writing on comparative method generates valuable insights in its own right. Small-*n* comparison remains indispensable to our field, and a creative dialogue with quantitative researchers is pushing work on comparative method in productive directions, including new perspectives on defining the universe of cases, selecting cases, designing contextualized comparisons, and carrying out causal assessment.¹

As comparativists engage in this methodological dialogue, they should note that from the discipline of statistics we continue to hear warnings that in some domains of research, including the social sciences, the assumptions entailed in advanced statistical techniques are routinely not met.² Obviously, advanced statistics does not provide all the answers to our methodological questions, any more than the comparative method does. We need the methodological tools of both the statistical and the small-*n* traditions, and insights drawn from each can strengthen the other approach. This dialogue is an essential component of a disciplined, rigorous center in comparative politics.

Second, the tradition of comparative-historical analysis, founded by Moore, Bendix, Lipset and Rokkan, and Tilly, has likewise seen substantial innovation. This tradition has been extended and consolidated through dozens of valuable studies, published in the 1990s, which use ambitious comparisons to address questions of great political and normative significance. These new studies are especially interesting because they are responding to sharp methodological critiques that have emerged in the field of historical sociology. We find criticism, for example, of the kinds of explanatory claims entailed in the macro, structural focus of comparative-historical studies, and also of procedures for causal assessment based on J. S. Mill's methods. Given the increased attention of comparative-historical scholarship to such methodological issues – including a focus on microfoundations, new understandings of path dependence, and the use of multiple strategies of causal assessment – this literature is a second component of a disciplined center.

Third, we have recently seen productive discussions of the interaction between theoretically-informed research and rich knowledge of cases that can create opportunities for “extracting new ideas at close range.” Such knowledge of cases not only serves to test hypotheses, but also is an indispensable source of new concepts and innovative research agendas. This multifac-

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News & Notes

The Section's annual business meeting will be held on Friday, September 3, from 5:30pm to 6:30pm. The room will be listed in the conference program. The section awards will be announced at the meeting (and will also appear in the winter issue of the *Newsletter*).



The Comparative Politics Section Nominating Committee has announced its **nominations for section offices** to be filled for the period 1999-2001. To fill the position of Vice-President and President-Elect, the committee has nominated Evelyn Huber of the University of North Carolina. The nominees for the two open positions on the Executive Committee are Kathryn Firmin-Sellers of Indiana University and Susan Pharr of Harvard University.

These nominations will be presented and voted upon at the Section Business Meeting at APSA 1999. The Nominating Committee included Herbert Kitschelt (Chair), Margaret Levi, Guillermo O'Donnell, Elizabeth Perry, and Matthew Shugart.

In addition, Section President David Collier has appointed Melanie Manion, of the University of Rochester, to serve as Comparative Politics Program Organizer for APSA 2000.



Political Behavior invites submissions for a **special issue focused exclusively on Comparative Political Behavior**, guest-edited by Richard Johnston (University of British Columbia). On the surface, at least, the 1990s have brought massive electoral change. Long-established parties of government have disappeared, as in Italy and Canada. In some places, social democratic and labor parties were moved to the electoral margin even as, elsewhere, overseas equivalents of Clinton New Democrats occupy the high ground. Are these changes the culmination of trends first observed in the early 1970s, the breakdown of cleavage structures dating back to the 1920s and before, gradual dealignment, deficit politics, the rise of postmaterialism and of new forms of identity politics? Or do the changes reflect the collapse of the Cold War system? Or has less changed than meets the eye? In this special issue we invite papers focused on these and other important topics re-

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eted interaction between cases and theory receives support from many sides. For example, forceful advocates of theoretical innovation – such as David Laitin and Robert Bates, my predecessors as Section President – are likewise forceful advocates of creative field research. Another example is found in the allocation of funding by the Social Science Research Council, which emphasizes the anchoring of theoretically-driven disciplinary agendas in field research and area-based knowledge (see the report on SSRC funding by Hershberg and Worcester in this issue of the newsletter). Relatedly, the current \$25 million Ford Foundation program for “Revitalizing Area Studies” serves as a reminder that area-based knowledge remains a basic component of the international studies enterprise in the United States. Against this backdrop, scholars seeking to construct a disciplined center in comparative politics face a crucial challenge in promoting this multifaceted interaction between cases and theory: rigorous training in field methodology and in strategies of inductive research too often receives insufficient attention in methodology courses within political science. The discipline of sociology, which has a stronger tradition of offering courses on these topics, may provide useful models for graduate training in comparative politics.

I am convinced that these three developments in our field – the dialogue between quantitative and qualitative methodology, innovation in comparative-historical studies, and the interaction between theory-driven research and inductive learning from cases – create an opportunity for consolidating a disciplined, rigorous center in comparative politics. This center combines the substantive richness that can derive from deep engagement in cases with the well-articulated standards for formulating and testing hypotheses offered by new theoretical and methodological approaches. A fundamental goal of ongoing scholarship and of graduate training must be to support the kind of theoretical and methodological pluralism needed to sustain this center ground.

Notes

1. See, for example, Gerardo Munck, “Canons of Research Design in Qualitative Analysis,” *Studies in Comparative International Development*, Vol. 33, No. 3, forthcoming.
2. David Freedman, “From Association to Causation: Some Remarks on the History of Statistics,” *Statistical Science*, forthcoming. Also available as Technical Report No. 521 at <http://stat-www.berkeley.edu/tech-reports/index.html>. ❖

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lated to the area of Comparative Political Behavior. Deadline for submissions: November 31, 1999. Submissions should be sent to the main *Political Behavior* address: Diana Mutz, Department of Political Science, 2140 Derby Hall, 154 N. Oval Mall, The Ohio State University, Columbus, OH 43210-1373.



Political Behavior invites submissions for a **special issue on Evaluating Citizen Competence**, guest-edited by James Kuklinski (University of Illinois). The purpose of the papers will be to grapple with the problem of choosing criteria by which to judge citizen performance. Converse was the first researcher to explicate precisely what those criteria should be: ideological understanding, issue constraint, and issue stability across time. Since then, authors have offered a variety of alternative criteria, including full information (an unattainable goal), the effective use of heuristics, and the ability to connect self interests and policy alternatives. Although each of these criteria has served a purpose, researchers have offered them on an ad hoc basis. Moreover, at times the performance criteria and the actual performance itself have not been fully distin-

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Good Reads

The Political Scientist's Can Opener

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American Academic Culture in Transformation: Fifty Years, Four Disciplines (Princeton, 1997) led me to extend the old joke about the physicist, the chemist, and the economist trapped on an island who need to open a can of food. The physicist says: "Let's drop the can from a tall tree and use the force of gravity to open the can." The chemist argues: "Let's use seawater to rust the can and then pry it open." The economist begins: "First, assume a can opener."

Few people know this, but there was also a political scientist on the island. When asked how to open the can, the political scientist replied: "First, assume the economist is right."¹

American Academic Culture in Transformation is an ambitious effort to examine changes in academia over the last fifty years by reviewing two disciplines that have maintained an "intellectual unity and tight professional control" – economics and philosophy – and two that have a "tendency to division and fragmentation" – political science and literature. The editors, historians Carl Schorske and Tho-

mas Bender, asked Robert Solow and David Kreps, and Charles Lindblom and Rogers Smith to recount developments in economics and political science respectively, over the last half-century.

In separate essays, Solow and Kreps show great respect for the progress made in economics, while Lindblom and Smith stress the lack of findings in political science and suggest that political science as debate is political science at its best. (It has been too many years since my BA in Russian language and literature for me to say anything intelligent about the literature and philosophy sections of the book, but having read these chapters I am glad that I am in political science.)

In marking the progress of economics, Solow seeks to dispel the notion that the field has become formalistic – i.e. too mathematical and divorced from real world concerns. In his view, most models rely on intuition and few are deeply mathematical. Solow notes that the relatively simple kind of model-building that now dominates the field is an improvement over the discursive method of economics that he learned in graduate school.

Kreps presents an intriguing analysis of developments in microeconomics. He argues somewhat tongue-in-cheek

that the canonical principles of greed, rationality, and equilibrium have held up well. He notes that adherence to these principles have allowed economists to transport models developed in one subfield of economics to others and thereby make progress. Moreover, as economics has expanded beyond the neoclassical approach to include studies of markets characterized by small numbers, costly information, and institutions, these three core principles have paid handsome benefits.

Yet, Kreps also leaves open the possibility that a revolution may be brewing that will weaken adherence to the canonical principles. He depicts how some mainstream economists are creating models based on adaptive learning rather than strict rationality, are viewing firms as organizations rather than as purposeful monoliths, and are modeling and analyzing non-equilibrium states. Kreps expresses doubt that scholars working "outside the church" will erase the advances made over the last thirty years, but recognizes the potential importance of this line of work.

Kreps writes effortlessly and with such good humor and humility that the essay is a joy to read. Moreover, his willingness to entertain thoughts deemed heretical by many

within his field only increases his credibility.

In contrast to the optimism of Solow and Kreps, Lindblom offers a deeply pessimistic view of the possibility of science in politics. Relying on reviews of social science progress by a host of major figures from Gabriel Almond to Maurice Duverger, Lindblom states: “[O]ne has to search tediously through six hundred pages of contributions to find less than a half dozen identifications of a finding – each itself dubious.” Lindblom is also troubled that “there is no clear, unmistakable, demonstrated connection between political science accomplishments and society’s achievements.”

Lindblom argues that political scientists rarely if ever make findings. Instead, they report evidence, construct checklists, make normative evaluations, and spend an awful lot of time simply correcting the discipline’s own errors. At best, political science refines lay thought. In his view, the discipline has been crippled by a failure to “resolve the contradiction between long-standing ideals on one hand and feasible productive methods on the other.” This criticism is far stronger (and far more sophisticated) than the usual lamentations about the lack of progress in political science. One is hard pressed to find a more thorough or trenchant critique of the field.

Smith shares Lindblom’s skepticism, but roots his concerns in the difficulty of combining a commitment to democratic institutions and positivism. He notes that “American political science has always been shaped by two oft-conflicting desires: to serve American democracy and to be a true ‘science’.” Smith argues that this inherent tension has led the field to swing on a pendulum characterized by “periodic debunkings of the prevailing forms of political science, followed by quests for a ‘new science of politics.’”

The editors have certainly stacked the deck by choosing two scholars from political science who are skeptical of politics as science. Both scholars mix normative and positive theory to an extent that would make many in the field uncomfortable. Moreover, both scholars are writing primarily about the study of American politics. These points aside, their critiques merit discussion.²

How severe are the splits between economics and political science? Is political science so plagued by normative issues that it is doomed to battle sociology for the lowest rung on the social science ladder, as Lindblom suggests? Is political science in such dire straits? It seems to me that the answer is no, but this may be a rationalization by a second-year professor seeking to validate a career choice.

First, many political sci-

entists work more like the economists of Solow and Kreps than the political scientists/philosophers of Lindblom and Smith. Solow describes how economists build models that seek to simplify complex realities and then test implications from these models by gathering data. With a relaxed definition of a “model,” this description fits many practicing political scientists well.

Part of the inferiority complex plaguing political scientists may stem from an overly rosy view of economics. We tend to view debates in economics as ending in clear victories, but as Solow notes “old models never die, they just fade away.” In a wonderful turn of phrase, Kreps recounts that the debate between freshwater economists (Chicago, Carnegie-Mellon et. al.) and saltwater economists (MIT, Stanford, et. al.) over the value of rational expectations in macroeconomics ended with both sides claiming a brackish victory.³ Ambiguous resolution to scholarly debates is familiar to political scientists – as is the faddishness that Kreps claims is so prevalent in economics.

