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Letter from the President

Trying to Navigate Between Scylla and Charybdis: Misspecified and Unidentified Models in Comparative Politics

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All empirical researchers, whether they use quantitative or non-quantitative methods, are confronted with two dangers. On the one hand, researchers must take care that their model is correctly specified, that is that the researcher has included the full set of relevant variables and that the causal relationships among the variables are correctly designated. On the other hand, researchers must insure that their model is identified, that is that there are enough restrictions on the possible relevant variables and their causal relationships that logically valid inferences can be drawn from the data. The dilemma is that more care a researcher takes to avoid potentially serious misspecification, the more certain it is that the model will be unidentified. With the typical data that is available to comparativists, the logical demands of identification force the researcher to use models that are almost certainly misspecified.

Consider the most common case in which the re-

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searcher seeks to estimate the effect of change in a particular variable on some outcome we care about. Misspecification generally arises from two sources. The first source of specification error stems from our lack of knowledge of the full set of relevant dimensions along which the cases differ. One cannot infer how much change in y is caused by a given change in x without removing (or controlling for) the effects of all other causes of changes in y . In case studies, the number of ways cases differ always exceeds the number of cases being studied. I once sought help from a Norwegian labor historian for a comparative project that I was contemplating on the organization of trade unions in the Scandinavian countries. His response was to tell me that anyone who would try to compare labor movements that were dissimilar in so many different ways as the labor movements of Norway, Sweden and Denmark was either ignorant or crazy.

When we use large enough data sets to employ statistical methods, the ploy is to assume that all of the other explanatory variables that we have left out are can be captured by an "error term" that is orthogonal to the set of independent variables that have been explicitly included. But who believes that all of the explanatory variables that have been left out are really uncorrelated with the explanatory variables that have been included? If we do believe that the left out variables are orthogonal with those we have included, it is certainly not a belief that has an empirical foundation. Some scholars insist that only time series data is credible since only when the unit remains the same can one reliably isolate the impact of one particular factor upon another. But many things change at the same time. Why should we believe that we could know all of the relevant changes over time any more reliably than we can know all of the relevant differences between units? The only time way to sustain the view that our cases differ in only a few dimensions is either to know very little about the cases or to willingly disregard most of what we do know.

The second major source of misspecification in studying the impact of an explanatory variable on some outcome of interest stems from the tendency to treat endogenous variables as exogenous. Treating an endogenous variable as if it were exogenous renders the association be-

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News & Notes

The **section's annual business meeting** will be held during the APSA annual convention on Friday, September 1, from 5:30pm to 6:30pm. No information on the location was available at press time.

At the annual meeting, the **Luebbert award for best article** will be presented to Stathis Kalyvas for "Wanton and Senseless? The Logic of Massacres in Algeria" in *Rationality and Society* (1999: 11: 243-285). The **runners-up** are Douglas Dion for "Evidence and Inference in the Comparative Case Study" in *Comparative Politics* (1998: 127-145) and George Tsebelis for "Veto Players and Law Production in Parliamentary Democracies: An Empirical Analysis" in the *American Political Science Review* (1999: 93: 591-608). The **members of the selection committee** are Adam Przeworski (New York University), chair, Matthew Evangelista (Cornell), and Bjorn Erik Rasch (Columbia).

The **Luebbert award for best book** will be awarded to Alexander Hicks for *Social Democracy and Welfare Capitalism: A Century of Income Security Policies* (Cornell University Press, 1999). The **runners-up** are Torben Iversen for *Contested Economic Institutions: The Politics of Macroeconomics and Wage Bargaining in Advanced Democracies* (Cambridge University Press) and Daniel Treisman for *After the Deluge: Regional Crises and Political Consolidation in Russia* (University of Michigan Press). The **members of the selection committee** are Jeffrey Herbst (Princeton), chair, Geoffrey Garrett (Yale), and Stephen Haggard (University of California, San Diego).

Barbara Geddes will receive the **Sage award for best paper** at the 1999 APSA convention for "Authoritarian Breakdown: Empirical Test of a Game Theoretic Argument." The runner-up is Dan Treisman for his "Decentralization and Corruption: Why are Federal States Perceived to Be More Corrupt?" The **members of the prize committee** are Peter Hall (Harvard), chair, Wendy Hunter (Vanderbilt), and Jim Robinson (Berkeley).

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tween the two variables in the data impossible to interpret. Suppose, to take a fanciful medical example, a survey researcher attempted to study the effect of aspirin on headaches by collecting a large data set in which the researcher determined whether respondents had recently taken aspirin and whether the respondents had a headache. A regression equation with the presence or absence of a headache as the dependent variable and aspirin consumption as the independent variable would almost certainly show that the use of aspirin significantly raised the likelihood of suffering from headaches. In this case the logical fallacy is obvious: the fact that aspirin provides relief from headaches induces people to take aspirin when a headache strikes. Is the situation so different when researchers study the impact of a particular set of institutions (or a particular set of policies, or a particular party in government) on some outcome? Institutions (or policies or governments) are not facts of nature but the results of political choices. Even if we were confident that the institution being studied was established for exogenous reasons, beneficial or harmful effects of the institution on outcomes that

people care about would affect the likelihood that the institution would survive unchanged over time.

In theory, there are statistical tools for addressing such problems. In theory, one should include all potentially relevant variables to the analysis and let the data reveal which ones have little impact and can be ignored. When endogeneity is suspected, one of the techniques for estimating simultaneous equations should be applied to the data to obtain consistent estimates. But the gap between what should be done in theory and what can be done in practice is wide. If we have too few cases relative to the number of potential explanatory variables, we need to collect more data. But if each new case adds more dimensions along which the cases differ, more data does not help. If we have suitable instruments, that is truly exogenous variables that are highly correlated with the endogenous variables, the possibility that an explanatory variable is endogenous can be incorporated in our analysis. But how often do we have truly exogenous variables that correlate highly with our endogenous variables? The more explanatory variables we include and the more variables we allow to be en-

ogenous, the more likely it is that our model is unidentified, that is that we lack sufficient information to draw any useful inference from the data. To learn anything from the data, we must impose a priori restrictions on the family of models that we consider. The trouble is that the inferences that are drawn are then only valid to the extent that the a priori restrictions are valid.

What is to be done when every case is unique and all variables are endogenous? Abandoning statistical work in favor of case studies is akin to killing the bearer of bad news. All of the problems of inference that are bedevil statistical analysis plague non-quantitative studies as much or more. The problems are just easier to see when the process of inference is formalized. Abandoning empirical work altogether may be tempting, but it is not an option for a discipline that aspires to be a science. The only response is to do the best we can and be humble about the results we obtain. Statistical techniques can expand the range of specifications that we consider, but there are no technical fixes in general. The problem is usually that the data do not contain enough information to draw infer-

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Symposium

Is there a Crisis in Area Studies Publishing?

Introduction

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Is there a crisis in book publishing on comparative politics—in particular, the part that focuses on individual countries or areas of the world? If so, is there anything that can be done about it? We asked a range of scholars and editors to share their thoughts on what seems to be emerging as one of the key sources of discontent in our subfield.

There is certainly more than enough frustration to go around on the subject. From the side of authors and would-be authors, getting a book into—and then out of—a major university press can be an agonizing process. Whole continents, as Jennifer Widner points out in her piece, have been written off by serious academic publishers. Even after editors accept a manuscript and peer reviewers have been satisfied, it can take 20 months to produce and print the book (my own recent experience). Prices set above \$50 seem to price almost all potential readers out of the market. The frustrations of editors at the university presses are equally palpable. For decades, they have watched the sales of academic books dwindle, fought for works that they thought important only to have them remaindered, and felt the squeeze as their budgets tightened.

The contributors to this issue bring a variety of perspectives. Sandy Thatcher, director of the Penn State University Press, presents some disheartening statistics on falling sales that threaten to turn books in comparative politics into an endangered species. Tim Colton suggests that financial support from universities or foundations could help to subsidize high-quality but small-market works of young scholars. Jennifer Widner argues that authors together with editors might do a better job of publicizing books and emphasizing policy implications; promotions committees could also be instructed not to dismiss books that are peer-reviewed but published by non-university presses.

Two contributors were slightly less alarmed about the trends. David Laitin warns against premature panic—despite falling sales, university presses do seem still to be publishing books on small countries at rates similar to those of previous decades. And Margaret Levi, who edits the Cambridge Studies in Comparative Politics series, suggests that books on particular countries and areas still have every chance of getting published—so long as they take on board the “methodologies and theories that animate the rest of comparative politics”. Reports of the death of area studies are, so far, unsubstantiated. We are sure this conversation will continue.

The Future of Scholarly Publishing in Comparative Politics

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In 1995 I wrote an article for *The Chronicle of Higher Education* ("The Crisis in Scholarly Communication," March 3) outlining the problems of publishing scholarly books in literary criticism and explaining why the Penn State Press could no longer afford to remain active in this field. Of the 150 books about literature the Press had put out in the previous decade, 65% had sold fewer than 500 copies, 91% fewer than 800 copies, and only 3% more than 1,000. The pattern of sales in this discipline had eroded to the point where a press without much of a subsidy from its parent university could not sustain a publishing program in it anymore. It seemed clear even then that what we scholarly publishers have come to call the problem of "endangered species" would be spreading to other disciplines over time. Five years later I think the writing is beginning to appear on the wall for a field like comparative politics, too.

