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In Search of Policy Theory

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In this brief essay I argue that the field of policy studies, like much of political science, has been insufficiently ambitious in its efforts to develop systematic generalized theory. I first offer some observations about why that seems to be the case. I discuss, second, a series of largely unoriginal ideas for how we might go about achieving greater theoretical progress. While these ideas are widely argued to be common staples in the larder of any theoretically ambitious social-science discipline, they are also ones I believe we ignore more frequently than employ in our policy scholarship. Thus explicit consideration of their relevance for policy research appearstimely. Finally, the essay proposes some pedagogical practices that comport with, and might enhance in several ways, the preceding recommendations for research.

I would be uncomfortable to be seen as passing judgment on this broad and eclectic field of study, where, doubtless, wise scholars in policy subfields quite different from my own recognize progress of which I am not aware or that I do not appreciate. The principal motivation for this essay is quite different: it is my criticism of my own scholarship—largely concerning democratic and representational processes as they affect public policies in America. Much of my own work exhibits the failure of ambition described in this essay, and reflection on that fact has led me recently to pursue some of the strategies for improvement described here. Yet I also admit to some frustration with the limited theoretical development generally in this field. And my concern for the success of other scholars is entirely self-interested: the greater the theoretical progress that others make, the better my students and I can understand the policy process. Thus this essay also affords the opportunity to suggest theory-building strategies for those

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studying different policy topics and using other methodological approaches.

Why Has Our Theoretical Progress Not Been Greater?

A host of reasons likely account for our limited progress. One of the most important is, in my judgment, that many policy researchers are not interested in theory. Instead, perhaps the majority of academic students of the policy process are interestedin contemporary policy issues or the immediate activities of policy making institutions. Nor would I equate such interests with history or journalism as some might. Informedconsideration of applied policy matters has a strong and legitimate tradition in political science. Indeed, such considerations were paramount in Lasswell's (1951) seminal review of the policy science field and his argument about how scientific research might inform actual policy making. Such scholarship is important for those of us interested in theory development, as well. We require a solid descriptive understanding of our subject matter as a necessary base for the theoretical abstractions to be derived from it. Those interested in applied policy issues provide us with much of that understanding. Yet my central point here is that the amount of scholarly energy devoted to theory building is far less than the popularity of policy studies in political science would suggest.

Second, I believe that a large number of policy scholars consciously or subconsciously suspect that the policy process is much too complex to succumb to generalizable explanations—even when one's ambition is restricted to "middle range" theory about selected components of that process. I suspect that such suspicions motivate the authors of many policy case-studies, as well as many scholars who produce "soaking and poking" analyses of one or another policy making institution or episode.

Some scholars doubtless produce case studies or soak their scholarly interests in the rich texture of particular institutions because that is what they find of special interest and intrigue. Yet at the heart of much of this research there frequently are assumptions about the inherent irregularity, disorderliness, or instability of the subject matter under study. I am not aware of a fully crafted expression of this assumption in the policy scholarship per se, but we can find quite representative examples of it elsewhere in political science. Almond and Genco's (1977) argument for the "cloud-like" as opposed to "clock-like" properties of political phenomena could just as well have been written by a student of the policy process. Riker's (1980) concern for whether equilibria are common in political processes is equally relevant to many views of the policy process. Doubtless, every thoughtful scholar has his or her favorite examples of such sceptical statements about the orderliness of political phenomena. And such scepticism fosters uncertainty about whether these phenomena are amenable to generalized explanations.

Despite the preceding concerns, many students of the policy process unequivocally proclaim an interest in theory building, and they assume that there is sufficient orderliness in their subject matter to make that possible. At a minimum I write, then, for the latter scholars. Yet even Almond and Genco, Riker, and many others who share their views argue that certain kinds of systematic knowledge of political phenomena is possible. Perhaps policy scholars who share their beliefs about the inherent irregularity of such phenomena can also be more ambitious about their own versions of theory by considering some of the proposals here.

I believe, third, that we in the policy studies field are too prone to faddishness-like the rest of our broader discipline. Many of us are seduced, first, by trendy new theoretical approaches. Principal-agent models certainly offer an example of that. In light of Carmines and Stimson's (1989) and Baumgartner and Jones' (1993) stimulating research with punctuated equilibria models, we might expect to see a host of those in the future, as well. Similarly, Lowery and Gray's (1995) clever use of a population ecology model to study interest groups in the American states will likely stimulate a host of theoretical camp followers. The promise of such approaches is that they may, indeed, provide new insights about one or another component of the policy process. What such trendy approaches typically deliver for theory building, however, is often much less because there is usually insufficient sustained research with any particular one of them. As with Andy Warhol's conception of pop-culture fame, their scholarly popularity is typically too brief.