The slower progress in political science compared to economics likely comes from the subject matter in our field. Economists typically study the choices of individual actors in the relatively stable institutional setting of a market. Political scientists, particularly

comparativists, study behavior in settings characterized by institutional instability and thus face a far more difficult task. The degree of institutional stability may also account for variation in progress *within* economics and political science. It may explain why corporate finance has progressed more quickly than developmental economics and why American politics has progressed more quickly than comparative politics.

Economics is not in the same shape as political science, but the distance between the fields may be less than is commonly recognized. Moreover, this distance is shrinking as formal theory takes a stronger hold in political science and the study of institutions takes a stronger hold in economics.

Second, the claim that normative concerns about democracy necessarily restrict progress in political science is debatable. Again a look at economics is helpful. It has not been held back by a concern for efficiency – which, with some alterations, can be seen as a disciplinary equivalent of democracy in political science. One can certainly make the case that many political scientists working in many different areas are less constrained by their normative biases than Lindblom and Smith claim.

Third, Lindblom's philosophy of science seems unduly restrictive. To put his argu-

ment (too) simply: scientists make findings by demonstrating with "high probability the truth or falsity of a nomothetic proposition;" political scientists have not yet done so; and therefore "political science as debate" is perhaps the best we can do.

To take each point in turn. Knowledge, if not science, can advance apart from nomothetic proofs. Save the law of supply and demand, it has been a while since anyone in economics proved a nomothetic proposition. Economics is generally seen to be progressing by generating logically consistent theories that account for important empirical regularities with a high degree of accuracy, if not law-like generalizations. This slightly lower bar for progress is what most social scientists seek and is a worthy goal in itself. Moreover, it is a close approximation of what most political scientists practice.

Lindblom is correct that the field has not proven any nomothetic propositions, and perhaps that it should have done so by now, but the empirical content of political science is also richer than Lindblom suggests. Most political scientists agree that proportional representation tends to produce multi-party systems, that democracies do not fight each other, that collective action problems plague social mobilization, that democracies tend to persist in high-income

countries, and that education is positively related to participation. Not law-like generalizations, but important findings, nonetheless.

At a slightly lower level of proof, we can certainly recognize that Barrington Moore improved the explanatory power of modernization theory, that Robert Bates improved theories of economic development, and that Gary Cox improved theories of electoral laws by accounting for empirical anomalies unexplained by existing theory. Lindblom himself advanced our understanding of pluralist theory by advancing the "privileged position of business."

In general we should be wary of a false dichotomy about the potential achievements of the field. Because political science does not make laws with the same precision as physics, we need not be limited to political science as normative debate.⁴ It is perhaps more useful to think of a continuum of scientific rigor and recognize advances along the way.

Despite these criticisms, the book works well at two levels. First, it works as personal histories of the scholars. Lindblom's self-effacing interpretation of his contribution to the field is refreshing. Moreover, to trace the intellectual trajectory of such luminaries in social science is a treat, particularly for young scholars.⁵

Second, it works as a challenge to political scientists to take a stand on the relationship between one's work and the goals of the field. Lindblom's critique of the goals of the field in particular forces the reader to reassess the value of the enterprise of social science in general and political science in particular. By making us think more deeply about the aspirations of the field, and its relation to our own work, the book merits attention for all social scientists.

Perhaps with reflection political scientists may yet devise their own can opener.

Notes

1. My colleague David Rowe noted that thirty years ago the political scientist would have said: "Assume the sociologist is right."

2. As is unavoidable in such retrospectives of the field, large areas of research are given short shrift. For example, rational choice approaches to politics are given particularly rough treatment. Smith's claims that formal modeling in political science is in decline and that rational choice scholars seek a "universal theory of politics" are subject to debate.

3. *The Economist* (May 8, 1999, p. 84) stated this point quite baldly: "Economics is not and can never be a proper science."

4. Two points are in order. First, as in political science, theories in physics often ad-

vance beyond the ability of scholars to test them well. Second, normative theory has a very useful role to play in political science, but there are dangers in mixing normative and positive research programs.

5. An overview chapter of political science by Ira Katznelson on the 1960s is particularly adept. ♦



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guished. The special issue will serve as an opportunity to provide a comprehensive and more fully integrated treatment of the topic. Deadline for submissions: December 31, 1999. Submissions should be sent to the main *Political Behavior* address: Diana Mutz, Department of Political Science, 2140 Derby Hall, 154 N. Oval Mall, The Ohio State University, Columbus, OH 43210-1373. ♦

Book Reviews Needed!

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

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

Continuing Debates

Expanding Paired Comparison: A Modest Proposal¹

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In his presidential article of the Winter 1999 issue of this *Newsletter*, David Collier makes a powerful case for country-based, comparatively-structured analysis. Collier is too modest to say so, but he and his collaborator, Ruth Berins Collier, are skilled practitioners of a particular type of comparative analysis: *paired* comparison (Collier and Collier 1991). On reflection, much of the pathbreaking work in our field falls within the tradition of carefully-paired, theoretically-structured comparison, but it has seldom been examined as a strategy of comparison (but see Lijphart 1975). Here is an effort to do so and to propose a complement to Lijphart's "comparable cases" strategy.

Common Paths and Foundations

Most paired comparison shelters methodologically under the umbrella of "most similar systems designs" – treatments of two, or few cases chosen to maximize comparability, mainly employing configurative, historical, and qualitative methods. These comparative treatments range from the anecdotal and ethnographic to the systematic and rigorous. (Not that there is no inherent reason why quantitative methods cannot be used in paired comparisons, but the examples are surprisingly few.)

Most advocates of small-*n* comparisons use solid bedrocks of similarity to gain control over the number of potential variations surrounding their cases. Their assumption: by allowing for variance in the dependent variable while limiting the number of potentially causal factors, they can approach the logic – if not the standards – of statistical approaches and enhance their claim to internal validity (Bunce 1999, p. 16).

Thus the strength of Barrington Moore's analysis of the contrasting French and British paths to democracy rested on their similar endpoints. Peter Katzenstein compared Swiss and Austrian corporatism as contrasting instances of small states' adaptation to international competition. Peter Hall viewed variations in political economic policy-making in Britain and France in light of the two countries' commonalities as liberal capitalist states. Robert Putnam and this author built comparisons of northern and southern Italy on the platform of a single state's institutions. These authors bet on the similarities in their cases, making it less likely that unseen variables explained the outcomes they wished to explain.

This "common paths and foundations" approach has both strengths and pitfalls. The major strength is the ability to combine analytical leverage with in-depth knowledge; the major weakness is, with so few cases, the inability to array a large enough number of variables to allow alternative theories to be considered.

We can illustrate both by recalling the collapse of democracy in Germany and Italy after World War One. When these countries turned to authoritarianism after recent suffrage expansions brought the working class into the polity, some observers concluded that "working-class authoritarianism" was the culprit. What they failed to see – because they were looking for similar underpinnings – was their different starting points: a Germany with a large, industrially-based, Social Democratic subculture and an Italy in which a reservoir of rural radicalism escaped the control of the Socialist party. Focusing on common paths and foundations led to interpretations that ignored different paths to similar outcomes. That is no reason to discard the "common foundations" approach, but it suggests another: comparing causal mechanisms in wider ranges of cases.

Uncommon Foundations

No less attractive for its qualitative depth than the “common foundations and paths” approach, but based on a broader range of variation, is the search for mechanisms that produce similar outcomes in different kinds of system. By stretching the boundaries of paired comparison to more-different kinds of system the analyst need not lose the advantages of context-rich comparison familiar from the comparable case strategy, but can identify causal mechanisms that repeat themselves across broad ranges of variation and concatenate differently with other mechanisms and environmental factors.

Consider Valerie Bunce’s research on policy innovation in state socialist and western democratic regimes (1981). Bunce had observed that Soviet leadership succession coincided with major increases in state budgetary expenditure and policy innovation. Rather than retreat to the sovietological instinct then dominant in her area – which might have stopped at factors like internal power struggles, centralized control, or the personality characteristics of new leaders – she turned to leadership succession in very *different* systems. By comparing leadership succession in state socialist and liberal capitalist regimes, she discovered similarities in the policy consequences of leadership change. Uncovering

these outcomes led Bunce to reach under the surface of these unlike systems for the common mechanisms that link succession in office with policy innovation. As Bunce puts it in a more recent contribution:

By examining similar outcomes across apparently diverse contexts, this approach can go far in eliminating a range of plausible causes (which is precisely the overarching goal of the scientific method) and in defining what constitutes necessary (but not sufficient) conditions (1999, p. 16).

This points to the major advantage of the paired comparison of different types of polity or process: the capacity to point to robust causal mechanisms that repeat themselves across broad ranges of variation and concatenate differently with different environmental conditions and with each other. Let us turn to this now.

Comparison of Mechanisms

What kinds of mechanisms? With Jon Elster, I define mechanisms as “frequently occurring and easily recognizable causal patterns that are triggered under generally unknown conditions or with indeterminate consequences” (1999, p. 1). Some well-studied mechanisms are the self-fulfilling prophecy, the prisoner’s dilemma, tit-for-tat, and so forth. They operate in many different contexts but be-

cause they are lodged at the individual level, they fail to exploit the full range of analytical leverage offered by comparative analysis. They are basically limited to only one type:

Cognitive mechanisms, which operate through alterations of individual and collective perception; words like “recognize,” “understand,” “reinterpret,” and “classify” characterize such mechanisms.

There are two other main types of mechanism of interest to social scientists that seldom appear in cognitively-based individualistic analyses:

Relational mechanisms, which alter connections among people, groups, and interpersonal networks; words like “ally,” “attack,” “subordinate,” and “appease” give a sense of relational mechanisms.

Environmental mechanisms, externally generated influences on the conditions which affect contentious politics; words like “disappear,” “enrich,” “expand,” and “disintegrate,” applied not to actors but their settings, suggest the sorts of cause-effect relations in question.

Consider *brokerage*, a relational mechanism compatible with a wide variety of environments and cognitions, which we can define as the mediation of the interests and identities of disparate and unconnected actors by a third actor who brings them together through communication, the offer of mutual advantages, and the threat of constraint. American urban politicians, Italian government ministers, Mexican *caciques* use it to bring disparate supporters together around their electoral interests. We even find it in insurgent episodes, like South African liberation, in which trade unions were linked to community-based actions through consumer boycotts (Price 1991, p. 165).²

We can learn much from seeing how similar mechanisms concatenate to produce what Elster calls “molecular processes.” (p. 33) Consider mobilization – a familiar relational process in contentious politics that takes a variety of forms as it intersects with different environmental and cognitive processes. In western Europe it had intersected with routine processes of electoral and trade union activity and built identities based on class socialization. But in the repressive environmental conditions of Czarist Russia, recruitment had to take covert and controlled forms; class was a symbol rather than a mechanism of recruitment, and activation was sporadic and

risky. The result was a mobilizational style that – in the context of the collapsing Czarist empire – produced the historically unique episode of the Russian Revolution. Common mechanism, radically different outcomes.

Mechanism-based comparison can also help us with a problem that common-foundations comparison has not solved: how to explicate and trace the paths of similar outcomes from different starting points. Consider the protest control systems that developed in a variety of Western countries after the 1960s. Starting in the United States, police across the Western democracies developed standard techniques of negotiation, advice, and facilitation of protest. In the decentralized American policing system, these techniques were propagated through associations of police chiefs, the U.S. Army Police School, informal networks, and state subsidies; in France and Italy they were imposed by centralized police hierarchies under national political control (della Porta and Rieter eds 1998). These environmental differences affected implementation but the important point is that through contrasting mechanisms, different types of system produced fairly similar outcomes – a result we might not have noticed had our comparison been limited to countries with common foundations (or had we slavishly obeyed the injunction *never* to sample on the

dependent variable!).

There are demands and costs to the paired comparison of unlike systems. The major demand is that like all different systems designs, it requires a strong theory – but this is a virtue as well as a demand. The main costs are three: first, there may be so many explanatory variables in play that independent variables of interest are unlikely to be tested in any rigorous way. Second, it is hard to build up the same degree of expertise of the specialists who practice comparable-cases comparison. And third, by comparing politics across wide ranges of political practice and structure, we are likely to overlook contextual factors that enrich the case study method.