Let's look at some numbers that illustrate change over time. My previous em-

ployer, Princeton University Press, did detailed studies of sales patterns in different disciplines. For political science as a whole, the average five-year total for books published in hardback only in the period 1960-1967 was 3,387. That average had already dropped to 1,889 for books published between 1971 and 1973. And for books published in 1985-1987 it had slumped even farther to 764 (a projected figure, based on actual first-year sales of 602).

Helped by an NSF-sponsored study of changing library budgets (documenting a trend of more acquisitions funds going to journals and less to monographs from 1969 to 1973), university presses began recognizing the seriousness of the erosion in library sales as early as the first half of the 1970s and adopted a new strategy of trying to recoup some of the lost hardback sales by issuing more titles simultaneously in hardback and paperback. Princeton was a pioneer in this effort with its introduction of Limited Paperback Editions early in 1972: LPEs were meant to make available scholarly monographs at prices between \$15 and \$20 to individual buyers who were increasingly unable to afford the higher prices of hardbacks that the steady rise in costs and reduction in

sales to libraries had combined to bring about. It was hoped, of course, that enough of these LPEs would sell to offset the decline in hardback revenues while allowing for a greater number of copies overall to be distributed. Eventually, as prices for regular paperbacks began to creep upward, the distinctiveness of LPEs as a new market niche began to disappear and many presses joined the trend of issuing more books in dual format on first publication.

How has this new strategy fared? It did seem to work reasonably well for a time. For books published as LPEs and hardbacks simultaneously at Princeton, the average sales for books appearing between 1985 and 1987 in political science were projected to be, for five years, 697 cloth and 2,931 paper (based on actual first-year sales averaging 446 cloth and 1,380 paper). With this experience as background, I carried over the same strategy to Penn State when I came here in 1989. In the area of comparative politics, we have done the most publishing in Latin American studies, so the statistics are most meaningful in this area. Between Fall 1992 and Spring 2000 we have so far issued 20 titles on Latin American politics, all simulta-

neously in cloth and paper editions. The average sales for this group of books as a whole so far has been 213 cloth and 862 paper. If we count only the 15 titles published before 1999 (so as not to bias the numbers because of the lower sales of books, especially in paper, in the first year of publication), the averages come out to 230 cloth and 1,054 paper. What is most ominous is the average of 165 for sales of the hardback edition of the 11 titles published since 1997, indicating yet a further significant erosion in library purchases in the most recent period.

We know already from statistics issued by the Association of Research Libraries that since the mid-1980s academic library purchases of monographs have declined nearly 25% as an ever greater share of their funds have gone toward sustaining journal subscriptions (even after libraries began canceling subscriptions in the early 1990s). Anecdotal evidence suggests that more libraries are opting to buy paperback editions instead of hardbacks when they are issued at the same time, thus contributing further to the drop in cloth sales that are so vital to the economics of scholarly publishing. Meanwhile, the proliferation of coursepacks in the

1990s, with more teachers preferring to cannibalize books for excerpts rather than assign whole paperbacks to their students (and often not even paying permission fees for such reproduction), have cut into the income presses have been deriving from that side of the market. Some presses have begun to respond to these new challenges by retreating to the earlier paradigm of initial hardback publication followed by a paperback edition a year or two later. While the sales of the paperback overall may not reach the same level (unless a book happens to become a staple of adoption for courses), the chance to sell 400-500 copies of a hardback in the first 12-24 months instead of 200 or fewer is persuading some presses that this is the only viable option left, short of abandoning publication in the field altogether.

But there are other options, of course. One is to adopt the business model long followed by European publishers: print only a few hundred copies of a new book in hardback, price it high enough to recover all costs from sales to libraries (say, \$125 for a 200-page monograph), and then any sales to individuals become icing on the cake. U.S. presses have shied away

from this approach, probably because we feel it is part of our basic mission as scholarly publishers to distribute as many copies as we can at as low a price as possible while still remaining solvent. Another option is to seek more subsidies from the universities whose faculty become our authors. Some universities are becoming more aware of the need to provide this kind of support, especially for junior faculty, as pressures for publishing monographs continue while outlets for them dry up. Stanford, for example, announced a couple of years ago that it would grant \$5,000 each to all junior faculty members for use in whatever way they best deemed to advance their careers, including as title subsidies to publishers. But why, then, shouldn't universities just subsidize their own presses more? We press directors would welcome that option, but it isn't being offered by many universities. A recent report from the Association of American University Presses (AAUP) shows that of the 62 presses participating in its survey for 1996-1999, only 46 received any subsidies, and the increase in parent-institution support was a mere .2% from 1998 to 1999 and only 6.1% since 1996, barely even keeping

up with inflation. At any rate, it seems unfair that the universities that do have presses should bear the full burden of sustaining the system of scholarly communication on their own, when it benefits scholars from all institutions; in this light, clearly title subsidies are a more equitable solution.

But what about saving costs by making greater use of technology? In fact, presses have been taking advantage of advances in technology for at least two decades, and the savings—in copyediting, design, marketing, order fulfillment, etc.—have been sufficient to keep the rate of increase in prices far below what it would otherwise have been, in the face of steadily eroding sales. More recently, there has been much talk not just about deploying technology to enhance efficiencies in producing books in the traditional manner but also about doing actual electronic publishing. Are e-books the wave of the future? If you look at what is happening in commercial publishing, with both Random House and Simon and Schuster recently announcing plans to digitize their entire backlist of 20,000+ titles and experimenting with new e-books like Stephen King's short novel issued earlier this

spring, you could easily get that impression. Indeed, there are new companies like netLibrary and Questia Media that have started up in the past couple of years for the specific purpose of digitizing what already is in print and making it available in a variety of electronic formats, and some of these companies, like Lightning Print and Sprout, are providing services to store titles in electronic form so that print copies can be produced "on demand" (even one at a time, on site in retail bookstores) and thus eliminate the need for publishers to keep inventory of slow-selling books. These offer new avenues for distribution of scholarly as well as popular books and a new stream of income (the potential amount of which is, at this early stage, very difficult to predict, however). Still, these are supplementary sources of revenue, just as permission fees for photocopying have become, but they will likely never cover more than a small portion of the overall costs of publishing a book. Those publishers who have experimented with doing full electronic publishing have quickly discovered that the added hardware, software, and staff costs of making the technology work equal, if not exceed, the traditional costs of typesetting,

printing, binding, and warehousing—which themselves only amount to about 40% of the overall expense of publishing a book. Thus, e-publishing offers no panacea for the dilemmas faced by scholarly book publishers today (though it may hold out greater benefits for journal publishers).

If technology is not going to be the savior, and if the other options outlined above seem likely not to be pursued to the extent necessary, what is the outcome going to be for a field like comparative politics in the near future? Another statistic from the AAUP report begins to loom large here: title output of 62 university presses increased only 2% in the period 1996-1999, whereas in the 1980s output was still increasing at double-digit rates. With increasing pressures on presses to pay their own way, many have resorted to changing the "mix" of what they publish, substituting more saleable titles (like regional books, reference works, "trade" titles, even textbooks) for less saleable monographs. At Penn State we expect to be cutting our list from about 70 to 60 titles annually soon, and those cuts will all come in traditional monographic studies, many of which haven't even sold well enough to recoup their manufacturing

costs, let alone contribute anything to overhead; the time and money we save will all be devoted to getting “more bang for the buck” out of titles with greater sales potential, especially regional books. Comparative politics, because of its sales profile, will be one of the areas we’ll be looking to reduce—thus pushing it closer to becoming another of the academic “endangered species.”

As if this were not bad news enough for all scholars in the field, there is even more bad news for junior faculty. More universities (including Penn State) are lining up to join the movement associated with the National Digital Library of Networked Theses and Dissertations (NDLTD), founded at Virginia Tech in the mid-1990s, which has the noble goal of making dissertations in the future more readily accessible worldwide for scholarly use through the Internet. But, unless graduate students are careful about how they participate in this kind of program, they may find that as junior faculty they have undercut opportunities for publishing revised versions of their dissertations as their first books. Libraries, which can readily tap into the NDLTD network, may think twice about spending scarce resources on books when they

know that they can access the original dissertations on which they are based for free (or license access to electronic dissertations from Bell & Howell, formerly UMI, for an annual fee). Even now there are libraries that instruct the vendors who handle their approval plans to inspect newly published books to find out if they started as dissertations and, if they did, to exclude them from their orders.

