

miss much that the ROAD project can provide. To put it differently, if you were an ambitious graduate student in the late 1940s or before interested in the quantitative study of American politics, you would probably be drawing maps, doing detailed studies of local politics. If, instead, you (like almost everyone in the field today) started any time during the second half of this century, after Robinson's (1950) ecological fallacy article and following the advent of modern survey research, you likely became a survey researcher. Today, the literature is dominated by survey analyses, but with new aggregate data and methods, we all have many new opportunities to redress this imbalance.

- For the first time, scholars will be able to study data from numerous offices at many different levels of aggregation—from precincts, to state assembly districts, to state senate districts, to U.S. House districts, or to states. (Counties and other aggregation levels are also possible.) Even without survey data, this will make it possible to study how the same voter groups cast their ballots across many different offices. ROAD data will

enable more detailed studies of split ticket voting and of the factors leading to divided government at many levels, for any or all states.

- The ROAD data should make possible many new studies of legislative redistricting, and associated analyses and forecasts of political and racial fairness, compactness, the consequences of equal population constraints on gerrymanders, and related issues.
- Finally, this is the first data set to be generally available to the academic community that is on par in terms of quality and quantity with the data politicians and political strategists have been using for decades to target campaign resources. As a result, this data set could also produce new, more detailed studies of campaign strategy, but on a massive and comprehensive nationwide scale.

In part because this data set is of such exceptional value, and in part because it would take many researchers many lifetimes to exploit it fully, we are releasing it prior to publishing much from it. The data have been deposited in the ICPSR. For further information, you can

find a copy of the documentation and data at <http://data.fas.harvard.edu/ROAD/>.

Notes

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1. The ROAD team, in addition to King and Palmquist, has included at different times Greg Adams, Micah Altman, Kenneth Benoit, Jeffrey B. Lewis, Claudine Gay, Russ Mayer, and Eric Reinhardt.

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Crosscutting the Subfields: Learning from Our Colleagues

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When sawing a log or carving a roast, one can go with the grain or cut across it. The difference is that cutting across the grain requires more effort. So it is with patterns of thinking about our research. It is easier to draw on familiar sources—to go with the grain—than to reach out for ones with which we are less accustomed.

We can, however, enrich our own research by drawing more frequently on "experiences from elsewhere." Americanists, for example, generally could profit by reaching cross-nationally for alternative models of institutions and processes. Indeed, if we all were more systematically com-

parativist in approaching our favorite research topics, we probably all would benefit. In this article, however, I wish to focus on a different type of "experience from elsewhere," one that often escapes us: exposure to substantive subfields other than our own.

We all, at times, fail to use relevant findings and methods from other disciplines and subfields, although our failure to do so often leads to generalizations of limited domain, which in turn hinder (indeed, defeat) efforts to develop more general theories of politics. We persist in this failure even though major works in our discipline have

demonstrated the importance of borrowing from other disciplines, whether it be sociology and social psychology, for major voting behavior studies, or economics, for public choice analysis. (Indeed, much of the success of political science as a discipline may be attributed to the fact that many of its practitioners have been effective scavengers from other disciplines.)

Drawing—or Not Drawing—on Other Subfields

The general proposition that those in one political science subfield

could profit from exposure to, and use of, their neighbors' work is not new. For example, in the APSA's own first state-of-the-discipline collection, Baum called on his public law colleagues to increase the value of their contribution to political science by using material from outside their subfield: "Studies of judges' role orientations do more to build general knowledge when they draw from research on other policymakers' roles and put their findings in the context of what we know about legislators and administrators" (1983, 205). Reminding ourselves that we should borrow both from other disciplines and from other political science subfields is still valuable, and is particularly timely because, unless we consciously work to combat it, our isolation within our own subfields is likely to become greater as specialized Internet mailing lists and the like reinforce existing fragmentation and separation.

The problem is not so much that we are not taught to read outside our discipline or subfield, although this is often the case; it is more that few graduate advisers have forced us to do so at the point in our careers when it might have become a habit. We have, in short, not been socialized to reach beyond the boundaries of the fields with which we identify. We may well be aware that there is material "out there" which may be of help, and are almost certainly *capable* of finding source materials wherever they appear. However, we are not in the *habit* of looking for materials off "tried-and-true" paths. Additionally, after our graduate training, we often have relatively little exposure to scholarly work outside our own "piece" of the discipline beyond what we may be forced to learn in preparing a basic course like American Politics. We remain captives of the information and perspectives we possess, and are victims of the information and perspectives we lack.