Some of these problems are the problems of any new approach, others are shared with more-different-systems comparisons of a more familiar kind, others can be confronted at a second stage – after robust mechanisms are identified across a range of cases through recourse to other methods.

This takes me to my final point – re-building a scholarly community of comparativists. The paired comparison of unlike cases is no panacea, but it may help to bridge the practices of different groups of scholars. Like most-different systems analysts, it stretches the boundaries of comparison to a wide range of polities; like area specialists, it is able to delve deep into the intricacies

of individual cases; and like rational-choice aficionados, it examines common mechanisms across a wide range of systems. Perhaps building these bridges can help us avoid falling into the abyss of paradigm warfare into which our subfield sometimes seems determined to descend.

Notes

1. This article is extracted freely from Doug McAdam, Sidney Tarrow and Charles Tilly, *Dynamics of Contention* (in preparation), ch. 5. Useful comments were made by Val Bunce, David and Ruth Collier, Miriam Golden, Peter Katzenstein, Peter Lange, and Jonas Pontusson.
2. For examples of brokerage and some of its manifestations – including the South African case, see McAdam, Tarrow and Tilly (in preparation), ch. 7.

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- Robert M. Price, *The Apartheid State in Crisis: Political Transformation in South Africa, 1975-1990* (Oxford).
- ❖

Book Reviews Needed!

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

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

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Articles

Nudging Serendipity: Support for Third Country (C3) Research

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When he was president of this group, David Laitin identified an important problem for comparativists and for our political science departments.¹ He noted that we each make a critical decision when we transition from our dissertation project to our second substantive research project: either we elect to follow our substantive/theoretical interests to a new geographic venue, or else we roam across the same real estate examining a different substantive/theoretical issue. He argues that the discipline has underestimated the costs of doing the latter and has overestimated the costs of doing the former. As a result, most comparativists head off to find a second problem to solve on the same soil. Laitin is correct. Over the course of their careers, most comparativists do remain more closely identified with the country or region where they undertook their first extensive field research (C1) than with the questions they asked while there.

But there is a second, related problem worth addressing. Either implicitly or explicitly, comparativists compare C1 with their home country (C2). Since most practicing comparativists

in the APSA are Americans, the United States becomes the most common C2.² In this essay I want to follow up some preliminary discussions held recently under the auspices of the Social Science Research Council and the Abe Fellowship Program concerning the special problems faced by that smaller number of specialists who eventually venture further afield and apply their hard earned C1 expertise and tacit C2 knowledge to a new, third case (C3). This may happen at any time in a comparativist's career and is as consequential for the scholar and his home department as is the choice to follow theory or regional specialization in second projects.

The C3 problem is not everyone's. The language and cultural skills of Arabists or Latin Americanists are portable in ways that others can only envy. Even Sinologists have multiple venues to apply their skills. Many Europeanists, for their part, seem to have little difficulty engaging materials from three or more national cases. Note, for example, the success with which Peter Gourevitch built his argument using material from France, Germany, Sweden, and Great Britain, as well as the United States, or how Gregory Luebbert reviewed the transformation of social coalitions across all of interwar Europe.³

C3 is a particular problem for those comparativists who specialize in countries like Japan, Korea, Turkey, India, or Russia, where

the ratio of investment in language and contextual learning to the opportunities for their application is particularly high. There is a double whammy in these cases: it takes considerably longer to acquire C1 expertise and, with respect to many analytic questions, there are many fewer places to apply it. When starting a new project, these "high ratio" comparativists may have little time or energy left for duplicating the detailed learning they did when they began working on their first country.

Over the course of a career, therefore, many such scholars write single country case studies *in seriatim*. They might write an initial book on *The Politics of Subject X in C1*. This positions them to become one of two kinds of niche players: either they become the "go to" author for a chapter on Subject X in a book on C1 or the author of choice for the chapter on C1 in the edited volume on Subject X. This same process can be repeated after the author's second book: *The Politics of Subject Y in C1*. In African studies, as Laitin pointed out, this meant that scholars who started out working on nation-building in the 1960s became specialists on debt in the 1970s, democratization in the 1980s, and failed states in the 1990s. The sorts of C1 and (at least tacit) C2 contributions (i.e., books spun off into chapters in edited volumes) that result from such intraregional roaming is certain to be contextually deep.

But, they may not be conceptually broad. And, in a discipline that celebrates breadth and depth as one route to fuller theory, contributions of these area specialists are apt to be discounted heavily.

Whether because their work finds so consistently limited a niche, or whether because they grow bored with work on the same country, some area specialists have begun to get restless. Some have forsaken their country studies and have reached out to theoretical pursuits. Others are reaching out to third and fourth cases in greater numbers than ever. But moving from C1 to C3 is fraught with peril – both for the field and the researcher. The field risks losing critical expertise as its deep area specialists migrate to new venues. Researchers risk becoming dilettantes who know more and more about less and less. But to the extent that there is a potential payoff in better theory, the incentive to migrate remains.

The path of one's professional migration from C1 to C3 varies with the kinds of questions that interest the researcher and with the sorts of analytic skills s/he has acquired. Two extremes frame the issue of how well defined research questions must be before C3 comparisons can properly be made. If a researcher is interested in many cases, many variable, large-*n* research problems, advanced "quantoidal" training is undoubtedly more important than language or deep cultural understanding. Here the issue is about preparation for "C_{*n*}" cases—where *n* can be in the dozens or

more. While the researcher may need the best multivariate statistical training money can buy, this is not the C3 problem I have identified or am trying to solve.

If, on the other hand, the researcher is a "narratoidal" process-tracer, then s/he has to make a tough choice between dependence on English-language sources and investing in learning an additional foreign language. The choice is not always easy or obvious. Some – particularly those armed with specific questions derived from well-designed concepts wrapped in a coherent theory – will find that English language sources are sufficient to allow an informed rough sketch of the situation in C3. The researcher will hold the sketch up against the more detailed account of C1 and (typically) the United States. This often works quite nicely – as long as the researcher then submits drafts to C3 researchers who can read and critique it to prevent fatal misinterpretation. S/he can generate reasonable hypotheses and identify relationships that the standard C1-C2 pairing might miss.

Others – particularly those on a more inductive hunt for parallels and differences, and with a somewhat greater threshold for the frustration of intellectual *cul-de-sacs* – are likely to need direct access to C3 language materials. How they get that access depends in large part upon the problem they are studying. If their process-tracing is particularly fine-grained (e.g., comparative study of decisions to extend suffrage, to redistrict electoral systems, or of social policy for-

mation), there may not be secondary sources available in English. If, on the other hand, the research is targeting particularly well-documented cases (e.g., decisions for war and peace), there may be better prospects for the non-speaker of "C3-ese." The latter can use secondary material and be relatively independent, while the former will need research assistance and hence are likely to have to be more senior scholars.

There are many roads to C3 research. Different scholars follow different trajectories because different questions invoke different approaches. Some projects require "thin" area skills, while others require "thick" area skills. That is, in some cases language fluency and cultural intimacy are the *sine qua non* for success, while in others, it may be sufficient to visit for a few months and get specific answers to specific questions. For example, John Campbell claims that "for many kinds of research, one would get 75 percent of the value of the third case in three or four weeks of reading and traveling."⁴

On the other hand, there are projects that have "thick" or "thin" theoretical ambitions. Journalists reside where thin area skills meet thin theoretical ambition. Barrington Moore lived where thick area knowledge meets thick theory. So do Theda Skocpol and Avner Greif. This is the realm of both historical sociology and narrative game theory. Clifford Geertz and the political anthropologists he inspired valued detailed description over grandiose theory. So do those

who, like Michel Crozier, focus intently upon single bureaucratic institutions. Gary Cox and other students of comparative electoral systems value coherent, deductive theory over excessive description. The range of routes toward C3 can be mapped as follows:

Where Theory Meets Context at C3⁵

Even if we would prefer living in one of the three ideal type corners where one or both of our central pursuits is “thick,” most comparativists live in a middle neighborhood, where most theorizing neither explains history across centuries for multiple countries, nor posits a parsimonious and universal explanation for political behavior. Instead, most of us try to provide theoretically sensitive explanations built upon close examination of empirical cases.

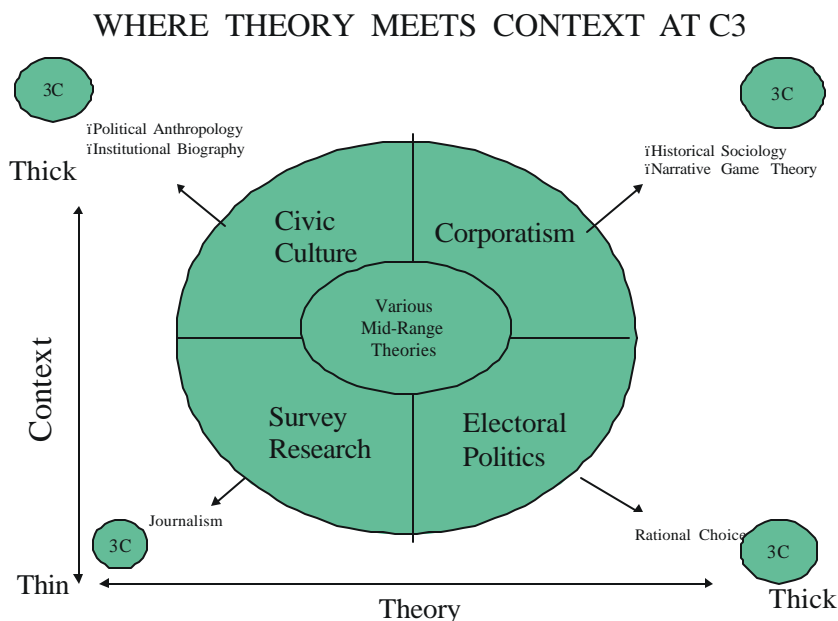
But if it is that “doubly thick” variant of C3 we are pursuing, there may be two ways to

get there. The most common route – and likely the one taken by incumbents like Barrington Moore and Peter Gourevitch – requires reliance on C3 research assistants and other English-speaking local interlocutors. This option may be more available to senior scholars than to dissertation students. But career stage ought to be a second order problem. Levels of expertise must first be matched to particular research agendas. Well trained and properly funded junior researchers certainly can complete contextually rich and theoretically informed C3 research. In a recent presidential missive, David Collier saw “a career sequence that moves from a single-country dissertation to multi-country research” as both common and logical, and while this certainly has been the path many have trod (myself included), I wonder if one size fits all.

It is instructive to look closely at those multi-country comparisons that include a close

analysis of the case about which the scholar has the greatest expertise – i.e., C1 – that work best and least well. In Japanese studies, for example, there are several studies that exemplify this sort of C3 project. Few are by political scientists, however. Robert Cole’s study of auto workers in Japan, Sweden, and the United States is a splendid example of clear thinking across sometimes unexplored cases, but Cole is an industrial sociologist.⁶ Gregory Kasza’s study of mass organizations under authoritarianism is the best example of such a study by a political scientist.⁷ Each regional or area subfield can take its own C3 census.

As I have already suggested, few comparative studies have been broader or more influential than Barrington Moore’s *Social Origins of Dictatorship and Democracy*. We know that this was not because Moore mastered five foreign languages – or even because he got every case right. Theda Skocpol made a similarly bold effort in her study of revo-



lutions, without pretending to master each case in the detail expected of an area specialist. A France specialist, John Zysman, included a detailed Japanese case in order to generalize his argument about the importance of financial systems in politics.⁸ That none of these scholars' efforts satisfied every area specialist does not detract from their intellectual and analytical impact.