This trend, if it grows, can have a severe impact on the introduction of fresh research into a field like comparative politics, which depends so much on the in-depth empirical study that graduate students undertake in preparation for writing their dissertations (as David Collier noted in his Winter 1999 article, “the best opportunity that many scholars ever have to become deeply engaged in the intensive analysis of politics in another country”). One possible solution might be to draw on the work of many graduate students’ research in different countries to produce a volume that becomes more than the sum of its parts. An example on our list is *Technopols: Freeing Politics and Markets in Latin America in the 1990s* (1997), which was produced by a team of graduate students from Harvard and Stanford,

drawing from their dissertation research, under the direction of Jorge Domínguez as editor. Indeed, one could imagine more such collaborative projects even among just two or three junior faculty, who might develop their dissertations in advance with a view to combining their findings eventually in a truly cross-country or cross-region book. This could be a healthy development for the field and, I believe, would be welcomed by publishers, too—although it would pose obvious problems for tenure and promotion committees.

Another healthy development, which would take maximum advantage of new technology, is experimentation with new kinds of “enhanced” books. This is already being pursued by the American Historical Association, with support from the Mellon Foundation. Robert Darnton, then AHA President, elaborated this vision in his article on “The New Age of the Book” in the *New York Review of Books* (March 18, 1999). With no great constraints on space in the electronic world, e-books in comparative politics could incorporate data sets, interview materials, documentary appendices, hyperlinks to other works cited and to digital archives, digitized maps, color illustrations, even audio

files, to create much more comprehensive, multimedia publications. They could be constructed in “layers,” aimed at different audiences, too, as Darnton explains. And there could be opportunities for readers to engage in constructive feedback online, making their responses part of a work growing in complexity over time as a larger collaborative enterprise. The mind boggles at the possibilities! But none of this will be cheap—and it certainly won’t solve the immediate problem of the “endangered” traditional monograph anytime soon.

Causes of the Publication Bottleneck in Area Studies

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The increasing difficulty scholars have had lately in finding publishing outlets for country- and region-specific studies in comparative politics, as highlighted in the present issue of *APSA-CP*, has many sources and many consequences. Any attempt to deal with the consequences of the phenomenon must rest on an accurate di-

agnosis of its sources.

The parlous financial state of many university presses is without doubt a major cause of the trouble. It can readily be seen that this distress in turn reflects wider trends in the publishing industry. Stagnant and dwindling markets for books, pressures to cut costs, and the rapid growth in conglomerate ownership and cross-media tie-ins, with a concomitant “dumbing down” of the literary product, have dramatically reshaped commercial publishing in the United States and many other places. Independent publishers of quality works of non-fiction are a disappearing species. Had American academic presses in the 1980s and 1990s been for-profit firms not linked by their charters to university administrations, many and conceivably most of them would long since have shut down or been gobbled up by bigger fish in the pond.

Some particular features of comparative politics and country/regional studies aggravate the generic problem. The ongoing and unresolved debate within our discipline about the shape of comparative politics, and of priorities within it, is one such factor. Acquisition editors in university presses, being conversant with that discussion, tend to be less certain today than

their predecessors were a decade or two ago that a first-rate book on, say, Russian, Indian, or Central American politics deserves to prevail in what is an ever fiercer competition for spots in the press’s catalogue. Among other things, this uncertainty can induce editors to solicit reviews of manuscripts from two or three referees who have markedly different and perhaps irreconcilable views about the merit of single-country studies and, for that matter, about the form such studies should take. Conflicting readers’ reports are then the predictable result. At best, they drag out the review, revision, and resubmission process by months or even years; at worst, they block the road to publication or force the author, after years of fieldwork, library research, and writing, to undertake what is in effect a new research project in order to get the work published and move forward. These burdens, of course, weigh most heavily not on established scholars with a track record and with the time to shop manuscripts around but on their younger and especially on their untenured colleagues, to whom quizzical editors and boards of directors are least likely to defer and for whom the consequences of failing to publish

are the most severe.

A further complication has to do with the reality of the oft-maligned enterprise of “area studies.” It is often overlooked that, unlike “women’s studies” or “environmental studies,” area studies as a whole does not constitute a field of inquiry, a quasi-discipline, an undergraduate concentration, or a community of the concerned and engaged. Area studies for all practical purposes resolves into the particularized study of specific world areas, the boundaries of which, needless to say, are constantly being contested. At the operating level, too, there is far less cohesion and common purpose than meets the eye, for the simple reason that the bulk of teaching, research, and publication of results occurs to a lesser or greater degree within disciplinary frameworks. If Middle Eastern, post-Soviet, or Latin American area studies as such had a clear focus and *esprit de corps*, and if its practitioners constituted a seamless market for information—if, for example, the historian specializing on Peter the Great or the literary scholar who writes on Pushkin was likely to buy and read the latest tome on Russian federalism or the politics of economic reform under

Yeltsin or Putin—the problem we are talking about in this issue would be many times less vexing than it is. Conversations with editor personnel of leading university presses, however, leave no doubt that for almost every world area, and for almost every country, large and small, the pooled market for monographic works is steadily shrinking in the aggregate, and is thus approaching the limit of viability from the point of view of the economics of publishing a great many otherwise deserving manuscripts. This generalization seems to apply to works executed in *all* disciplines and fields.

University presses, in contrast to commercial houses, are constrained by the marketplace, not driven by it. As financial decision makers, most of them are at root risk minimizers, not profit maximizers, responding more to the fear of losing money on a given title than to the hope of making money on it. For an American university press, I have been told that the more or less secure market for a specialized hardcover on a foreign or comparative subject in the social sciences or humanities—mainly sales to libraries and a sprinkling of others through booksellers and direct marketing—is to

day about 500 to 600 copies. Twenty years ago, it was approximately 2,000 copies. The break-even figure on a title in this category is roughly 1,200 copies sold. The numbers, sadly, do not add up. Pricing books at ever higher levels to recoup publication and distribution costs helps little if at all, since it inevitably deflates demand for the product.

Setting aside other fields within political science, the implications for comparative politics are, in my view, especially disturbing. Comparative politics has long been distinguished by its diverse and therefore lively mix of research traditions, orienting topics and questions, and methods. The loss of one of its central strands—hands-on studies based on field research and use of native languages—would be an irreparable loss to the whole and would push it inexorably in the direction of an intellectual monoculture.

So what can be done? To the extent that the problem springs from global trends in publishing and in the finances of the university presses, no local solution can fix every aspect of it. Some of the possible remedies the editors of this issue of *APSA-CP* asked us in their instructions to address would have such large overall or distribu-

tive consequences for the discipline of political science—and, more to the point, for departments of political science in American universities—that they cannot be assessed in isolation from the needs and expectations of the other branches of the discipline, from political theory to international relations. For instance, greater acceptance of internet publishing would need to be arrived at across the board. Likewise, it is hard on the face of it to see how departments can extend the time from hire to tenure decision for country or regional specialists without adjusting it for other political scientists, or at a minimum without some consideration of possible inequities and frictions. And any departments that do reach internal consensus will subsequently be obliged either to operate within existing university regimes or convince higher-ups to make changes in them.

Comparativists who work on countries with healthy publishing industries that happen to contain a niche for intellectually serious books on public affairs may be able to find some relief by seeking outlets in those countries. If the country is the United Kingdom or Canada, no problem will arise for departmental review committees. If it is further afield, publication

in a non-English tongue will be a challenge. This not to say that departments will not be able to surmount it with good will. It is no exaggeration to say that some of the rest of us manage to come to responsible judgments about work executed in abstruse professional idioms which can be very nearly as challenging for the non-expert as another language.

Consciousness-raising about the publication bottleneck in comparative politics obviously makes sense across the board. One practical objective, which could appropriately be pursued through APSA and its comparative politics section, would be to educate all comparativists, junior and senior, to build publication expenses into their financial plans and especially into their grant applications. Some lobbying with the NSF and other public funding agencies may be necessary in this connection. For financial estimates to be realistic and applicable to the need, we all need a radical improvement in our shared knowledge base. How much money is actually needed to facilitate timely publication of a monograph? To whom should it be transferred, by whom, and how? What are the guarantees that it will offset actual costs of production and distribution and not be

diverted to general subsidy of the publisher's operations? Perhaps most crucial, how can we be sure that subsidies will not subvert scholarly quality control over the acceptance of manuscripts, and how can we ensure that it is *known* by all concerned that peer review has not been compromised by financial subventions. All these are questions that need to be answered before any concrete action is taken.

It is in this regard that country and regional centers in the universities can play a valuable entrepreneurial role, if awakened to the task. These centers are often in a position to raise modest project funds from foundations and, when included in campus capital campaigns, endowment money from alumni and friends of the university. As an example of what may be accomplished, the Russian and post-Soviet studies center I currently direct at Harvard recently solicited a \$1 million gift from a generous alumnus to endow a range of library- and publications-related activities. The money in hand, we concluded an agreement with Harvard University Press to underwrite one or two publications per year in our fifty-year-old book series at the press, which had been dormant for some time because

of financial problems. Some of the sum invested will eventually return to the center as income from sales, once the break-even point has been reached. Our priority will be to support the publication of first books by younger scholars. I stress that this is not a mere distribution agreement—which can be a problematic arrangement, especially for junior scholars, since books in a distributed series are not accepted by the press in question through its usual arrangements. Rather, Harvard University Press will continue to review and accept (or reject) manuscripts using the same procedures and quality standards as for any manuscript it considers, only this time in the knowledge that it will be relieved of financial exposure for titles it chooses to accept under the program. This is a model which other area centers, in collaboration with departments and individual scholars, may want to consider, as part of a concerted response to the problem.