If, for whatever reasons, we have attempted to seek out material that is foreign to us, we face many obstacles. With the many new books and journal articles we must absorb just to stay on top of our own field, time to read outside it is difficult to find. Simply learning about what one

should consider reading is difficult. One reason for this is that publishers often decide to advertise a book in some disciplinary catalogs but not others (for example, sociology but not political science); another is that scholars often lack access to journals' tables of contents in disciplines related to their own, although the Internet may provide improved access to them. Whatever the reasons, the result is that individual scholars very often miss material which would assist them in their current research, and the entire discipline suffers as the same wheels are invented several times, each with different wobbles, in different fields.

The failure of scholars in adjacent subfields to draw on each other's work on nearly identical topics became concrete for me more than a decade ago while I was preparing a course on Policy Impact and Implementation, which stemmed from my early interest in the impact of the Supreme Court's decisions (Wasby 1970). The literature on policy implementation had grown exponentially since I had last reviewed it thoroughly, so I examined it as well as newer public law scholarship on impact. Not only did neither body of literature speak to the other, each was constructed in apparent lack of awareness of the other. On the public law side of the fence—for it does seem as if there is a fence between subfields, and not the type that makes good neighbors—the then-current leading book on the Supreme Court's impact (Johnson and Canon 1984) drew only on earlier work by public law scholars. With the exception of a chapter on the impact of judicial decisions in Nakamura and Smallwood's (1980) text on policy implementation, all the books on policy implementation which I reviewed had barely more than a footnote referring to the judicial impact literature, which considerably predated the implementation literature.

Both sets of researchers failed to interweave material on judicial impact with that on implementation of policy developed by other institutions even when the factors—like clarity of policy or the extent to which policy provides for implementation—used by one set of research-

ers were very much like, or certainly compatible with, those used by the other set. This basic problem persists although, as Baum (1997) suggests, students of the implementation of judicial rulings have begun to make use of the more theoretically sophisticated work of scholars who study the implementation of other (non-judicial) policies, such as principal-agent theory. Similarly, Barclay and Birkland (1997) have shown how major studies in the policy literature—like Kingdon's (1995) work on political, policy, and problem streams and Baumgartner and Jones's (1993) use of "punctuated equilibrium" to describe the policy process—can be of utility in explaining courts as policymakers.

The reasons for the continued separation of the policy implementation and impact literatures are not difficult to discern. One reason, suggested by Barclay and Birkland (1997), is that public policy researchers do not see courts as policymakers while public law scholars have long believed them to be such. The former, having decided a priori that courts are restricted in their ability to formulate policy and only react to policy developed elsewhere, relegated courts to the implementation phase of policy making. Another reason is those who study the impact of judicial decisions were trained to focus on the U.S. Supreme Court, while policy analysis scholars, because they came from backgrounds in Congress, in public administration with its focus on the executive and bureaucracy, or in substantive policy areas in which the president and/or Congress are the prime movers, have generally been unfamiliar with the judicial impact literature. When the policy area was one, like civil rights, that was significantly developed by the judiciary, its students were likely to be in public law and thus likely to draw almost exclusively on that subfield's literature.¹

Whether or not one agrees with this diagnosis, the result is clear: those in one subfield have stayed within it and have consistently failed to draw on the rich, related literature of their political science colleagues in other subfields. One can bring together people from different backgrounds to work fruitfully (see

Grumm and Wasby 1981), but such joint endeavors are not frequent, since scholars in different fields are not likely to know each other. This especially true in political science, where the APSA's Organized Sections' institutionalize subfields, thus further reinforcing the separations among them.

Examples of Learning from Others

Counter to the above tale of separation and self-encapsulation, my recent reading prompted me, in the hope that others would look further afield than they are accustomed, to offer the following personal account because I believe such specific instances of learning from neighbors within the discipline may help convince readers to turn an infrequent practice into something closer to a habit. Although my own research has been predominantly in public law and civil rights policy, with some work on the legislative process, from which I draw my principal examples here, I write this not to speak to my public law colleagues but to suggest that *all* members of the discipline can benefit from paying more heed to subfields other than their own. I am certain that readers, regardless of subfield, can easily supply their own examples of works outside their own subfield that would add valuable dimensions to their research.

Over the last eighteen months, I read two books, which I came to in different ways—one, with the specific purpose of determining whether it might contain something of value for me for a project I was only beginning to undertake, and the other, solely because of an interest, apart from my public law work, in the subject matter. Because I hit the proverbial jackpot with both, my experiences serve to underscore the points that scholars of the judiciary can learn much from those with a focus elsewhere in American government and politics, and, by extension, more generally, that those in one subfield can learn from other subfields. (If I could find much in another part of the discipline that was rewarding, so could others.)