How can we make more of this happen, should we wish to do so? The obvious place to start is the graduate training programs. So-called "broadening grants" are already an important color on the SSRC pallet. Collier's letter warns that adding national cases to dissertations has been frustrated by the practical and intellectual limitations of dissertation writers. There are other, less obvious possibilities. Foundations might consider grants to research institutes that encourage the sorts of team teaching or interdisciplinary research that nurture exploration of new cases. Faculty who reside in functionally defined research institutions (e.g., Social Policy Research Centers, Centers for Security Studies, Research Centers for the Study of Elections, Survey Research Centers) as well as in area centers might be identified and rewarded with seed funding. So might scholars from different regions or sub-fields who team-teach graduate research seminars. Other scholars could be provided summer grants to visit dissertation students they supervise who are doing field research in third countries. They might be provided

support to deliver papers and serve as discussants on panels at professional meetings in countries they have not yet studied firsthand.

In short, we should nurture networks that nudge the serendipity of scholarly inquiry across geographic and intellectual borders. The result, I suggest, will be simultaneously deeper and broader knowledge. Getting there will depend on acceptance of the premise that deep knowledge derived from one case can profoundly illuminate understanding of other cases – and that this sort of illumination is not available through deductive modeling or through quantitative data sets alone. This is not about "retooling" tired area specialists, but about broadening some who will contribute to a fuller comprehension of politics comparatively. Some who travel this way will parlay their hard-earned C1 knowledge by generating new theory. Others will test existing theory. Some will identify entirely new puzzles. Others will explain important outlying cases. There are a great many ways in which nurturing C3 capabilities can enhance comparative political research.

Notes

1. Laitin, David. "Retooling in Comparative research," *APSA-CP Newsletter* (Summer 1994).
2. This issue was first addressed by Donald T. Campbell, "'Degrees of Freedom' and the Case Study," *Comparative Political Studies* (Volume 8, Number 2, pp.178-193).
3. Peter Gourevitch, *Politics in Hard Times: Comparative Re-*

- sponses to International Economic Crises* (Cornell, 1986). Gregory M. Luebbert, *Liberalism, Fascism, or Social Democracy: Social Classes and the Political Origins of Regimes in Interwar Europe* (Oxford, 1991).
4. John Campbell, "Trying to Study Long-Term Care Insurance in Germany," Paper prepared for the Social Science Research Council and Abe Fellowship Program Workshop on The Third Case, Airlie House, Virginia, March 1999, p.1.
 5. Thanks to T.J. Pempel for help with this sorting.
 6. Robert Cole, *Work Mobility and Participation* (California, 1979).
 7. Gregory Kasza, *The Conscripted Society: Administered Mass Organizations* (Yale, 1995).
 8. Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Beacon Press, 1966). Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge, 1979). John Zysman, *Governments Markets and Growth: Financial Systems and the Politics of Industrial Change* (Cornell, 1983). ♦

Symposium

Data Collection and Fieldwork in Comparative Politics Issues, Incentives, Opportunities

Editor's Introduction

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In this symposium, we take as the theme the raw materials – the “stuff” – of which comparative politics is made. The word data, as Webster’s reminds us, comes from the Latin *datum*, meaning “something given.” But, as we all know, it isn’t. Data must be painstakingly gathered, recorded, sorted, labeled, stored, and periodically sniffed for freshness. Whether numbers in a spreadsheet, jottings in a coffee-stained notebook, or photocopies from an archive, good data are both hard to get and perishable.

In his “Letter from the President” in the Winter 1999 *Newsletter*, David Collier started a conversation on this question that others were eager to join. From a variety of angles, the authors in this issue take up some of the topics he raised. Some tell of opportunities and incentives. Jennifer Widner reports on a new award of the Organized Section in Comparative Politics to recognize the creators of original datasets. David Epstein and Sharyn O’Halloran introduce a cross-national database for comparative politics soon to be launched on the web. Others raise notes of caution. José Cheibub, himself an experienced practitioner in quantitative data-gathering, warns that in this age of “data optimism” we should not forget E.H. Carr’s injunction to “ask questions of our documents” – and suggests that today we should also interrogate our spreadsheets.

Running through several contributions is the central dilemma Collier raised: how the profession should balance the “deep engagement with data” that comes from one- or two-country studies against the opportunities to test generalizations that only a larger number of cases can provide. Eric Hershberg and Kenton Worcester of the SSRC describe results of a review by the Council of its recent pattern of support for different kinds of graduate research. Contrary to the impression of some, they point out, SSRC funding for extended periods of field work abroad has not diminished but increased in recent years. Finally, David Laitin reviews the suddenly data-rich field of post-Soviet politics as it rapidly rejoins the mainstream of comparative politics, and urges scholars not to miss ways in which the varied ferment east of the Elbe can challenge and reshape our thinking about politics in general.

Maintaining Our Knowledge Base

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This year the Comparative Politics section of the APSA has

created a new prize for the best committee members. data set in comparative politics. Many of my colleagues who study developing areas find they are caught in a paradox. At precisely the time universities seek to “internationalize” higher education, several trends undermine the capacities of scholars to serve as independent sources of

information about other regions of the world. The full impact may not reveal itself for several years, but in many departments, mine included, empirical research outside American boundaries is at risk. As a discipline, we need to confront the challenge of maintaining our knowledge base.

It is true that problems have always beset primary data collection in comparative politics. The survey research of the 1960s attracted criticism for its insensitivity to the difficulties of cross-national comparison. Econometric analysis of aggregate data often omitted important variables or used inappropriate proxies for want of necessary information, and produced biased results. The “area studies” counter-revolution also failed to deliver. Ethnographic studies too often collapsed into stories about particular cases, with little potential for providing general insight.

For all of their flaws, each of these intellectual movements sought to improve upon the other – to make up for deficiencies or attend to neglected stages of the research process. What is different about our situation today is that the pressure to abandon the enterprise has started to exceed the incentives to do what we do better.

There are five worrisome changes afoot. Most universities suffer from their effects in some measure, although some may be more prone than others. None of the changes is an unmitigated bad. Each has a positive aspect or offers something we all value as scholars. Indeed, it is this quality that makes it so difficult

to confront the unintended, negative side-effects that undermine our data infrastructure.

One challenge lies in the unhealthy interaction between the enterprise of social science and changes in our tenure and promotion requirements. Inquiry always has several stages. The best scholars are those who demonstrate creativity in identifying interesting questions, building insightful theories, collecting information to test their ideas, analyzing evidence, and communicating their results. They lavish years on their projects, especially when much of the work must take place abroad.

In a world where salaries are tied to annual productivity and time-to-tenure is 5 to 6 years, the incentives work against “doing everything well.” Instead, we focus our attention on the manuscript that we can write quickly at the desk – a think piece, an article that relies on made-up facts (“factoids”), or re-analysis of a stock data set. This institutional context does not lend itself to good writing about other parts of the world. We all need to maintain our productivity. But the annual reward system means that empirical research generally, but especially work in difficult settings, precipitates salary erosion. And the short time to tenure discourages “second projects” that involve serious empirical research. Inevitably, primary data collection in developing areas suffers.

Funding trends also interfere. The existence of preliminary data or prior investigation by other scholars provides a tremendous advantage in the re-

search world. For example, sample size calculations for surveys and a lot of other technical aspects of data collection require information about standard deviations. The only way to get some sense of these is to turn to earlier, related work, which asked identical questions or at least very similar questions. It is possible to do so in the study of American politics and it is often possible in the study of European politics. Elsewhere, no such information exists. That means proposals to fund basic data collection are very likely to fail, if they involve work in a developing country. Review panels rarely reflect on the “chicken-and-egg” problem that confronts us. Without preliminary data, they say, no research support.

This trend has two consequences. One is that scholars are forced back into forms of research funding where the time demands are unpredictable or risky and mesh poorly with academic schedules – bids on USAID projects, for example. The other is that once a region is marginalized by the academic community, it grows ever more so. Breaking out of the vicious cycle depends almost entirely on self-financing of the preliminary work.

Data release requirements can also discourage primary data collection. The rule that researchers should make their data publicly available has great merit. It enables us to assess data quality, replicate results, and build on others’ work. But too often we forget that the amount of time which elapses between collection and release affects the

incentive to invest. In developing country-based research, where scholars often have to sink substantial amounts of their own savings into their work and where data collection usually absorbs all of a year's leave, a quick turn-around time discourages empirical investigation. It is important that exclusive rights run for a reasonable period. That period should take into account the fact that most comparativists have to balance writing with teaching and other term-time obligations, since few can win release time for both data collection and writing.

If the researcher can assert no property rights to the fruits of his or her labor, there is no sense in carrying out difficult empirical work. Quick turn-around times serve only the short-term interests of the users of others' data. They do not serve the community as a whole, over the medium- and long-term.

A fourth unhealthy trend emanates from the abuse of formal modeling. There is no inevitable trade-off between this kind of work and others. Mathematical models can be useful. They provide one way to generate testable hypotheses about the world around us. They allow us to think systematically about a limited range of political behavior, drawing on a few assumptions and "factoids." They are important first-steps in research on some topics. They have other advantages too. They are cheap, compared to field work. Moreover, we can build them at our desks, with all the comforts an American university is able to provide.

Unfortunately, new enthusiasms

often go beyond reasonable bounds, and in the case of formal modeling, the excesses threaten our knowledge base. The problem is an attitude, not a method.

A cartoon in *The Chronicle of Higher Education* captures the most distressing sin. An editor peers avidly at a manuscript, apparently having objected to inaccuracy, while the author waves his hands and says, "You can check facts, but these are factoids!" The search for general insight inevitably trades some measure of accuracy for parsimony, but usually social scientists think it is important to limit the disparity. In the hands of some of our newer formal modelers, that no longer appears to be the case. Replied one job candidate to a challenge this past year, "I don't actually know anything about my cases." One would have thought that would have been enough to sink him, but the young man had defenders: "Facts aren't important. We are political philosophers. It is up to you to test our theories." Others in the candidate list showed similar abandon and cared not at all that the "facts" to which they alluded were pure products of the imagination.

These exchanges are worrisome. Understanding is the product of a delicate interplay between substantive knowledge and abstract theory. Of course it is acceptable to develop a model with reference to what might be an odd or exceptional case, as long as the author tests the theory and then offers appropriate caveats to other members of the research community. But to

abandon any concern for accuracy leads us into irrelevance. The big insights come from a healthy curiosity about where our models fail, not from dogged insistence that others do the work of testing our ideas. Relying on "factoids" in an effort to escape discussion of accuracy is as anti-intellectual as the post-modernist shrug that the meaning of a text or an event is whatever the reader says it is.

To the extent that departments accept the new attitude on the part of modelers, while insisting that others adhere to a broader panoply of quality criteria, the incentives to carry out difficult empirical research diminish. The asymmetry in the significance we accord reasoning errors in theory construction, compared to reasoning errors in evidence, leads us to constitute departments where respect for fact disappears.

The final troublesome trend comes from the publishing world. In the past few years, academic presses have had to become self-supporting. As a result, few now gamble on empirical studies, especially books about parts of the world not at the forefront of American foreign policy interests. A hint of data from Africa, parts of Southeast Asia, or a few other parts of the world is enough to elicit a demand that an author guarantee the purchase of several hundred copies. Without the money up front, the manuscript won't go to the board, no matter how good it is.

As a result of these pressures, the maintenance of our knowledge base resides increas-

ingly in government organizations and in other countries. There is no disagreement that researchers in places like the World Bank and colleagues in other countries *should* be engaged in data collection. But the withdrawal of American universities from the enterprise is problematical. It means that policy will lead scholarship to a much greater degree than it has in the past. Although I worry most often that academics fail to heed what ordinary people care about, I do think we have an important countercyclical function to play in the intellectual world. We will not be able to perform that function if we are wholly dependent on data generated for current policy purposes. Maintenance of projects that generate important time-series data also requires a broader outlook. It demands the willingness to provide a public good in the face of political pressures to do other things. Finally, it is inappropriate that we allow ourselves to become wholly parasitic on the labor of our colleagues abroad.