Averting the Impending Literary Drought

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Each year, my undergraduates see *Je te plumerai*, a prize-winning Cameroonian film that laments the loss of heritage and community that results from the difficulty African authors experience in finding publishing outlets. Dependence and underdevelopment can arise from many sources, but the inability to communicate through the written word is especially dis-empowering. In this instance, the gap is not filled by writers from industrial countries, with their own viewpoints. Today's marginalization of Africa comes not only from the difficulty the continent's own citizens encounter in finding ways to express their views but also from the parallel challenges their American counterparts confront.

Editors, authors, and academic departments need to work together to avert an impending literary drought in the African politics sub-field. Current publishing trends potentially threaten our capacity as a society to respond to some of the major humani-

tarian challenges of our era. Why? As publishing of Africa-related material dwindles or shifts to reflect Western fads, both younger and older generations of scholars go on to other pursuits or turn to public sector employment. As a result, we risk losing independent, university-based expertise. Constructive debate is likely to decline as university faculty, news sources, and government consultants become one and the same. We also surrender the lessons systematic study of the region has to offer us. And in these turbulent times, U.S.-based scholars, African and American, have an important role to play in sharing ideas across national boundaries, getting stories out, and maintaining the chronicle of great events and lives. This obligation comes with greater means and greater security.

The Trends

The publishing problem is partly evident from the roster of important university publishers which will not entertain social science manuscripts on African topics and from the preferences displayed by those that do. Harvard University Press categorically tells authors that it cannot make money on a book with Africa content and declines submissions. Princeton University Press has long

had the same policy, although recently it has deviated to publish Jeffrey Herbst's *States and Power in Africa*. University of California Press, once a mainstay, has joined Chicago University Press in narrowing its list mainly to "cultural studies," excluding most serious social science writing. Duke University Press also favors post-modernism. Cambridge University Press's political science division has succeeded with *Democratic Experiments in Africa* by Michael Bratton and Nicolas van de Walle, but editor Alex Holzman, comments that most writing on Africa is a threat to the press's bottom line. Very few publishers will consider sending single-country monographs out for review, unless they have a "cultural studies" orientation.

The academic presses explain that their decision rules are market-oriented. The demand for books on Africa in the United States is small. It is hard to sell the numbers of copies required to break even, much less generate a profit.

There is certainly considerable truth in these pleas, yet two counter-trends stand out. The first is embodied in a person, the remarkable Lynne Rienner, who has built a press publishing the very books others considered

money-losers. Her company, which shares her name, has helped give new authors and African colleagues a footing in the discipline. It has kept social science writing about Africa alive. She deserves great recognition for her efforts. Heinemann and Westview have performed similar services to the profession. None of these presses has failed as an economic venture.

The second counter-trend lies in the willingness of major commercial presses to experiment with books on Africa. Farrar, Straus, and Giroux produced two prize-winners in 1999: *The Seed is Mine* and *Tomorrow We Will Be Killed With Our Families*. Both books sold more than 5,000 copies. Robert Bates and I both owe a debt of gratitude to W. W. Norton, which also has gambled that the bottom line will work out favorably with respect to my book, *Building the Rule of Law*, and Bates's *Prosperity and Violence*. These presses submit manuscripts for peer review, just as others do, but they are often more willing to rely on their own judgment. One editor at Hill & Wang observed that the commercial press editors are deeply puzzled by the bright-line rules their university counterparts seem to have adopted with respect to Africa-related

manuscripts.

These trends and the counter-trends have implications for university decision making. Good dissertation research can rarely embrace comparisons across national boundaries, except in a very general contextual chapter. Given the current decision rules, which either exclude Africa entirely or enforce a multi-country comparison standard, younger colleagues are going to encounter more trouble in publishing their first books than my cohort has. Keeping a small base of independent expertise alive may prove more difficult than it has been for the past forty years.

Even those who do succeed in publishing their work may encounter trouble. Review committees at many universities take signals from the identity of publishers. In the pecking order, Lynne Rienner, Westview, and Heinemann tend to occupy the lower ranks, regardless of the quality of any specific contribution. Similarly, books produced by commercial presses for the trade market often carry a stigma. They often contain less jargon in order to reach a wider audience. For the same reason, they relegate equations to an appendix or eliminate them entirely. Committees ask the same question one of my

younger colleagues posed when I returned to another project after *Building the Rule of Law* was finally done: "Will it be an *academic* book this time?"

Partial Remedies

The counter-trends suggest that there is more at work than pure economics in the decisions publishers make about books with Africa content. As a result, it may be possible to help university presses abandon the bright line decision rules they have adopted in favor of more flexible standards.

First, editors' own sense of security with the subject matter sometimes makes a difference in willingness to consider manuscripts. Work on Africa can be worrisome. Few editors have an extensive acquaintance with the continent. It is often difficult to spot the big questions or enduring issues, as well as their constituencies. It may be hard to gauge what a good answer or convincing evidence looks like, especially when reviewers offer scant comments in this regard or when there is a lot of personal lobbying.

As a sub-field, we don't spend much time communicating with editors about the important trends and policy problems behind the headlines. Group conversations

between scholars, policy makers, and publishers at professional meetings could help highlight the big questions. Universities also could foot the bill to bring publishers to important meetings that will yield good manuscripts, either directly or indirectly.

Second, organization and management matter for the bottom line. The ability to publicize books to the audiences most likely to want to read them varies greatly across presses. Some are more active in working with authors to identify potential readers than others. As a sub-field, we can do more to help target audiences for presses, sending "business plans" with manuscripts to indicate likely readerships, and pooling mailing lists. The presses can also do better in this regard.

A third challenge is to keep methods and approaches in perspective. Presses that employ a methodological decision rule in their selection of manuscripts to review only amplify the current misplaced tendency to use method as a litmus test for quality-of-mind. Although it is important to encourage colleagues to take up new tools and to choose approaches self-consciously, the fact remains that a lot of good political science writing

on Africa isn't going to have the same "look" as writing on Europe or the United States. The fit between current methodological trends in the social sciences, the kinds of evidence and issues that present themselves in the study of Africa, and the backgrounds of potential readers is often difficult. Students of Africa rarely have large data sets to work with. And formal models of complex bargaining situations frequently fail to generate much light, since we usually know little about the identity of the relevant actors, the options available, and the preferences for risk. Indeed, this work often has the same fairy-tale quality as "cultural studies" that dispense with our standards entirely. For these reasons, work in this sub-field often relies more heavily on well-executed ethnography, history, and case studies.

Authors must do their part with respect to this problem. Even if we cannot always engage in the same kinds of methodological pyrotechnics as some of our colleagues in other sub-fields—if neither our questions nor our data make these desirable—we still have an obligation to show the significance of our work and to approach data collection and analysis thoughtfully. After several years of service on

the African Studies Association's Herskovits Prize Committee for the best book in African Studies, I had to agree with my non-political science colleagues that it was the rare political science book that presented a new and interesting answer to an important question, in an engaging manner. Perhaps in our effort to look like Americanist colleagues we hew too closely to a template and miss what is different and significant.

Fourth, it is a relatively easy matter to expand the audience for a book by showing the implications of an argument for policy. The thirst for ideas in policy circles is considerable. An accessible style and some sense of the relevance for what governments, non-profits, activists, or others should do or how they should think about a problem often improves marketability, though not every important subject can support this extension.

More likely than not, some of the current trends will continue. That shifts the burden to universities to re-evaluate some of the standard components of promotion reviews, both at the departmental and college levels. A short note about publishing trends could accompany the dossiers of young scholars in the sub-field to keep col-

leagues and deans informed about the publishing context. The identity of the press that produces a book should no longer serve as a proxy for quality. Instead, colleagues must assume more responsibility for making up their own minds, based on a careful reading of a portfolio. And books that use plain language in order to reach larger audiences, reserving quantitative models for appendices or for companion journal articles, should suffer no stigma. In other words, good judgment and an understanding of the interaction between economics and expository style must enter the decision making of authors, presses, and universities more forcefully.

Is There a Crisis in Publishing for Comparative Politics?

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The modal career path for scholars in comparative politics begins with a dissertation that is built upon basic research in a single country, and then a book based on that dissertation. This is the path followed by four of the

six presidents of this section (Bates on Zambia, Laitin on Somalia, Collier on Peru, Stephens on Peru), both editors of this newsletter (Golden on Italy, Treisman on Russia), and many other leading comparativists at top universities (Kohli on India, Lustick on Israel, Bermeo on Portugal, Stokes on Peru, Tarrow on Italy, Comisso on Yugoslavia, Lijphart on the Netherlands, Apter on Gold Coast, Hough on the Soviet Union, Shirk on China, Keller on Kenya, and I can go on).