I will discuss first the book I se-

lected to further my research. We have taught that the United States does not have a career judiciary like the European model where one opts to become *either* a lawyer *or* a judge; instead one becomes a judge *after* having been a lawyer. After conducting research on a judge who had served first as a state trial and appellate judge and then as a federal trial and appellate judge, I had become interested in judges' career patterns, the increasingly clear paths which American judges follow within the judiciary. I had observed these patterns in recent presidents' practice of nominating to the U.S. Supreme Court almost exclusively individuals with prior U.S. Court of Appeals service, and a similar pattern had been identified for appointments to the New York Court of Appeals (Bierman 1994).

Although political scientists have given much attention given to judges' backgrounds *prior* to their judicial service, they have not studied patterns of movement within the judiciary. So, where to look for relevant ideas? An obvious starting place was work on other politicians' careers. What beckoned was a book by Williams and Lascher (1993) which drew on Schlesinger's (1966) study of political opportunity structures. This new collection, by providing numerous statements about legislators' career paths through summaries of previous research and findings from an original study, prompted many thoughts about the applicability of the material to a study of career paths in the judiciary. The observations or conclusions could be applied in some instances with relatively little modification, in others with some stretching.² Finding Williams and Lascher's book so helpful repeated an earlier experience I had of annotating Cook's (1989) study of media use in Congress, furiously—not in anger, but in the speed with which I filled the books margins with comments about its applicability to the judicial process, particularly with respect to what Cook had to say on the media influence on setting an institution's agenda, which was, although of course with appropriate differences, parallel to what one would say about media treatment of the courts.

As a court-watcher, I have thus been helped more than once by those who watch legislators rather than judges. I might grumble that the Williams and Lascher volume would have advanced our knowledge of politicians' careers even further had they included judges in their analysis and chide the authors that a book on "Career Paths of American Politicians" should not have been restricted to legislators but should also have included judges because, after all, we have known for some time that judges *as judges* are politicians of a sort and that many have been politicians holding elective office before they took the bench. (We have frequently heard the old saw, "A judge is a lawyer who knew a governor or a senator—or a party boss.") Likewise, Cook could have contributed broader generalizations if he had explicitly compared media relations to the U.S. House of Representatives with media relations to the judicial branch, a task which would not have required him to conduct a separate full study. These would, however, be minor grumbles, *sotto voce*, because the books, offered so much to someone in a different field.

I came to the book with the "accidental" application to my work because of my interest, stemming from my time as a Congressional Fellow, in legislative staff, a topic I always included in courses on Congress or Legislative Process. Because I knew that the staff in the offices of members of Congress were now much larger and more specialized than they were thirty years before, I was interested in possible differences between the present and that earlier, less complex time. Thus the title *Communication in Congress: Members, Staff and the Search for Information* (Whiteman 1995) hooked me immediately, although my own research on communication among federal appellate judges (Wasby 1977, 1987) also increased my interest in the title. To my pleasant surprise, the book did not only teach me more about the role of legislative staff in the policy process, as I expected it would, but it also contained an element directly relevant to my then-current research—the notion that one should focus not on the

legislator alone but on legislator *and* staff, who constitute an *enterprise*. Applications of the enterprise notion to judge *and* clerk, or judge-in-chambers, rather than judge alone, were immediately crystal-clear because I was in the midst of examining case-files containing communications between law clerks and judges.³ Whiteman's work was applicable to judicial biography because public law scholars most often look at individual judges and not at their law clerks' roles, even when the clerks have been interviewed about their "boss." Likewise, the focus has been on the *judge's* vote and final published opinions, not preceding activities including law clerks' benchmemos and opinion drafts. Whiteman's argument properly forced me to change my mindset so that I would view a memo from "Judge X" as really being from "Judge X, who has (perhaps) modified a draft memo prepared by Clerk A."

The lesson to be learned here, even if it is obvious, is that, at a minimum, useful material from subfields other than our own does exist. It follows that we should be reading outside narrowly-constructed boundaries and we should be open to draw on what specialists in other subfields have to say. Even when their findings are not fully a propos our own research because of differences in situations or institutional structure, their work can nonetheless provide us heuristic guidance and much more.