What should we do to address these problems? We could let data collection in comparative politics lapse until demand drives up the rewards and reverses the incentives. But this strategy will impose high costs. We need to intervene, instead.

The APSA comparative politics data prize, newly created, is a small step to show that the discipline recognizes the important contributions of people who are engaged in data collection. It does not suffice, however. Several other steps are required, in my view.

- Universities, private foundations, the SSRC, and NSF should offer seed money for collection of preliminary data in “marginal” or difficult parts of the world. Without seed money, it is difficult to break out of the vicious cycle that besets research in these regions. Eligibility should be independent of the existence of pre-tests and of sample size calculations based on previous work. It should be a function of having a clear, interesting question, a good research design, a sophisticated grasp of the literature, a favorable track record, and the ability to work with host-country colleagues.
- We need cross-university and cross-regional collaboration to develop and maintain time-series data sets or explicitly comparative data compendia. These are “public goods” – infrastructure projects that generate information used by the discipline as a whole, but impose high individual costs on researchers who organize them.
- University administrators and departments need to think carefully about the ways tenure and promotion standards affect the character of knowledge. Deans may need to counteract the effect of rules and procedures that negatively influence the fortunes of those who engage in empirical

research abroad, compared to others.

- We need to consider whether rules regarding the release of data provide a reasonable amount of time for a researcher to use what s/he has worked hard to assemble. The standard of reasonableness should not be what is acceptable within the boundaries of the United States, but should instead take into account the demands that comparativists face.
- We need to renegotiate relationships with publishers. Possible terms of a deal would include the creation of a fund to help finance book publication on “marginal” areas, with the provision that if sales exceeded a floor level, the publisher would return the money to the common fund. The support would only be available on a competitive basis for manuscripts that had received favorable peer reviews.

Finally, we need to think about drawing mid-career and senior scholars back into primary data collection. Work in remote areas is hard on family life. It is unreasonable to expect a scholar to devote a lifetime to the enterprise. In order to preserve past lessons and improve the quality of our data resources, we need to think about ways to draw colleagues back into this kind of work, after they have “dropped out” for a time. Collaborative, cross-university projects may be one vehicle for d-

ing so. University-administered retooling grants may be another.

Data Optimism in Comparative Politics: The Importance of Being Earnest

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There is a sense among many comparativists that we are now in a position to study statistically many of the questions that have remained unanswered or understudied for lack of comparable, reliable, and consistent data covering a large set of countries. It would be fair to say that we are living through a period of data optimism in comparative politics.

Indeed, there has been an explosion of cross-national studies examining all sorts of relationships; relationships that, only a few years ago, would have been definitely beyond systematic scrutiny for lack of data. A number of studies come to mind, all based on relatively large sets of cross-national data, involving either as dependent or independent variables factors such as political regimes, economic development, corruption, financial repression, property rights, political instability, income inequality, rule of law, human rights, foreign investment, electoral systems, labor relations, leadership turnover, economic liberalization, federalism, and so on. There is a sense that there are no more “can’t do’s” because data do not exist. The increase in organizations producing cross-

national data, the ease with which large data sets can be stored, transmitted, and manipulated would all make the life of comparativists quite easy: all we would need to do would be to think up an interesting puzzle, specify the proper relationships to be examined empirically, find the organizations that produce the relevant data and download the data from their web pages. This, of course, is not, or not yet, how things work. But the increasingly frequent exhortations for coordinating data collection efforts not only implicitly recognize that we are living through a period of unprecedented data abundance; they also suggest that difficulties in getting to the data may be just transitional and that soon enough, we will be able to access at the snap of our fingers the data relevant to testing our hypotheses.

I am of course more than a sympathetic observer of this process, as I am actively engaged in analyzing and producing cross-national data sets. I too share in this data optimism and look forward to the day when data-related constraints will be minimal. But I also think that this optimism may be blinding in some respects. I thus feel compelled to sound a cautionary note regarding the data optimism which I sense among many of my fellow comparativists and suggest that, emulating good historians, we be more critical of our sources. In particular I want to stress the importance of recognizing some of the inherent limitations of many existing and widely used data sets. I also want to suggest that we use what

we learn about their limitations in putting together data bases that can be used in comparative politics.

Let me start with an example that comes from my own work. I have been part of a collaborative project that aims to assess the impact of democracy on economic development (Przeworski, Alvarez, Cheibub and Limongi, 1999). When we started this project, much of the economic data necessary to study this question across a large set of countries over a relatively long period of time had already been made available. But a similar set of political data did not exist. Our first task, therefore, was to generate the data on political regimes that could match the existing economic data. Thus we coded political regimes for the 141 countries between 1950 and 1990 for which comparable data on economic growth were available. We treated as democracies regimes that hold elections in which the opposition has some chance to win and to assume office. In operational terms, a country in a given year was classified as a dictatorship if at least one of the following conditions were true:

- The Chief Executive is not elected.
- The Legislature is not elected.
- There is no more than one party.
- A regime passes the previous three rules, but there has been no alternation in power.

Space does not allow me to

go into the justification of these rules here. Let me point out, however, that the classification that results from their application differs from existing regime variables in at least four respects:

- It covers a large number of countries over a relatively large continuous period of time.
- It is dichotomous, thus deviating from most existing classifications that think of democracy as a continuous feature over all regimes, that is, that assume that one can distinguish the degree of “democracy” for any pair of regimes. Even though we do not disagree that some regimes are more democratic than others, we believe that regimes that meet at least one of the criteria above should not be considered democratic.
- It explicitly distinguishes between systematic and random error. For example, instead of creating “intermediate” categories whenever we were faced with regimes that could not be unambiguously classified by our rules on the basis of all the evidence produced by history, we chose to make “type II errors” whenever we knew we had to err. In other words, whenever a country’s history had not provided the crucial evidence of contestation – alternation in power – we chose to classify as dicta-

torships regimes that could have been real democracies.

- Finally, our classification is strictly based on observables.

It is this last point which I want to emphasize and highlight for discussion here. The value of the regime classification we have produced lies, in part, in the fact that it involves the application of a set of rules that meet two important requirements for proper measurement: first, the application of these rules is not biased by the extent of our knowledge of specific countries; second, the information they require can be potentially obtained by any individual who wishes to apply them to any country at any point in time. In this sense, our classification of political regimes differs drastically from a number of widely used regime variables.

Many of the existing measures of political regime that have been widely used in cross-national comparative research are based on subjective scores, produced by one or more individuals according to criteria that are not entirely apparent for anyone who tries to understand what the measures refer to. In itself this is not necessarily a problem. Most of the existing measures of democracy, objective or subjective, are highly correlated. Yet, the correlation is not perfect. And it is in the incongruence across measures that we can see how important it is to rely on objective criteria of observation when producing data for use in comparative research. After all,

no measures of political democracy are likely to produce very different readings for, say, England, the United States, Sweden, North Korea, or Iraq. The problem arises with “difficult” cases such as Mexico, Botswana, Malaysia, Peru, Guatemala, and scores of other countries whose institutions are sufficiently ambiguous in their operation to make us wonder whether what we see is really what we think it is. These are the cases that require explicit criteria of observation. With such criteria we can evaluate the decisions made by those who produced the variable and change them if we so desire. Without them, we may never know what exactly is being measured.

Unfortunately this problem is not confined to measuring political regimes. One widely used measure of “property rights security” and “contract enforcement” (two variables that have assumed exceptional theoretical relevance in recent work on political economy), is based on the judgement of experts who are asked to locate countries on numerical scales referring to topics as varied and as intangible as the quality of the bureaucracy, degree of corruption in government, nationalization potential and level of contract enforceability. More recently, some analysts have used an index of corruption that is a composite of polls conducted by different organizations which, in a variety of ways, “assess” the extent of corruption in a given country.

Note that the point is not that measures based on observables are not subject to bias, whereas

measures based on subjective judgements are. Both kinds of measures may be biased. The point is that one is subject to more and different kinds of bias. There is an obvious bias that comes from reporting, and that affects even the most objective and strictly observable measure. Consider, for example, that according to one widely used measure of "political unrest" – the number of riots and anti-government demonstrations in a year – the US was by far the most unstable country in the world in the 1960s. But this is the kind of bias that the "information revolution" may help reduce in very significant ways: we can expect that whatever bias is introduced in the data due to reporting will be reduced as the notion of "remote" countries about which nothing is known becomes a thing of the past, and raw information about a greater number of countries is increasingly made available to researchers. Subjective measures compound the reporting bias since they take information that is likely to be skewed and generate numeric scores based on criteria that are far from explicit.

Thus, measures based on observables are to be preferred over measures based on "expert judgments." Yet, succumbing to the latter's allure is quite easy. For one, the temptation is enormous. Many of the recently used cross-national data sets provide information for a number of countries, often over time, on just that theoretically important aspect that we always wanted to know about: an index of corruption, measures of the security of

property rights, scores on the extent to which a country abides by the "rule of law," scales of economic freedom, indices of civil society participation, levels of political instability, and so on. Faced with a spreadsheet full of "data," it becomes just too easy for us to forget that these numbers are likely to reflect the idiosyncrasies, even if "expert" idiosyncrasies, of some individuals.

Second, the cost of collecting and organizing cross-national data on political phenomena based on observable criteria is very high. Sometimes this is so because the phenomenon is just too difficult to observe, corruption being perhaps the best example. But sometimes we simply lack the data, even on the most basic, observable, uncontroversial political events such as elections and incumbency. Let me give an example to make this more concrete.

Adam Przeworski and I have studied the relationship between elections, economic performance and the survival in office of democratic and authoritarian leaders. In view of democratic theory and a large empirical literature, we expected to find that the survival of presidents and prime ministers in office would be affected by economic performance, whereas the survival of dictators would not. Our findings were very surprising. We found that while the survival of democratic leaders in office is hardly affected by economic performance, the survival in office of "bureaucrats," leaders in dictatorships that have an elected legislature, is strongly influenced by the growth of per cap-

ita income, the growth of consumption and the growth of government spending (Cheibub and Przeworski 1999). Thus, the relationship between economic performance and the survival of the government that we would expect to find in democratic regimes was, instead, observed in non-democratic regimes.

There are several factors that can account for these findings, some of them already suggested by the data available. We found, for example, that over half of the time, prime ministers in parliamentary regimes are removed from office not by elections, but by intra-party struggles or the collapse of the ruling coalition. We also found that while the 86 presidential elections we observed led to the departure of 66 presidents, 53 of these departures were necessitated by term limits: voters could not have re-elected the president if they had wanted to. It is possible, therefore, that specific institutional features of democratic regimes affect the role that elections can play in promoting government accountability.

As for "bureaucrats," it is possible that the connection between economic performance and their survival in office is mediated by voting turnout. To be sure, very few authoritarian leaders left office because they lost elections. However, elections may matter for the survival of dictators to the extent that the turnout reveals something about the regime's degree of popularity. Whereas high turnout rates are often celebrated by the regime as an indication of its ability to control and mobilize the

population, low turnout rates are cause for concern since they may indicate the current leader's weakness, thus stimulating actions by rival factions within the regime. If turnout is itself a function of economic conditions, we can see how economic performance may affect the survival of dictators in office: when the economy is bad, turnout is low and the chance that the dictator will be removed from power by some rival faction increases; when the economy is good, turnout is high and dictators are more secure in power.