There are good reasons to justify this step in a modal career path. Knowing at least one country and its political experience creates a foundation for constructions at higher levels of abstraction. If we can assure ourselves that the abstract theories or models that motivate future writings in fact were played out in a country we know well, we can write about those theories and models with more confidence. Also, providing the details of institutions and processes in a country is a public good, one that will be useful to others far longer than the theoretical models that motivated the study.

Country-based dissertations continue to be the modal product in our subfield, and not only because they are a good first step in career

development. Since most single country monographs is declining incidence of coun-try jobs are advertised by region, becoming quite difficult, I try-study book publications and it is nearly impossible for performed a small test. I cannot from this table be sus-tained. Indeed, this finding is a young scholar to carry off chose four countries, each on tained. Indeed, this finding is field research at several dif- a different continent, and consistent with David Col-ferent sites, country-based each small enough such that lier's observations for this dissertations are the route to there would not be mass newsletter (Winter, 1999), in employment. To be sure, sales appeal just because the which he pointed to the cross-national statistical work country was important. I plethora of single country is making inroads. But the went to the Stanford cata- publications at several excel- dominance of the country logue, and counted the num- lent presses.

study as a first project is well ber of books on each of these It is true that each decade institutionalized in the *habi-* countries by decade. The has a favored topic. Going *tus* of comparative politics. If sults are on the accompany- from the 1960s on to the 1990s, I saw a trend from it were to be the case that ing table. modernization to military rule there was a secular decline in From these data, it to bureaucracy (nearly all the the publication of mono- should be clear that there is publications on Thailand for graphs whose focus is upon a no trend to be found. There two decades were studies of single country, though of is a small and steady stream its civil service), to corpora- course written in a of books on each of these tism, to structural adjustment, "comparative perspective," country's politics that ap- and now to democratization. this would be devastating for pears every decade. Multiply There was also a fundamen- the future of our sub- these decadal publications by tal shift in author names. In discipline. the number of countries in the more recent decades, au- thors seemed more likely to In order to see if there is the world, and this would have their origins in the any plausibility to the fear, as make possible the advance- country being studied. Fi- expressed by several mem- ment of many careers in the nally, there have been some bers of our section's editorial field of comparative politics. board, that publication of The premise that there is a

**Area Studies Publications in Political Science:
Political Science (Broadly Conceived) Manuscripts Published in English**

	1960-69	1970-79	1980-89	1990-99
Uganda	9	15	10	10
Colombia	6	6	12	9
Thailand	10	13	10	11
Norway	3	6	1	5
Total/Decade	28	39	33	35

Note: From the Stanford University library catalogue, I entered "Politics and Government" for "Subject," and each of the four countries as "Keyword." I then counted all non-edited books in which the politics and government of that country received primary attention. If more than two countries were in the title or in the card description, I considered that to be evidence that the politics and government of any one country were not getting primary attention.

changes in methods, with evidence of quantitative and formal work in the 1990s that were not as evident in the earlier decades. But this may be more of a reflection of the topic (the economic revolutions of the 1980s) than of methodological principles. From a reading of titles and basic card-catalogue summaries, the monographs of all decades reflected theory-guided descriptions of structures, events and processes.

My own experience having edited for a decade the Wilder House Series in Politics, History and Culture (about which I wrote in a previous issue of this newsletter) is consistent with the table presented here. Although there were some difficulties in getting Cornell University Press to publish monographs on some countries (e.g. France, whose intellectuals do not buy books about their country written by Americans, or so I was informed by Cornell's editorial junta), we published country studies on France, Hausaland (a comparison of Niger and Nigeria), Senegal, Sri Lanka, China, Greece, Spain, Britain, and the former Soviet Union. The Cambridge Studies in Comparative Politics publishes fewer country studies of the traditional sort, but the tendency is to have regionally specialized foci, for

example, parties in Western Europe or language repertoires in Africa. Still it has published country studies on Canada, Senegal, Brazil, and the Soviet Union. And the most famous study in comparative politics in the past decade has been Robert Putnam's country-study of Italy. Not only do University presses continue to publish country studies in political science, but Westview, Rienner, and several other presses have entered the comparative politics field with gusto.

In a recent colloquy in *The New York Times* (February 26, 2000), Ian Shapiro mourned the imminent erosion of the vast "stock of knowledge" that political science had acquired through the gritty activities associated with field and archival study. His position was most ironic, as he seems to be guilty of exactly what he accuses his adversaries (rational choice analysts) of ignoring. He argued that "typically rational choice theorists either ignore or recycle conventional wisdom" and therefore have no incentive to acquire new knowledge about the political world. Yet here Shapiro himself, instead of examining empirically whether there has been any loss in that great warehouse of wisdom

(sounds like the snows of yesteryear to me!), assumed a reduced inventory (modeling the effects of the rational choice explosion on the discipline as a whole), and then sought remedies to restore it. In my albeit limited (but at least empirical) test of this perspective, I find no such evidence. The publication of country studies has remained steady from its heyday in the 1960s.

Comparativists face several barriers to career development that require remedy, and I wrote about some of them in my presidential column in this newsletter a few years ago. Given the now-standard skill package (regional knowledge, cross-sectional statistical techniques, theory), comparativists have a longer route to a Ph.D. than do scholars in other subfields. Yet most universities set normalized time criteria for fellowships and other graduate perquisites, as if all subfields required the same amount of training. Also, comparativists who write narratives of events find it nearly impossible to fit their papers into the page limits in our disciplinary journals. These page limits favor statistical and formal analyses over narrative. Finally, comparativists face an immense hurdle in skills investment for their second projects, one

that is not easily jumped during the period when they are assistant professors. But I do not see the demise of the theoretically-informed case study as imminent; and young comparativists will have no more than normal trouble—associated with peer review—in publishing such studies to launch their careers.

Making a Case for Case Studies

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The Cambridge Studies in Comparative Politics promotes high quality social science scholarship on critical issues in comparative research. All books in the series must represent both a theoretical and empirical advance. All must contribute to the social scientific enterprise of building a cumulative body of knowledge.

The varieties of ways of achieving a substantive contribution and a synthesis of theoretical and empirical material are varied, however. The kinds of perspectives on which authors draw reflect the plurality of views and the conflicts in approach that make comparative studies such a vibrant field of in-

quiry. The series has already published and will continue to publish books that use historical institutionalism, the new economic institutionalism, democratic theory, formal theory, and collective action and resource mobilization theory, among other possibilities. The methodological approaches are equally diverse. Some of the books compare several countries and others offer a single case study; some use large-*n* samples and others focus on a single case; some rely on statistics and others on field work; some are more historical or sociological and some more economic.

Despite the immense variations in projects and approaches in the series, the standards imposed on all authors are the same in some important respects. All must engage in rigorous scholarship, use and use well the appropriate methods for the problem being analyzed, and advance our understanding of significant questions in comparative studies. Hypotheses should be falsifiable, and the procedures for data collection should be transparent and, to the extent possible, replicable.

Even so, there is some preference for manuscripts that compare across states and nations, but this is not a hard and fast requirement.

Many first-rate books investigate an event that is so significant to world history that its seeming uniqueness is almost irrelevant. The French Revolution is a case in point. Other major works investigate a single country or even a single party or village; Robert Michels' *Political Parties* comes to mind or the books by Samuel Popkin and James Scott that generated a paradigm defining controversy in comparative politics. What makes books acceptable for the Cambridge Studies in Comparative Politics is not comparison per se but the generalizability and significance of the findings.

Nonetheless, the bar for single case studies is higher than that for cross-national comparisons, and the bar for single case studies of small or politically insignificant countries is even higher. The requirement of transportable theoretical implications means that many traditional modes of area studies research are unlikely to be accepted. When the focus is on a country for which there is not a large market of readers, presses are particularly wary of publication. Studies of China, Russia, Germany, or South Africa are more likely to be published than studies of Australia, Costa Rica, Vietnam, or Hungary. Authors, their reviewers, and series

editors must make a very strong and convincing case that the work is so inherently interesting and theoretically momentous that it will well repay the costs of publication with both money and prestige for the press.

In the past, university presses had some obligation to publish work of scholarly significance but market insignificance. That is no longer the case. The economics of publishing have changed. University presses are no longer subsidized to the extent they once were; they must now be largely self-supporting. That makes press decisionmakers even more wary than they once were about taking a risk. They do not want to take the chance that a book, scholarly though it is, is the product of an arid Casaubon.

There are, of course, market-driven solutions to the combination of the economics of contemporary publishing and the requirement to publish a book to achieve tenure. For some time now, there has been increased emphasis on peer reviewed articles and niche journals. There is serious consideration of electronic journals and even books. There are series that publish monographs—in cloth and at a high price—for limited markets. Yet these remain

only partial solutions, and many young scholars find them disappointing and inadequate. They also raise the issue of the standards being used to differentiate those books that get major publishers and those that do not. Would the first books of such distinguished contemporary comparativists as Robert Bates or David Laitin or Sidney Tarrow have a harder time today in finding a distinguished publisher than they did several decades ago? Perhaps I'm an optimist, but I like to believe they would still be published by a major university press.