Obstacles to, and Suggestions for, Change

Obstacles. We must keep in mind that trying to make a habit of engaging and employing ideas from other subfields will require cutting against long-standing habits and against institutional inertia. One problem is that there are few incentives to prompt us to go outside standard boundaries—and perhaps some disincentives for attempting to do so, which we have been "taught" explicitly and implicitly. As the rewards we receive from our own departments and the respect from our colleagues derive from our recognition within our own subfields, there is less pay-

off from working across disciplinary or subfield boundaries. Someone seeking tenure or promotion also can ill-afford to "waste" effort by publishing in journals not familiar to those who are most likely to evaluate his or her work. Because interdisciplinary journals are often seen as marginal to each of the disciplines they encompass, there is a further disincentive to submitting material to them even when they are the principal source of stimulation and insight for those who read them and even if their sponsoring organizations are those hardy souls' primary professional "home."⁴

Many journal editors often reinforce our inclination to stick to the tried-and-true through their disinclination to publish what is for them nonparadigmatic material, primarily as a result of unfamiliarity, reinforced by habit. One editor may be comfortable with your theory but not the institution to which you are applying it, while another knows the institution but not the theory. Even editors personally inclined to publish "innovative" material are limited by their need to serve the center, not the periphery, of their constituency. Manuscript reviewers who downgrade submissions not within standard paradigms further hinder innovation.

Suggestions. After presenting the foregoing litany of difficulties, proposing any options may seem foolhardy, but one must start somewhere, and readers will undoubtedly be able to add even better options or variations on these modest proposals and to suggest ways of making them practicable. One possibility, perhaps less difficult than others to put in place, would be to encourage each journal to publish mini-reviews of books and articles outside its normal scope, or to call attention to other journals' reviews, particularly of volumes which are important to, but not central to, the interests of the journal's primary readers. For example, as Review Editor of *Justice System Journal*, borrowing an idea from a practice I had seen years before in some journals outside political science, I developed summaries of articles from journals not usually read by the *Journal's* regular readers

(those interested in court administration), and of research reports from government and not-for-profit organizations.

Another possibility would be to use professional meetings to begin the process of cross-cutting, by encouraging unlikely combinations of special sections to sponsor panels on topics to which the several sponsoring subfields would have something to contribute. Implementing this suggestion might encounter the same resistance engendered by the recent addition to political science meetings of poster sessions to increase the number of research projects presented, even though psychology associations had long used them. One could, however, start with only a few such cross-cutting sessions without displacing the usual talk-within-our-own-subfield panels which constitute virtually all of contemporary professional association conventions.

A more substantial accomplishment would be to make it acceptable, or even rewarded, for an individual to publish similar articles in different journals with different audiences. Some scholars already engage in this practice, but perhaps less from a desire to expose other audiences to their work than from a desire to find an outlet—any outlet—that will accept their work. If that is the present motivation, we must now make writers understand the need to expose those outside their own subfield to their work and to recognize the possibilities of receiving stimulating feedback from people whose views and perspectives they would not usually obtain. One problem to be faced here is that publishing multiple, only slightly differentiated versions of the same article is frowned upon, so those seeking such publication should be explicit about their desire to expose new audiences to their work.

With that "short list" of possible ideas as a start, we must go forward. We *must* do something to look for materials of relevance outside our own limited domains, and must not continue to rely, as we have for so long and with too few exceptions, on the tried-and-true within our own subfields and narrowly defined specializations. There is much out there which will illuminate our own work,

just as we can illuminate that work in turn. Get out your crosscut saws and saw away!

Notes

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1. Cigler (1997) has noted a similar phenomenon in the study of interest groups: Most interest group scholars tend to come from either a tradition of seeing political parties and interest groups as mediating entities or from voting behavior research (leading to an interest in PACs). They thus pay relatively little attention to interest groups in courts, and leave such activities out of the larger picture. Public law scholars, on the other hand, conduct most research on interest groups in court, but without adequate use of the broader interest group literature.

2. An example: Does it matter that U.S. district judges can seek elevation to the U.S. Court of Appeals without leaving the district court, while members of the U.S. House of Representatives must give up their office to seek a Senate seat?

3. See Romzek and Utter (1996) for another study highly useful in prompting thoughts of the differences between legislative and judicial staff.

4. While those conducting social science research on the law have access to several such journals, particularly *Law and Society Review* and *Law and Social Inquiry*, comparable journals may not be as readily available in other subfields.

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The Future of Our Discipline: The Status of Doctoral Students in Political Science

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The future of our discipline rests in the hands of those individuals still pursuing their doctorates. The way we train our doctoral students to handle their roles as future professors will have a lasting impact on the discipline. When graduate students make the transition from student to professor they are supposed to have

learned the skills they will need to be successful teachers and researchers. However, it is unclear whether graduate students in political science receive the training they need to immediately assume these roles. This article is intended to be a first step in a long-term scholarly inquiry into the status and training of graduate

students preparing for careers in political science. In particular, we investigate the extent to which graduate students have already begun engaging in professional activities such as teaching, presenting at conferences, and submitting manuscripts to scholarly publications.

Specifically, we examine three re-