It is clear that we need a lot more data – and very basic data at that – than is now readily available to test these conjectures. For one, we need to know more about elections, in particular those that took place in dictatorships and in non-OECD countries. Just to give an idea of the magnitude of what is entailed, between 1950 and 1990, there were 246 presidential elections and 1,012 legislative elections in 141 countries, of which 134 and 534, respectively, were held under dictatorships, and 226 and 725 in non-OECD countries. Moreover, we need to be able to answer basic questions about how democratic regimes operate: What is the frequency with which incumbents in democratic regimes lose power because of elections? What is the relationship between the incumbent's vote loss and the incumbent's loss of power in these regimes? Are there institutions that modify the impact of vote loss on incumbency? Do they affect incumbents' loss of votes in the same way that they affect their

loss of power? This, of course, entails assembling data on each and every election that took place, say since 1945, on the governments that existed since then, and on the constitutional features of the regimes under which elections took place and governments existed. This is very basic political information. It is amazing, but true, that such a data set does not yet exist.

Producing such a data set, however, requires more than the effort of one or a few individuals, particularly in view of the pressure to publish that academics face today. Under these circumstances, data collection and organization are likely to remain incomplete and narrowly focused, as they are shaped by the concerns of one specific project that one researcher happens to be working on at the moment.

That we need more comparative political data is uncontroversial. What I would like to suggest is that there is a large collective payoff in coordinating efforts to collect very basic facts about political institutions in all sorts of countries before we move to complex classificatory schemes, or attempt to directly observe the more abstract effects that our theories suggest. This means that the range of questions we will be able to ask is likely to remain limited for some time, which may be disheartening for hard-core "data optimists." However, there are so many basic, factual questions that we cannot now adequately study for lack of systematic data that I believe we will have a full agenda for the years to come if we concentrate our efforts on

producing and organizing such data.

While we are all eager to take advantage of the several new cross-national data sets that have recently been made available, we should thus approach them with a critical eye. E. H. Carr's injunction that historians should ask questions of their documents – questions about who produced them and in what context – also applies to political scientists working with cross-national data sets. Many of the existing and widely used data sets reveal a lot about those who produced them. As interesting as this may be, however, it is not the primary concern of comparative political research.

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Trends in Funding for Graduate Student Field Research in Comparative Politics: Evidence from a Review of Social Science Research Council Fellowship Programs

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There has been much discussion among political scientists in recent years concerning the relative importance of language proficiency and field work for graduate students engaged in research outside the United States. The conversation highlights divergent views about the relationship of area expertise derived from field research abroad to conceptual innovation in social science, and has profound implications for the training of junior scholars aspiring to careers in comparative politics. Prominent voices in the profession have noted that the strong emphasis of some currents in contemporary political science on sophisticated formal methods and on the acquisition of mathematical and statistical skills needed to carry out large-*n* comparisons may discourage even the best departments from requiring that doctoral students take the time needed to develop language fluency and in-depth cultural and historical understanding of specific research sites (Bates 1997). The implication, celebrated by some of our colleagues and denounced by others, is that area studies knowledge may no longer be essential to the production of innovative comparative research. And if this is the case, field work itself may be deemed a dispensable component of the skill pack-

age traditionally expected of graduate students emerging from top tier political science departments.

The evolution of programs sponsored by the Social Science Research Council (SSRC) has been invoked by political scientists and other scholars as indirect evidence in support of a variety of reflections on the continued relevance of area studies expertise (e.g. Bates 1997, Collier 1999). Several factors are undoubtedly at work in making the Council a key referent in the debate. Although the SSRC is hardly the only organization that offers support for graduate student field research abroad – the Fulbright-Hayes program, the National Science Foundation, the United States Institute of Peace and the Institute on World Politics also play significant roles – the Council has been providing graduate fellowships since the 1920s and remains one of the most visible sources of funding for internationally-oriented research. In addition, the extremely competitive nature of most SSRC fellowship programs arguably has enhanced the prestige of its awards. Perhaps most importantly, the eleven area committees administered jointly by the SSRC and its counterpart organization in the humanities, the American Council of Learned Societies (ACLS), were disbanded in 1996 as part of a broader restructuring of the Council's international programs. This measure, which was widely – and appropriately – perceived in the context of a broader questioning of the role of area studies programs in

American higher education during the post-Cold War era, captured the attention of scholars concerned about the future shape of area and international studies and about likely trends in funding for student and faculty research.

This article presents results of a staff review of trends in graduate fellowship support at the Council, focusing on programs of special interest to students of comparative politics. The underlying objective of the review was to inform the ongoing discussion among our colleagues by providing empirical information about what the SSRC actually has been doing with regard to graduate training. The specific goals were to determine whether funding for international field research had retained its prominence in Council programs, to identify trends in the relative weight of political scientists in the pool of grantees, and to see whether political scientists and other fellows were increasingly open to comparative research. We also saw the exercise as a timely opportunity to consider whether our international fellowship programs seem to be developing in line with the goals that motivated the mid-1990s reorganization.

Our analysis drew on data from all SSRC programs that have offered fellowship support since 1983 for international work at both the pre-dissertation and dissertation levels. (For information on the full range of fellowships and grants currently offered by the Council, please consult our web site, <http://www.ssrc.org>). We included in our

review only those programs that offer support to political scientists conducting internationally-oriented work. In addition to data on dissertation field research grants provided by area committees between 1983 and 1996, our assessment encompassed region-specific programs that continued to fund field research after the latter date (on the former Soviet Union, the Near and Middle East, Japan, Bangladesh, Vietnam and Germany), as well as the International Peace and Security Program (IPS) and two programs, the International Predissertation Fellowship Program (IPFP) and the International Field Research Fellowship Program (IDRF), that began offering awards in 1991 and 1997, respectively.

It is worth pausing briefly to sketch the principal features and objectives of the latter two programs. Funded by the Ford Foundation, the IPFP aims to encourage the best graduate students in the social sciences (especially economics, political science, and sociology) to develop both disciplinary expertise and sophisticated understanding of developing country contexts prior to admission to candidacy. The IDRF program, in turn, targets doctoral candidates in all disciplines of the social sciences and the humanities whose proposals for dissertation research outside the United States hold exceptional promise of combining theoretical innovation with close attention to the specific conditions of particular settings. Supported by Mellon Foundation grants to the Council, the IDRF awards in effect consti-

tuted a replacement for dissertation support previously allocated in a decentralized manner by the individual area committees. Both the IPFP and the IDRF typically require program fellows to spend between nine and twelve months conducting field research abroad, and explicitly encourage comparative research. Both programs are also quite large by Council standards, with the IPFP averaging approximately 30 awards per year and the IDRF offering full fellowships to nearly 150 students during the first three years of its existence.¹

Our inquiry yielded persuasive findings pertaining to two of our three central questions. First, as David Collier suggested recently in the pages of this newsletter, rather than diminishing, SSRC support for extended periods of field research abroad has increased in recent years. Whereas the thirteen years spanning the 1983 to 1995 competitions saw the relevant programs provide 1087 awards for dissertation research, or just over 83 awards per year, provisional data show a 20 per cent increase (a total of 301) in the number of dissertation field research fellowships awarded during the three competitions following the 1996 program reorganization. While we would like to have tracked the evolution of stipend levels, it has not been possible to do so with any precision, since different programs have employed a variety of mechanisms to calculate award levels at different points in time. We are persuaded, however, that these numbers would reinforce our findings: stipends provided to

IDRF recipients are nearly double those allocated to dissertation fellows by the majority of area committees during the first half of the 1990s, when the monetary value of those grants had been declining steadily, in absolute as well as real terms, for more than a decade. Further support for our conclusion that there has been a marked increase in support for area and international research is provided by the IPFP, which since its inception less than a decade ago has provided substantial field work experience to nearly 300 graduate students in the social sciences.

A second important finding is that while some programs have invested in political science research more extensively than others, the overall place of the discipline in the international program has remained strikingly constant over the past decade and a half, both within specific programs and across the Council. Results of area-based dissertation competitions through 1995 mirror those of the first three years of the IDRFs and illustrate a pattern evident across the entire program portfolio: political scientists received 16.2 per cent of the grants provided between 1983-1995, and have merited 17.7 per cent of IDRF grants awarded to date. By contrast, roughly one fourth of IPFP fellowships have gone to political scientists, an outcome that is not surprising since the program is restricted to the social sciences and its promotional material explicitly encourages applications from the discipline. Similarly, it is not uncommon for as many as

half of the handful of International Peace and Security fellows to be drawn from political science departments. Overall, political science steadily ranks third among disciplines receiving support for international field work, behind history and anthropology, and slightly above sociology.

The third question that motivated our review of Council fellowship programs was whether there had been any significant changes in support for comparative research. Our data permit us no more than tentative conclusions in this regard. We limited our search to a review of project titles, and many studies that employ the comparative method to investigate multiple locations within particular countries or to compare single sites over time will inevitably have been overlooked. Nor is it likely that every fellow who initiates a comparative project will in the end manage to carry out research in each of the locations they anticipate at the outset. Indeed, it is to be expected that fellows will revise their research plans along the way, and over the years we have both encountered cases in which the mid-course corrections of dissertation level grantees have involved reducing the number of cases to be studied in depth, particularly where those cases spanned boundaries of different countries or world regions.

Despite these caveats, we were able to establish that the numbers of fellows who proposed projects that entail field research in two or more countries or regions has risen considerably since the inception of the

IDRF program. Approximately one in six fellows has proposed to carry out field work in more than one country, whereas the ratio never exceeded one in ten prior to the reorganization of the international program in 1996.

Interestingly, even the area-based fellowship programs have provided support to comparative projects during this period, and the phenomenon is especially noteworthy in political science. For example, of the five political science applications funded by the Berlin Program in 1997-99, four requested support for cross-national research, including one that compared processes of democratic consolidation in Russia and the former Eastern Germany, and another that looked at partisan politics and foreign policy making in the European Union. In the case of the Near and Middle East program, four out of eight political science proposals between 1996 and 1998 required multi-nation field work. These projects examined issues such as policy-making in Syria and Jordan; state-building in Jordan and Yemen; and pan-Arab politics in Tunisia and Morocco. Of seven awards offered to political scientists in 1996-1998 by the Title VIII program, six were comparative in nature – with projects studying topics ranging from regulation of property rights in Russia and the Czech Republic to legislative-executive relations in post-Communist states and revenue sharing arrangements in the Russian Federation. The interest of the current generation of political science in cross country comparisons is evident among IDRF recipients as well.

In 1999, for example, two of the five political science fellowships were awarded for cross-national research (on unemployment in Europe, and presidential impeachments in Latin America). Two years earlier, seven of the ten fellowships awarded to political scientists consisted of cross-national projects on such topics as inter-generational conflict in Italy, Spain, and the Netherlands; the legacy of British rule for Northern Ireland and Palestine; and the role of multilateral development banks in East-Central Europe.

Finally, we set out to explore whether trends we identified would square with the vision of social science that motivated the reorganization of the international program three years ago. Contrary to some of the most dire predictions, the decision to replace a set of institutional structures that privileged geographic regions over other mechanisms of scholarly cooperation by no means implied an abandonment of the Councils' long standing commitment to what then-Council President Kenneth Prewitt referred to at the time as "place-based knowledge." The distinguishing feature of SSRC international fellowship programs continues to be provision of funds for extensive periods of field research by well-prepared graduate students with superior training in their disciplines and with a knack for posing important questions in ways that engage colleagues in other fields. If anything, the increase in the number of such fellowships testifies to the degree to which area scholarship is as

central to the Council today than at any time in the recent past.

At the same time, we are cautiously optimistic about the willingness of the current generation of comparativists to accept the difficult but potentially rewarding challenge of conducting field research in multiple settings. To be sure, there is a risk that students will gloss over the complexities of the contexts in which they are working and produce scholarship that overlooks the crucial nuances that define the ways in which local conditions interact with the wider universe of relationships in which they are embedded. But it is also entirely possible that highly trained young social scientists will more often than not be up to the challenge of combining deep understandings of particular places with the ability to situate those places in a broader international context. The questions confronting humankind at this moment in its history call for precisely that ability to move across multiple levels of analysis, to probe beneath banal generality and beyond esoteric details. Properly grounded in a sharp eye for things local and a keen sensitivity to their interactions with processes unfolding elsewhere, comparative research can be an invaluable strategy for comprehending the puzzles that make up the human condition and that preoccupy the social scientists we hope to be able to support.