The move by university presses away from limited market monographs contradicts the move in research university departments towards the requirement of a published book for tenure. Do these two factors combine to encourage certain kinds of work and discourage others, irrespective of intellectual and scholarly rationales? Yes and no. Is the effect to doom area studies and the careers of young scholars whose dissertations depend on in-depth knowledge of a particular time and place? Not at all.

The new hurdles create new challenges but also new opportunities. They reflect as well as precipitate shifts in the research strategies of

comparativists. I have already discussed market-driven solutions to the problems created by the contradiction between the requirements of the publishing industry and of tenure granting departments, but of even more interest are the intellectually-driven solutions.

Let me be very, very clear. It is not field or archival work that prevents publication; it is the failure to use that data in the service of a larger set of issues. The incentives to advance social scientific understanding and not just knowledge of a particular country do have market sources, but the more important pressure is coming from within the disciplines of political science and sociology. Both historical institutionalists and rationalists, for example, are demanding that field, archival, and case study research be part of a theory-building and theory-testing enterprise. And we have been demanding that for some time. This in itself is not new. What is new is that the research programs of junior faculty and graduate students are increasingly meeting this standard, and, when they do not, they are even less likely to succeed than they were before.

The effect is indeed a transformation of area studies. There is still an incentive

for departments to hire comparativists who know some part of the world well, but it is no longer true that all comparativists must be area specialists. Political science still seeks and values scholars with in-depth regional knowledge, linguistic skills, and cultural sensitivity. These are no longer enough, however; area studies students must now also demonstrate theoretical and methodological capacities that allow them to put their knowledge into comparative perspective.

There are several strategies scholars have taken, and all produce quite publishable books for a series such as Cambridge Studies in Comparative Politics. The first and dominant strategy is to undertake regional comparisons, either within the same country or among neighboring countries. Forthcoming books by Susan Whiting, Pauline Jones Luong, and Susan Stokes offer excellent models of how to do this. The next two strategies are more risky but can have high payoffs if successful. Reliance on an approach such as analytic narrative, historical institutionalism, or econometrics can make it possible to generalize from a single case or small set of cases and build theory. Alternatively, the discovery, collection, and rigorous analysis of an important

set of data with significant social science implications can justify a book that focuses on a so-called insignificant country.

These are strategies for dealing with single case or small-*n* studies. There is no bias, so far as I can tell, against field or archival research per se. Indeed, qualitative methodology is increasing in stature, becoming a science as well as an art. The traditional tools of the area studies scholar are intact although more has been added to the tool kit.

Area studies is definitely experiencing transformation. Reports of its death are unsubstantiated, however. Rather, it is taking on board the methodologies and theories that animate the rest of comparative politics. Consequently, area studies, like the rest of comparative politics, is becoming more methodologically and theoretically sophisticated and more interesting for those of us committed to social science. And, in its new guise, area studies has a secure place in the publishing program of series such as the Cambridge Studies in Comparative Politics. ❖

(Continued from page 4)

ences without restrictive assumptions. Theory helps, as always, but how do we know which theory to trust except on the basis of empirical verification? Testing the robustness of our findings by observing (and reporting) the changes in specification that would eliminate or reverse our results would be useful. All findings can be killed with a general enough model. Openly reporting what we know about how our results can be destroyed would facilitate communication. Above all, we would benefit from dropping the pretense that our inferences are the only inferences that a reasonable scholar could draw from the data.

In the end, empirical work is not so different from normative work. The arguments improve over time, as we gain knowledge about the conclusions that follow from different sets of assumptions. But arguments are rarely resolved, since different assumptions lead to different results and we disagree about the assumptions. ❖

Use the Newsletter in the classroom!

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Continuing Debates

To paraphrase Riker, “Six Articles in Search of a Subject”

On “Political Parties and the Political Economy of Decentralization and Federalism”*

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In his introduction to the symposium on “The New Political Economy of Decentralization and Federalism,” Daniel Treisman noted that the shadow of economist Charles Tiebout continues to tower over the study of the political economy of federalism. Conspicuously absent from the six-article symposium, however, were substantial references to one of our own, the recently-departed William Riker, for his contributions to the study of federalism.

Students of federalism owe Riker a huge debt, and we owe it to ourselves to ask what he might have added to this symposium. First, we might remember that for all of his writings on federalism,

Riker exhibited a profound skepticism about the concept’s utility as an independent variable (e.g. 1969). His early writing on the subject (1964) focused on the emergence of federalism (i.e. as a dependent variable), and his later work (e.g. 1987) focused on its evolution. Riker repeatedly expressed serious doubts about the utility of using “federalism” to explain differences in policy *output*. He did believe that federal institutions can affect a country’s political “style” (1975), but held this to be relatively unimportant, because actual policy outcomes “depend more on political culture than on the style of debate” (1969). “Federations are more unlike than alike,” he stated, “having in common only the logically necessary features of the moment of contract” (1975). After formation, each federation develops along a distinct path, making generalizations about the operation of federalism impossible, if the implicit point of comparison is non-federal systems.

At this point the authors in the symposium might even agree with Riker, for their implicit research agenda is *not* to compare federal and unitary systems on either policy

style or policy output. Instead, they mainly focused on understanding the operation and evolution of federal systems themselves. Leaving aside the question of whether the political economy of federalism ought to also attempt to tell us whether federal systems do or do not differ substantially from unitary systems on some important output variables or not, what might Riker add to this discussion?

Wagering another guess, I believe he would suggest that the key to understanding the political economy of federalism and decentralization lies in the structure of political parties. In my view, this was the symposium’s main omission. Of the six sets of authors, only Inman and Remmer and Wibbels even mentioned the potential impact of political parties and the party system. Rose-Ackerman and Litvack and Rodden do not mention Riker or parties at all. Weingast merely noted that Riker held that democracy might help sustain federalism, and Bednar notes that Riker, for all his interest in rational choice and formal theory, never combined these two bodies of thought into a theory of federalism per se.

The main research ques-

tion in the symposium was “What causes centralization or decentralization of federal systems?” A Rikerian response might hold that the degree of centralization in federal systems is tightly linked to the degree of party centralization. This idea originated with David Truman, in his chapter “Federalism and the Party System” in Arthur MacMahon’s 1955 edited volume, *Federalism: Mature and Emergent* (a fact that Riker acknowledged). In his many works on federalism Riker built on Truman’s insight, and went so far as to argue that “the structure of parties is a surrogate for the structure of the whole [federal] constitution.” If federalism and party structure are closely linked, then an explanation of phases of centralization and/or decentralization in federal systems ought to concentrate on the evolution of parties and the party system. Changes in federalism ought to follow similar episodes within the structure of the dominant political parties.

What specific variables could we use to operationalize the relationship between party structure and federal structure? Riker focused on whether the national party controls subnational party organs, and whether the national party itself is cohesive.

More specifically, I suggest that the following three variables are important to explain variations in “centralization” in federal systems:

- 1) The extent to which national party leaders control nominations for both subnational executive office (e.g. governor) and for national legislative office. If elected officials (at subnational or national levels) do not owe their careers to national leaders, then they have few incentives to listen to national leaders’ calls for cohesion on policy issues, and federalism ought to be decentralized.
- 2) The extent to which the national party can provide electorally valuable goods for candidates (independently of the first variable). For example, a strong national party label might be valuable to candidates for the national legislature because it reduces the costs of campaigning. This strong national label might overcome the pull of subnational political identities and foster political centralization of the federal

system. In contrast, in the absence of strong national labels, candidates may look to subnational political forces for politically valuable resources like money and media access. Because federal systems by their nature provide an “opportunity structure” for political leaders who gain subnational executive office to gain autonomy from national leaders, national party leaders in federal systems may have to compete for the allegiance of national legislators with subnational party leaders.

- 3) The extent to which the electoral rules encourage “distance” between national and subnational elections. For example, if presidential elections are held concurrently with legislative elections, we expect the presidential election to provide “cues” guiding voters’ choices for other offices. This would give the legislative elections a national tinge. However, if instead gubernatorial elections are concurrent with legislative

elections, and the presidential election is held separately, then gubernatorial elections in each state might drive the national legislative election in each state, giving *national* legislative elections in each subnational unit a flavor of their own - a distinctly non-national flavor.

A focus on parties, party systems, and electoral dynamics in federal systems could inform the study of the political economy of decentralization and federalism. One might, for example, posit a link between Weingast's "market-preserving federalism" (in democratic countries) and the emergence of cohesive national parties, or argue that Hard Budget Constraints (Rose-Ackerman, Litvack and Rodden, Inman, Remmer and Wibbels) are much more likely to emerge in systems where parties are centralized rather than decentralized, all else equal. The formal study of federalism sometimes incorporates electoral dynamics (as Bednar describes), but might also benefit from greater incorporation of party structure into modeling efforts. In short, insights from the "electoral studies" literature - not the *number* of parties, but their *character* - can

be applied to the study of the dynamics of federal systems.

A recent article in the *Latin American Research Review* on decentralization by Garman, Haggard and Willis (1999) best exemplifies a comparative approach to this question. Case-specific research on Mexico (Diaz-Cayeros, Nacif, Langston), Argentina (Eaton, Jones), Spain (Linz and Stepan), Russia (Ordeshook and Shvetsova), Brazil (Mainwaring, Samuels) and other countries confirms that pressures to decentralize and/or alter federal institutions arises from electoral pressure and/or from changes in parties or the party system.