Notes

1. The remaining programs included in our analysis are all more modest in scale, if not am-

bitious. Several offer between eight and twelve fellowships per year but some (e.g. for field work in Bangladesh) have provided as few as two grants in a given year.

2. At this writing (June), a handful of fellows selected during the spring of 1999 have not yet formally accepted grants, and it is not certain that alternates would be selected in all instances.

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Announcing A New Comparative Political Institutions Web Database

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Let's say you have an interesting idea for a paper on comparative politics, and you know (or suspect) that the data needed to test your theory exist somewhere, but you don't know exactly where. Even if you could locate the data, downloading it, finding a codebook to explain it, and converting it to a useful format all require so much energy

as to often make the entire enterprise more costly than it would be worth. Thus die many potentially fruitful research exercises.

Many excellent data sets on comparative political institutions exist now, but the individual researcher is fighting an uphill battle trying to locate and use them. We hope to change this unfortunate situation by harnessing the power and transparency of the internet. Together with Robert Bates of Harvard University, and thanks to generous funding from the World Bank, the Harvard Center for International Development, the Bechtel Corporation, and the Stanford Institute for the Quantitative Study of Society, we are currently creating a web-based comparative politics database that will be freely accessible to all members of the academic and non-profit communities.

The idea is simple: you will hit the web site; select which variables you want, for which countries and which years, and what format you would like the results in: an Excel spreadsheet, a comma- or tab-delimited ASCII file, a SAS, SPSS, or Stata data set, etc. The requested variables might come from a single original source, or represent a combination of variables from a number of different sets. A custom-made data set will then be created for you, along with a codebook, available for downloading to your hard disk to analyze at your leisure. The focus of the web site will be on comparative political data, but it will also contain common economic indicators – GNP, inflation, imports, exports, unemployment, and so on – as well as some basic demographic statistics

on population, education, and health.

Our enterprise is guided by three principles. First is the importance of easily and publicly available data for the research enterprise. American politics, for instance, has many of the highest quality data sets in the social sciences: every vote ever taken in Congress, every committee assignment in the House and Senate, all election results, campaign finance, interest group rating scores, and so on. Yet research is often hampered by the fact that many of these data sets are hard to access; they are held in proprietary formats through large, centralized distribution services, so that the compilation of even the simplest subset can be a frustrating experience. Our site will be openly available to researchers all over the world via the web, with an intuitive interface that will make the downloading of data as simple as possible.

Our second principle is that of cumulative, continually improving, data. Even the best data sets are not perfect; they contain errors and omissions, miscodings and transcription errors. Typically, the researcher will download a major data set to his or her own computer and clean it up to some extent, so that a slightly improved version of the data set exists on their own hard drive. Other researchers replicate this process on their own computers, but their efforts are in no way cumulative. Our site will include a feedback mechanism through which users can suggest corrections and additions to data sets published in our database. We will review these sugges-

tions, and when enough have accumulated, we will update our site to reflect them. Thus a centralized, single-source best version of the data sets will exist on our site, with the data being constantly refined and improved. In addition, older versions of the web site will be archived; the idea is that users can report that their article used data from the Comparative Politics Web Database Version 1.4, for instance, and then anyone else interested in replicating their results can download the data from that version and re-run their analysis.

Closely linked to this is our third principle, which is the desire to build a virtual community of researchers dealing with issues of comparative political institutions. Associated with the web site will be a threaded news discussion listserver, so that researchers world-wide can discuss issues related to the definition, collection, and refining of data sets. Those with data sets near completion, for instance, but who find the last pieces of missing data hard to fill in, will be encouraged to submit their data to the site as is to see if anyone else has access to the missing information.

The web site described here is currently under construction, and it should be available by the end of the summer; watch for announcements. In the meantime, the project home page at <http://ksgwww.harvard.edu/CID/Politica.htm> has links to a number of data sets, including a unique Africa data set created by Robert Bates. We are also soliciting original data sets of the form variable-country-year to

publish in our database. As an incentive, all data will be accompanied by a citation to the work in which it was first published, and users of the data will be required to cite these sources if they produce analysis using the data.

Post-Soviet Area Studies

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The editors of the newly created *Annual Review of Political Science* invited me to write a review essay on a sub-field of political science of my choice. Having become an autodidact in the field of Soviet and post-Soviet studies, and having a strong intuition that the battle between “area studies” and “positive theory” was framed by caricatures, I chose to examine the domain of “post-Soviet politics.” The full essay will appear in volume 3, which will come out in the 1999-2000 academic year. In it, I first discuss an insight that generated an experimental course I taught with Stephen Holmes – that the collapse of Weimar thoroughly recast the agenda of social science (e.g. concern for the authoritarian personality, the rise of the behavioral revolution), and that the collapse of the Soviet Union, equal in historical significance to the collapse of Weimar, is likely to have similar effects. Examining the detritus of the Soviet Union might give us clues, we surmised, about the future substantive concerns of our discipline. I then review contributions made in the past decade, using data from countries of the former Soviet Union (but mostly Russia) for empirical support, on a range of big topics: democracy, state and revolution, the nation, the political foundations of economic growth, federalism, and foreign policy. I find that a new

generation of field workers is carefully attuned to the major questions being asked at the heart of the discipline, and many of the findings present important challenges to standard theories. I reproduce for this *Newsletter* a draft of the final two sections, without the bibliography. First, I review some of the studies that enrich political science in large part due to their sensitivity to historical and cultural context. Second, I sum up my findings with some suggestions, based on the Weimar challenge, as to where post-Soviet studies might make even more important contributions to political science.

Contextual Research in the Post-Soviet Era

Several long-term students of Soviet politics have expressed worry over the all-too-rapid entry into “their” field of social scientists who are comparative in orientation and ignorant of Russian and Soviet legacies. Meyer (1994, p. 191), for example, rails against “the new breed of opinion surveyors and statisticians currently entering post-Soviet studies [who] arrive with no knowledge of Russian history, culture, literature and language.” These researchers, Meyer contends, miss the historical patterns of “anti-Western westernization,” elite attitudes towards the “dark” masses, “the antagonism between the intelligentsia and the *meshchanstvo* [petit bourgeoisie, implying philistines]” and “the frontier spirit of Siberia.” Also missed, he notes, is the legitimating myth of the WWII trauma collectively suffered.¹ This criticism is unfair

to Burawoy and Laitin (both of whom learned the language and lived in Russian society) and to Ordeshook and Brady (who collaborated with area scholars) as well. But there is more than a grain of truth in the claims of long-term Sovietologists that the work of theoretically attuned area specialists must not be ignored by scholars who want to exploit the possibilities of testing universal theories with data from post-Soviet republics.

There are four types of discipline-enriching material that are provided by area specialists. First, area specialists are especially attuned to legacies, as Meyer (1994) points out, and these legacies block or divert political processes from the directions predicted by universal theory. Political science ought not to ignore the rather extended period of transition before the values on outcome variables are commensurate with the predictions on the effects of institutional change. The institutional legacies diverting outcomes from their predicted directions need to be accounted for, and the information in area based scholarship is crucial for such analysis. Or legacies may help explain equilibrium selection when models allow for multiple equilibria. Jowitt (1992, p. 285) insists, “Whatever the results of the current turmoil in Eastern Europe, one thing is clear: the new institutional patterns will be shaped by the ‘inheritance’ and legacy of forty years of Leninist rule.”² Leninist rule, according to Jowitt, reinforced salient features of traditional culture, such as the rigid dichotomization of

the official (seen negatively) and the private (seen insularly) realms, leading to a political culture dominated by dissimulation and rumor-mongering. It also created autarchic collectives that fragmented rather than integrated society, such that the members of each collective have no regard for the life situation of those belonging to other collectives – thus there is no cultural support for tolerance of others' plights. These Leninist legacies are likely, Jowitt contends, to confront the Civic Forums and other democratic and liberal organizations in Eastern Europe with "anti-civic, anti-secular, anti-individual forces outside and inside itself" (p. 304). Other political scientists have stipulated the effects of historical legacies in a less grandiose way. Hendley (1997) shows how legacies of Soviet law, in which top-down regulations were invariably decreed to fulfill interests of those in the center, made shareholders of post-Soviet firms incredulous that the cumulative voting mechanisms required by the joint-stock law of 1995 were written to serve their interests. More broadly, Hendley finds enterprise directors appealing to personal networks in Moscow for support when they face conflicts with outside firms, rather than on the courts, in large part because this was more or less their mode of operation in the Soviet period. Crowley (1997, p. 187) has examined the strategic moves of Soviet and post-Soviet miners in both the Donbass and Kuzbass. Although sensitive to large arenas where strategy was determinative of action, in other

areas ideological legacies structured choice. In seeking to explain miners' flip-flop from an embrace of the market (even when, especially for the Donbass miners, a move toward the market was a step towards redundancy) to an embrace of the Communist Party in the mid-1990s, he reexamines Soviet rhetoric. This rhetorical legacy, he argues, left the miners not only without an alternative to capitalism (other than communism, which they supported), but without an alternative within capitalism (such as social democracy). The miners, he concludes, had "no institutional channel to express their grievances in the political realm" other than as communists. The Manichaeon world view – capitalism or communism – was for the miners the whole choice set. "While miners everywhere are given to radicalism," Crowley concludes, "the direction their radicalism takes is underdetermined," (p. 189) and in part determined by ideological legacies. Bahry and Way (1994, p. 352) examine participation among the Russian electorate, and controlling for a variety of factors, they show that the old and poor are far more likely to vote than the well-to-do. They attribute this in part to the Soviet electoral legacy since "The residues of Soviet mobilization ... have an impact on all forms of conventional activity; but they seem to be especially pronounced for voting, the most ritualized form of Soviet participation. Soviet elections may have been designed as vehicles for legitimating the status quo, but they appear ulti-

mately to have given older citizens a habit that has become a powerful political weapon." Beissinger (1995) shows how the legacy of "empire" continues to drive the foreign policy thinking both of Russia and of the now-independent but once Union Republics of the USSR. Burawoy and Krotov (1992), based on observations of the Soviet wood industry in summer, 1991 (Polar Furniture), hypothesized on likely Soviet legacies and argued that most economists were "underestim[ing] the capacity of the Soviet economy to reproduce itself and resist transformation." With the opening to the market, a regional conglomerate parastatal, the NTWO (Northern Territories' Wood Association) emerged with the goal to connect firms that had supply networks with each other. In a sense, Burawoy and Krotov report, it replaced the party state as the mechanism to reduce anarchy in the relations of production. The real profit within the system, however, was in controlling the barter and other intra-industry trade networks, and to have a monopoly over those networks. The pursuit of profit through trade and monopoly, they argue, would continue to result in poor rates of production. Opening up markets, given the Soviet legacy, has not opened up competition. McAuley (1997) has a keen eye for Soviet legacies, and has a strategy for finding them. Once you leave Moscow where elites have an incentive to hide their ingrained Soviet practices, she reports, they are easy to detect. In Naberezhnye Chelny, home

of the KamAZ auto plant in Tatarstan, she examined a by-election to the Supreme Soviet, and saw the electoral material as almost a satire on Soviet-style electioneering, except that they were serious. "New constitutional rules on the separation of powers and democratic electoral procedures," she concludes, "not only failed to dislodge the incumbents but also allowed them to secure their position as patrons." (pp. 91-108) In fact, the old elite in the republics created an even stronger than before executive presence, marginalizing the legislature and marginalizing the nationalists. Elsewhere (in ch. 4), she studies electoral dynamics in Krasnodar krai, and finds the old division of reds and experts dividing the elite, as the grounds of political battle hardly changed from the Soviet period. These legacies – at least for some period – constrain the workings of newly created institutional incentives.