In this brief essay I cannot do justice to Riker's writings on federalism, and of course my exclusive focus on Riker necessarily ignores many political scientists who have studied the relationship between federalism and political parties. My purpose here was to insert a Rikerian view into the study of the political economy of decentralization and federalism: to understand the dynamics of the political economy of decentralization and federalism, Riker would admonish political scientists to pay relatively less attention to the economists, and more attention to the dynamics of party and party system change. Of

course, this means that we should turn our attention to the question of what factors drive party and party system change, a question I leave for another newsletter symposium.

* See William Riker, "Six Books in Search of a Subject: Does Federalism Exist and Does it Matter?" *Comparative Politics* Volume 2, Number 1 (October 1969), pp. 135-146. Riker at his rhetorical best. ♦

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.

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Good Reads

Durkheim on Method

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Durkheim on method a “good read”? It might be hard to think of anything less appealing than sifting through *The Rules of Sociological Method (RSM)* in search of enlightenment on how to do social science. I must admit that I did not read this book for fun. But I did find that it furnishes strong tonic for anyone interested in the theoretical and methodological debates that dominate contemporary comparative political science. At the very least, it reminds us that social scientists were pummeling each other a century ago over exactly the same matters that we pummel one another over today.

General approach to causation

Durkheim has sometimes been accused of advocating a naive enumerative inductivism, in which broad conclusions supposedly emerge from counting up observations. But in all of his major works, including in *RSM*, Durkheim embraces not enumerative induction, but rather eliminative induction,

a method advocated by J. S. Mill in which putative causal hypotheses are systematically eliminated by comparing them with facts. While defending induction, Durkheim also assumed that a hypothesis, or something like one, was needed even to engage in meaningful observation. Just as a hypothesis cannot materialize out of thin air (*contra* the logic of pure deductivism), neither could one imagine an “observation” in the absence of some more general understanding that made sense of the thing (*contra* the logic of pure inductivism). Durkheim regarded inductivism and hypothesis testing (or “hypothetico-deductivism” as his mentor, Emile Boutroux, labeled it) as the mutually dependent elements that constituted the core of the effort to advance scientific knowledge. The methods for establishing causal links that Durkheim opposed, and against which he regarded inductivism and hypothetico-deductivism as allied, were those based on *a priori* principles and “introspective” methods.

Specifically, Durkheim rejected two approaches that were current in his time and that are still fashionable in

social science. The first was the axiomatic-deductive reasoning of mathematics. In Durkheim’s view, such reasoning was perfectly appropriate for mathematics itself. Mathematicians construct their own concepts and are aware of what they invest in them; they analyze pure abstractions that they themselves create. But such reasoning, according to Durkheim, is inappropriate for analyzing things that we scholars do not make ourselves and do not fully know in advance, which include social phenomena. Thus Durkheim rejected the reasoning of some of the political economists of his day. He accused them of imposing an “ideology,” or “pure conceptions of the mind,” on social facts and of submitting their governing notions to empirical tests of “a few examples” drawn from a universe of “countless facts” in order to validate their preconceptions (*RSM*, 67-70).

It is remarkable how little the essence of polemics over method and epistemology have changed in social science over the past century. “The economists” on whom Durkheim trained his fire find their contemporary successors among scholars who

embrace rational choice approaches and rely upon microeconomic foundations. It is helpful to compare Durkheim's thinking with that of an exponent of the type of approach that Durkheim criticized. Since his defense of axiomatic-deductive reasoning is so lucid (and since I have virtually memorized his book after teaching it repeatedly in my graduate seminar on comparative politics), George Tsebelis is my choice. According to Tsebelis, all actors must be assumed to act rationally: If "an actor's choices appear to be suboptimal, it is because the observer's perspective is incomplete." Such reasoning is self-consciously self-confirming. According to Tsebelis, "one might say that all rational-choice models are tautological...there is nothing more in the conclusions than in the assumptions." This tautological quality, "far from being trivial, is difficult to achieve," and lends rational choice models virtues that models developed in other approaches lack. Chief among these are that purely deductive models are "truth preserving." They may serve as vehicles for the cumulation of knowledge "because even the models that lead to false predictions are essentially 'correct'." According to Tsebelis, "Up to now, a com-

mon procedure in political science scholarship has been to observe an empirical reality, then to establish it through statistical methods, and finally to produce a plausible argument consistent with the regularity." But, he continues, "The deductive arguments of rational choice have demonstrated conclusively that the last part of this procedure...is not equivalent to theoretical reasoning." This is so because "Each step of a plausible inductive argument is not completely truth preserving, so by the end of the argument, what is left out may be as or even more important than what was preserved" (1990, 6-7, 42-43).

Such reasoning was common among the political economists of the early 20th century, just as it is among us. Durkheim rejected it. He acknowledged that the economists with whom he argued were capable of constructing aesthetically imposing mental conceptions. But in Durkheim's estimation, beauty was not truth, and aesthetics furnished flimsy criteria for evaluating social science theories and modes of inquiry (*RSM*, 67-68). Any approach in which "there is nothing more in the conclusions than in the assumptions," to quote Tsebelis's phrase, could not pack much punch since it did not sub-

stantially expand our understanding of social reality, which Durkheim took to be the sole end of social science. According to Durkheim, "We do not start by postulating a certain conception of human nature, in order to deduce a sociology from it; it is rather the case that we demand from sociology an increasing understanding of humanity" (*RSM*, 236). Durkheim was deeply concerned with discarding "prenotions" or "preconceptions" prior to undertaking sociological inquiry. By "prenotions" or "preconceptions" Durkheim did not mean hypotheses, but rather operational assumptions that were self-reinforcing and that placed themselves *a priori* beyond methodical doubt (*RSM*, 72-74). To the argument that inductive arguments are not "truth preserving" Durkheim might respond that a *purely* inductive approach that merely posited a "plausible argument consistent with the regularity" (Tsebelis's words), completely detached from any prior established knowledge or argument about the regularity, would indeed suffer from shortcomings. But combining inductive methods with the use of hypotheses and hypothesis testing, guided by existing bodies of knowledge, permits the cumulation of knowledge while

avoiding circular reasoning, the preservation of “truths” that consistently fail to stand up to empirical test, and the degeneration of empirical tests into an exercise in cramming facts into preestablished theoretical frameworks. Durkheim might also simply deny the idea that “truth-preservation” in Tsebelis’s sense is an appropriate epistemic criterion.

To Durkheim, the notion that only axiomatic-deductive and tautological models qualify as “theoretical reasoning” short-circuited the social-scientific enterprise by treating “unknowns” whose status was properly considered a matter of inquiry as established social facts. In Durkheim’s mind, this move expunged the very process of inquiry—and therefore science—from social science. Like many other founders of modern social science, including Max Weber, George Herbert Mead, and William James, Durkheim started with what he found in the world and attempted to make sense of it through categorization and gradual building toward higher levels of generalization. For the mathematician to arrive at a simple identity after progressively simplifying a series of more complex equations might indeed represent an advance. Pure mathematics, however,

is concerned with neither causation nor empirical science. The logic of inquiry is completely self-contained and the terms established by the mathematician himself or herself. The scientist attempting to explain regime change, social conflict, political violence, child behavior, or the behavior of amoebas is, by contrast, concerned with both causation and empirics. The logic of his or her inquiry is not self-contained and the terms cannot be established exclusively by the scholar himself or herself. To the social or natural scientist, is there really anything so arduous about showing that X indeed equals X? Though it is currently a dangerous heresy to say so, finding that individuals act selfishly because they are selfish is perhaps a slightly less spectacular triumph than it is sometimes purported to be in contemporary American political science. Reading Durkheim reminds us, moreover, that to accomplish this task after beginning with the *assumption* that people act selfishly may render the achievement even less overwhelming.

None of this is to say that genuinely deductive reasoning has no place in social science. But the most valuable work of this type takes the form of—and contributes to—pure theory. It usually

requires truly advanced mathematics rather than elementary algebra that merely restates prose. Its contribution is precisely in its ability to suggest counterintuitive possibilities and to provide logical underpinning for nonobvious conclusions that may subsequently be tested against empirical facts (for example, Powell, 1996; Acemoglu and Robinson, forthcoming). Durkheim did not fully appreciate this possibility, but social scientists’ capacity for building models that uncovered and explicated nonobvious relationships was more modest a century ago than it is today.

But for all of his skepticism about axiomatic-deductive reasoning, Durkheim was no advocate of naïve hermeneutic methods. Durkheim opposed not only purely deductive reasoning but also “introspective” or “inward-looking methods” that involve accepting what participants themselves say about causation. Durkheim took what the subjects of his study said and did and their interpretations of their own words and actions very seriously. But he vigorously resisted accepting their own explanations as scientifically valid. In a polemic with a historian who embraced such methodology, Durkheim argued that “causes, pointed to

by the participants...must generally be held to be very suspect hypotheses" (*RSM*, 212-13).