Second, area specialists provide iconic narratives of general political processes that have been more globally theorized. This work not only gives flesh to skeletal theories, but also provides information on the mechanisms that translate values on independent variables to values on dependent variables. Breslauer and Dale (1997) provide new flesh to the Hobsbawm and Ranger notion that traditions are invented, as they trace the rhetoric of Russian "state" and "nation" from the late Gorbachev period to 1995. While Yeltsin's idea of a de-ethnicized Russian nation conjoined to a powerful Russian state remained stable for

much of this period, Breslauer and Dale show how the changes in the political opposition (the nationalists and communists had purged their own radicals, and were seeking the votes of the median voter) pushed Yeltsin towards the articulation of a new tradition which entailed the invention of a glorified Russian national history.

Third, area specialists can undermine the very foundations of comparative analysis by showing either that the structure of situations, the principal actors, or the goals of these actors are not as postulated by the generalists. For example, the notion that the Russian party system is fragmented due to the early calling of the founding election, or due to a coordination problem faced by party entrepreneurs living in the same Downsian neighborhood, may make for sharp theory. But these explanations, according to Hough (forthcoming), are misguided. He provides evidence that Yeltsin *paid* for minor parties, enriching their entrepreneurs, in order to siphon off votes from any united opposition. Hough similarly seeks to discredit theories that seek reasons why MPs in the Duma do not win elections based upon the resource situation of parties, which is presumably so weak that they are unable to produce coherent candidate lists. Rather, Hough argues, MPs seek not to maximize reelection (as political scientists educated by Mayhew's work automatically assume), but the chances of getting a job in the presidential administration where they can sell licenses or

reap benefits from graft. Hendley (1997) too examines Russian strategic logic from the ground. General directors of enterprises, she shows, rely on privately retained "contract enforcers" rather than courts to settle inter-firm conflicts, even if the latter will allow for a wider range of low transaction-cost contracts. However, she finds, relying on the law would require the general director to cede internal authority to the firm's legal division, while reliance on contract enforcers assures the director of uncompromised control over the firm. The strategic game here is not between firms seeking to lower transaction costs (as the new institutionalism, which Hendley calls in this context the "Development Argument," associated with Boycko and Shleifer 1995), but within firms with General Directors seeking to marginalize their firms' newly created legal departments. Woodruff (1999) has also made a broadside against too-early modeling of post-Soviet politics. Instead of modeling the game of "market reform," Woodruff argues, analysts should have examined the prior strategic situation, that of "monetary consolidation" (pp. 67-68). Woodruff changes the focus from issues of distribution and allocation to the issues of rule. In the literature on reform, analysts rely upon a "rational expectations" model where a reformist government seeks ways to commit to austerity, such that private actors condition their behavior on the expectation of non-inflation. Yet, Woodruff argues, the actual situation is one

of a central government too weak to block local creation of alternate means of payment. To fill in a vacuum, local governments in Russia promoted non-monetary exchange (barter) to protect industry and maintain critical services. While the central government was strategically fighting a battle for the monopoly rights to issue money, Woodruff charges, American political scientists were interpreting their behavior as if they were seeking to promote liberal reforms. Work of this nature is crucial to keep theorists modeling what is actually going on rather than what would be theoretically interesting if it were going on. Area specialists have an eye for detail, and that as Darwin has taught us, is where truth lies. To be sure, area specialists are sometimes too lost in detail. McAuley's (1997) descriptions of the difficulty for Shamiev, the governor of Tartarstan, to sleep one particular night may be excessive in detail. But without a commitment to the details of political life, our models are too easily unhooked from political reality. Theorists who ignore this literature, relying on off-the-shelf models rather than quandaries that arise from detailed field observations, are losing a great opportunity for connecting their work to what is perhaps the most significant political transformation of our time.

Fourth, going back to the Weimar analogy, a focus on post-Soviet life in all its gory details should compel social scientists to rethink their agendas concerning which political questions are worthy of our attention.

For Plato the primary question was that of justice; for Hobbes, that of order; for Tocqueville, that of democracy; for post-WWII behavioralists, that of totalitarianism. In the final section, I will address what the collapse of the Soviet Union means for our future agenda as political scientists. But my point in this section is that the Soviet collapse invites us to go beyond extending our theories; it demands that we ask new questions, and area specialists provide clues as to what those questions might be.

Area studies defenders, as is the case with Meyer, are often too harsh in their critiques of the unwashed interlopers. But, as we have seen, the work of specialists cannot be written off as Gorbachev did his *nomenklatura* – making them all guilty of old-thinking, and seeking to bypass them to bring fundamental change. If political science does to area specialists what Gorbachev did for the *nomenklatura*, our discipline too will be inviting encounters with unreality.

Conclusion

The field of post-Soviet political science has been reseeded, and the early yield has been impressive indeed. A new generation of scholars has combined field work with theoretical concerns driving that work. Political scientists who have been credentialled in other fields have moved into Soviet studies bringing new methods and perspectives. And senior scholars whose careers were forged during the Soviet period have played an important role in adapting new methods

while at the same time speaking to theory in their countries of specialty. While the field of comparative politics is nowadays portrayed as a battlefield between “area studies” and “theory,” in the post-Soviet field the tensions (combining modern methods with field observations) are most often within the framework of each particular study (and therefore productive) rather than in wars of maneuver between groups of scholars representing opposing camps (and therefore destructive). This excursion into a field that is territorially defined demonstrates that portraits of the comparative field are too often caricatures.³

Data collected from the FSU have provided important amendments to partially established theory. Institutions do not spontaneously arise to protect property once traders are permitted to flourish. People who are more highly educated do not always vote with greater probability than those who are less highly educated. Federations that are based on ethnic regions do not inexorably seek greater autonomy from the center, until the center collapses. Revolutions do not always yield strengthened states. These findings are not so weighty as to knock established theory out of the water (but such findings, however strong, seem never to have that effect on any social science theory). Rather, these findings compel students of markets, of voting, of federations, and of revolution to narrow the range of conditions in which their theories have explanatory value. Setting the limiting conditions in which rela-

tionships will hold is an important part of science, and post-Soviet area studies has performed that task well.

But the post-Soviet field has made additional contributions, even if not as boldly as it might. Observation of the basic trends of the Soviet collapse has reordered the questions that have long stood on comparativists' agendas. Fermat's greatest contribution to mathematics, after all, was a conundrum that countless generations of number theorists could not resolve. Riker's challenge concerning the stability of congressional rules, for which he had no answer, has inspired a generation of exciting research. Fermat and Riker should provide a lesson for comparative politics. Observing the detritus of the Soviet Union in the 1990s – in which our entire political landscape has been altered – should inspire us to pose big questions for which the wider field of political science has no answer. Here is where we can return to the Weimar analogy, with two conjectures.

The first conjecture has to do with liberalism and rights. Holmes and Sunstein (1999) have suggested that until recently liberal theorists considered mostly the *benefits* of rights. The Soviet collapse and the Russian transition compelled them to ask new questions for liberalism about the *costs* of rights. If in the Soviet period citizens could not hope to get treated fairly by the law for lack of constitutional protections, in the post-Soviet period (as Solomon 1995, p. 98 details) citizens may not get justice because

courts lack heating oil. The institutional construction of the most basic public goods, merely a theoretic fantasy of the new institutionalists in the 1980s, has become a dominant theme in post-Soviet comparative politics. And so, when the dominant "other" for the US was Soviet *totalitarianism*, liberalism for American social scientists was equated with the benefits of rights to all citizens; but when the dominant "other" is Russian *anarchy*, liberalism becomes equated with the capacity to provide rights. The dominant "other" sets the agenda for the very framing of research on liberalism. The Soviet collapse has pushed leading liberal theorists to begin new work on an aspect of liberalism previously ignored, and it should be an invitation for other liberal theorists to develop the connection between liberalism and paying the cost of rights.

A second paradigm-shifting perspective that is driven home by the collapse of Soviet communism is one that has diverted our attention away from "institutionalization" toward that of "equilibrium". In the Soviet period, it was common to describe Leninist organization as highly institutionalized and therefore stable (Huntington 1968), yet examination of these very institutions in the late 1980s showed that they could disappear as if they almost never existed (Solnick 1998). Or another example: in the Soviet period, because of the "nativization" campaigns (reinforced by Soviet passports), all citizens were coded in terms

of their nationality. Soviet maps could specify precisely the number of each nationality in every district. Yet nationality was understood as a cultural but not a political form. An institutionalized outcome in which all people were members of a nationality, but in which nationality did not matter politically was thought to be stable as well. By the late 1980s, however, nationality rather suddenly became salient politically for many Soviet citizens, and for many others, their nationality was changeable and ambiguous (Laitin 1998). Institutional and cultural practices are in part sustained by coordination dynamics. That is, people continue conditioning their behavior on sets of norms and rules because they expect others to be doing so. But if their expectations change, radical cascades away from standard practices are possible. The idea that institutionalized social outcomes are subject to cascades such that new patterns are almost immediately established was not well understood in American political science. Students of Schelling (1978) knew this to be the case in understanding local processes, for example on whether ice hockey players would wear helmets, but these ideas were not applied to areas of cultural identification or societal institutions. The notion of an equilibrium suggests – and this is quite different from what is suggested in 1960s notions of institutionalization – that things are stable only because no person has an incentive to deviate from normal practice. But under conditions where a few have an incentive to devi-

ate, and where others see the possibility of a better individual existence if a critical mass of their fellow citizens deviate, cascades to a radically different equilibrium are possible. To be sure, theoretical work by Schofield (1999) suggests that the recognition of cascades in certain kinds of markets should induce us to give up the assumption of equilibrium. I think none the less that equilibrium theory, far more so than institutionalization, sensitizes researchers to the ever-present yet low likelihood of institutional collapse. The collapse of the Soviet Union should help push social science away from seeking explanations for values on “dependent variables” thought of as institutionalized outcomes. Rather social scientists should seek to describe equilibria in such a way that the conditions for radical shifts in value (off the equilibrium path) are well delineated. While it would be folly to have demanded of social science that it predict the Soviet collapse (Remington 1995, Kuran 1991), it would be equally imprudent to continue working with a methodology of social science that does not see the fragility of coordination in political life. The brittleness of our institutions, even when they successfully condition behavior for long periods, is a major lesson of the Soviet collapse. It should help foster in social science the study of institutional equilibria rather than institutional outcomes.

Notes

1. Stephen F. Cohen (1999) offers a more devastating criticism,

claiming that post-Soviet Russian studies “is in an intellectual shambles.” (*New York Times*, March 27, 1999) This outrageous charge does not merit specific rebuttal. This essay is rebuttal enough, as Cohen appears ignorant of the field reviewed herein.

2. Hanson (1997), following Jowitt’s notion of the Leninist “charismatic-rational conception of time” seeks to explain the waste of resources, the shoddiness of goods, and lack of incentives which undermined the socialist experiment. There was a “final-exam economy – since an endless summer vacation (communism) was always held to be just around the corner, the most rational thing to do was to ‘cram.’ Under Brezhnev this sense began to dissipate. But Gorbachev attempted, unsuccessfully, through “acceleration” (*uskorenie*) to re-establish charismatic-rational time.” This legacy, the reader surmises, cannot but have an impact on the current attempts to rationalize the Soviet economy. Another student of Jowitt (Geddes 1996), however, finds the Leninist legacy to have little explanatory power for questions of party strategy.

3. In Soviet studies, meta-commentary about area studies and cross-regional comparisons all-too-often rests on caricature. This is even the case for comparativists with excellent area studies credentials. For example, Snyder’s (1984-85) critique of the area students and Bunce’s (1995) critique of the transitologists are rhetorically compelling but vague when it comes to

questions such as whether the problems they identify are general to the literature, or specific to a particular set of contributions. ★



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