Functionalism and methodological individualism

Durkheim was, despite some interpretation to the contrary, the quintessential antifunctionalist. He insisted that "when one undertakes to explain a social phenomenon the efficient cause which produces it and the function it fulfils must be investigated separately...social phenomena generally do not exist for the usefulness of the results they produce" (*RSM*, 123, italics in original). Durkheim's antifunctionalism cut so deeply that he generally rejected interest theories. He saw the needs of self-interested individuals as entirely insufficient as explanations of social life. Durkheim's rejection of individual-interest explanations was rooted not only in his antifunctionalism, but also in his well-known opposition to methodological individualism. He argued that "society is not the mere sum of individuals, but the system formed by their association [which] represents a specific reality which has its own characteristics" (*RSM*, 129). He regarded mutual interest among individuals as a grossly insufficient basis for

creating society and holding it together. His view of society as far more—and much different—than the sum of individuals, and as constitutive of the individual (rather than the reverse), fed his opposition to methodological individualism.

Durkheim's objections to both functionalism and methodological individualism placed him further at loggerheads with the economists of his day and with the advocates of rational actor models in our own. In the logic of rational choice, committees in Congress take the forms they do because such forms best serve the reelection interests of the incumbents. Political parties are configured the way they are because such an arrangement serves the electoral aspirations of their leaders. Such individual-interest functionalism differs from the déclassé structural functionalism that no contemporary American political scientist would associate himself or herself with to save his or her own life. But according to Durkheimian reasoning, the former is functionalism all the same.

Just imagine young Durkheim on today's job market. He has just completed his dissertation, *The Division of Labor in Society*, and obtains an interview in a leading department of political science

or sociology. Durkheim's happy circumstance resulted of course from a rave letter from a prominent adviser, in this case Emile Boutroux of the École Normale Supérieure. During his job talk, Durkheim claims to have hit upon an important idea and proceeds to lay out his theory of the division of labor and to flesh out his points with illustrations. But there are problems. At the outset of the talk, the young scholar admits that he arrived at his grand ideas "inductively"; he even defends "inductivism" as a fruitful alternative to axiomatic-deductive reasoning! The conspicuous absence of a formal model in his work only adds to his appearance as an atheoretical fact-grubber—or, at best, someone whose advisers did not prepare him fully for the rigors of the profession. To make matters worse, he questions the adequacy of methodological individualism for the study of collective behavior. Then, when asked during the question-and-answer period by a very skeptical and very well trained graduate student why he did not work harder to proliferate the number of observations in a particular area of his empirical work, young Durkheim answers with words that he would put

down on paper in his final major work several decades later. He states that he didn't have enough information to engage in the required multiplication of observations and adds apologetically: "We can usefully compare only facts that we know well." With that, he promises to gain a firmer grip on his data in order to engage in the necessary proliferation. After being reassured by the same acute graduate student that his research design, like almost any qualitative research design, could be reformulated into one with many observations, the interviewee shows some relief but nevertheless

artlessly blurts out: "The quality of facts is much more important than their number" (*The Elementary Forms of Religious Life*, 1995, 91, 92). Naturally, faculty and even some graduate students begin quietly filtering out of the room. Why stick around for the rest of this ordeal?

Well, as we all know, not every junior search turns up a future superstar.

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Book Reviews

**Arend Lijphart's
*Patterns of
Democracy:
Government Forms
and Performance in
Thirty-Six Countries*
(Yale University
Press, 1999)**

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Arend Lijphart's recent book *Patterns of Democracy* is more than just an updated version of his widely-read 1984 book *Democracies:*

Patterns of Majoritarian and Consensus Government in Twenty-one Countries (Yale University Press). In the earlier volume, Lijphart introduces an analytical framework consisting of the institutional features of democratic governments, such as their constitutional types, electoral systems, and party systems. In *Patterns of Democracy*, he revisits that analytical framework and changes some of its institutional characteristics. Furthermore, Lijphart's new book contains data from 36 democracies (including several non-Western ones), as

well as new data from recent years. The addition of more data from more cases makes the author's comparisons more interesting and his conclusions more generalizable. Perhaps the most important change, however, from *Democracies* lies in how Lijphart explicitly addresses what he calls the "so what?" question: "does the type of democracy make a difference for public policy and for the effectiveness of government?" (xii). As expected, Lijphart answers in the affirmative, but some readers may not fully agree with his con-

clusions on *how* institutions matter. In any case, *Patterns of Democracy* will be useful to many readers, from undergraduates who will use it as an introductory textbook in comparative politics courses, to graduate students and advanced scholars who will benefit from the book's useful analytical framework, helpful literature reviews, and provocative questions (and answers).

The "patterns" in this book's title refer to the features of political institutions, which are compared against two ideal types: the majoritarian (Westminster) model and the consensus model. Lijphart has taken ten governmental characteristics and grouped them according to what he labels the "executive-parties" dimension (single-party majority cabinets versus multi-party coalitions; executive dominance versus executive-legislative balance of power; two-party versus multi-party systems; majoritarian versus proportional electoral systems; pluralist versus corporatist interest group systems) and the "federal-unitary" dimension (unitary versus federal government; unicameralism versus bicameralism; flexible versus rigid constitutional amendment; parliamentary supremacy versus judicial review; independent

versus dependent central banks). Within this framework, Lijphart is describing how power is concentrated or dispersed; the executive-parties dimension refers to areas of joint responsibility, while the federal-unitary dimension applies to areas of divided power. Individual institutional features, such as electoral systems, party systems, legislatures, constitutions, federalism, and central banks are then discussed in detail throughout the chapters in the middle of the book.

The comparisons made in this book will be meaningful to more readers than those in *Democracies* due to the inclusion of more countries, particularly since the new cases are mainly non-Western. This means that the generalizations made about the effects of political institutions in Western democracies are now being tested on a broader group of cases. Countries that were democratic continuously between 1977 and 1996 are included this time, meaning that a great deal of geographic and cultural diversity is represented. Along with the advanced industrial world (Western Europe, North America, Japan, Australia, and New Zealand), cases from Latin America (Colombia, Venezuela, Costa

Rica), the Caribbean (Bahamas, Barbados, Jamaica, Trinidad and Tobago), Asia (India and Papua New Guinea) and Africa (Botswana and Mauritius) are now included. The greater diversity and number of cases, along with the institutional focus, make Lijphart's book useful as a text for general comparative politics courses that do not exclusively focus on Western democracies.

Lijphart's description and analysis of the institutional features that form his patterns of democracy is highly informative, yet clear. The examples provided illustrate his points well, making the book suitable even for introductory undergraduate courses. Throughout his tour of political institutions, Lijphart cites and reviews the pertinent literature, both the "classics" and some more recent works. This part of the book is also a convenient source for looking up important measurement formulas in the institutional literature, like the effective number of parties, the implied electoral threshold, and the index of electoral disproportionality. This attribute will be useful to graduate students and scholars who wish to find out more about a particular research topic. Most of the tables and charts Lijphart uses

to help explain his points are easy enough to understand, for graduate as well as undergraduate students.

The last chapters of *Patterns of Democracy* deal with the crucial “so what?” question that has plagued scholars who claim that institutions matter. Lijphart boldly tackles this question head-on. He questions what he calls the “conventional wisdom” among many of the scholars who study political institutions. They believe that governments cannot be both highly representative and effective at governing; there is a trade-off involved (258). Lijphart cites research which has tested this “conventional” assumption empirically, and conducts his own tests using macroeconomic and political violence data. He concludes that majoritarian governments are *not* necessarily better at the management of the economy and civil order than governments of the consensus model (274). However, he does agree with the conventional wisdom’s point that governments of the consensual variety are more representative of their populations, and thus promote a higher “quality” of democracy (275). Lijphart’s assumption that a high quality democracy has “kinder, gentler” policies (they have higher

welfare spending, are more environmentally friendly, and lack the death penalty, for example) is likely to be controversial, however. Quality of democracy is in the eye of the beholder, and some readers may hold a different view of what constitutes quality than Lijphart. Other traits of Lijphart’s high quality democracies (such as high voter turnout at elections, a large number of female legislators, and congruence of government policies with voter preferences) are bound to be less contentious, but critics will still argue about whether the institutions of the consensus model of democracy can be given full credit for what most people would consider to be desirable outcomes.

While some readers may consider Lijphart’s own definitions of democratic quality a shortcoming of this book, many others will agree with him or at least embrace his effort to show that there is a relationship between a democracy’s political institutions and its policies. *Patterns of Democracy* is an important new addition to the growing list of publications which systematically address the question of whether institutions matter. Not only does Lijphart conclude that institutions do matter, but he challenges the North American

conventional wisdom on *how* they matter. Lijphart is once again questioning the status quo, as he did in the 1970s with his works on the advantages of consociational democracy. The findings of this new book challenge those who prefer the majoritarian model of democracy to provide convincing evidence of its superiority (or of the shortcomings of the consensus model), rather than falling back on the anglophilic sentiments that have historically characterized the outlook of many US political scientists.

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