Doubtless, we're faddish too with research methods. As but one example, many of us who work in the state politics and policy field have recently seized upon pooled-time series models—with the result that their use is becoming both wide and indiscriminate. Whatever their legitimate strengths for theory testing, these models pose formidable methodological problems as the sophisticated statistical literature (Greene 1993, 444-485) and recent applied investigations (Beck and Katz 1995) indicate. Time-series models more generally may, of course, come to enjoy the same fashionable status. Yet the risk here is not quite the one that is typically assumed and lamented. Hammers, in skilled hands, are wonderful tools for certain tasks. Yet they'll never replace a saw or lathe or level. So it is with statistical tools. And faddish use of such tools tends to obscure these distinctions.

In my judgment we're too quick to jump on the latest trendy policy controversy, as well. Eagerness to switch our collective attention to new policy controversies leaves us at risk of ignoring what's enduring for what's ephemeral. In my view we already devote too little sustained inquiry into enduring questions. And by devoting a great deal of attention to trendy subjects we aggravate that problem. A central argument of this essay is that seldom do we adequately explore the theoretical opportunities presented by enduring questions about the policy process, much less those arising from new theoretical models or topical policy questions.

A fourth general reason for our lack of progress is what I term theoretical timidity. Most of us have a comfortable research niche where we typically pursue one of four common research strategies: (1) the proposing of elaborate theoretical arguments about some aspect of the political process with only the most cursory and superficial tests for empirical verification (is our field the last stand of "grand theory" based on scant or no empirical evidence—after the seeming demise of that kind of scholarship in comparative politics?); (2) intensive, qualitative analysis of one or a small number of policy cases of unknown representativeness; (3) the analysis of additive, linear main effects relationships with multiple regression models or some statistical analogue thereto appropriate to our data—and for cases or samples of unknown representativeness; or (4) the creation of formal, deductive models whose relevance to the empirical world is left unexplored. Most of us pursue one or another of these avenues, further, with insufficient sustained attention to the development of generalized theory for a single, particular research question. This we call middle-range theory research.

In virtually all the empirical work under the first three categories above there is, first, far too little replication or concern for the representativeness of cases (even for those studied by quantitative scholars—who are frequently doing case studies of a kind themselves) and little recognition of how particular cases, measures of concepts, methods of data collection, or methods of analysis might shape our results. Thus the generalizability of any particular set of research findings is typically uncertain.

Worse still, the vast majority of us who do empirical work of any stripe are literally too timid about articulating and then testing more encompassing or generalized theory. Yet there are obvious reasons why we should do so. Our middle-range studies of single policy episodes, agencies, or policy areas may produce findings of limited generalizability. Political processes are also quite unlikely to be well-represented by the linear, single- or multiple-equation models we typically employ in such research, as various scholars have reminded us from time to time (Freedman 1991, Leege and Francis 1984, 23-25, McGregor 1993, but see Blalock 1991). Nor do policy processes necessarily "look like" what many case-study descriptions or grand-theory explanations suggest either.

Perhaps, this timidity arises because we are uncertain about what form more advanced theory should take, that is, what theory might "look like." Yet the general solution to that puzzle should be clear, even though different scholars will adopt different particular versions of it. The character of advanced theoretical formulations should be determined by our assumptions about the character of the real-world phenomena we are attempting to describe in abstract terms. We have a number of representations, too, of the character of social and political phenomena and, hence, of what more advanced theory might look like.

At one extreme there is the orderly view of the world implied in "covering law" models (Braithwaite 1953, Nagel 1961). Such models would appear to be the inspiration for a good deal of contemporary policy scholarship that explicitly claims an interest in theory building. Consonant with the assumptions of much of the latter research, covering law models imply that we can develop by the accumulation of systematic empirical evidence hierarchical, deductive systems of explanation for particular kinds of phenomena, with propositions higher in the hierarchy having higher degrees of generality. Yet the key assumption here is that political processes are sufficiently regular and orderly to permit some notable degree of such generalization and abstraction. While a fair amount of policy research seems inspired by this conception of the character of political phenomena, our theoretical timidity means that few efforts have been made at actually articulating and then empirically testing advanced explanatory systems of this kind. Most of us express high ambitions for eventually arriving at such theory, but we generally remain stuck in the niche of middle-range scholarship described above.

What appears initially at least to be an entirely contradictory view of the character of natural and social phenomena exists in the far more complex, if not entirely disorderly conceptions underlying chaos and catastrophe theory (Brown 1995, Kiel 1991). While particular versions of it. The character of advanced theoretical formulations should be determined by our assumptions about the character of the real-world phenomena we are attempting to describe in abstract terms. We have a number of representations, too, of the character of social and political phenomena and, hence, of what more advanced theory might look like.

Despite the fact that one can readily identify these alternative vies of social phenomena and, hence, of the possible forms of theory, it is only quite exceptional research that attempts to articulate and find empirical support for policy theory conceived in any of these ambitious terms. And I emphasize that advanced theoretical development necessitates the testing, as well as the conceiving, of theoretical formulations of high abstraction or generality. Merely conceptualizing a new theory about some policy phenomenon, perhaps illustrated with suggestive but not literally theory testing evidence, is as deficient as empirical scholarship that remains stuck in the middle-rage niche.

Deductive research typically does not suffer this theoretical timidity, and such work is model of sorts for the rest of us—at least in the theoretical scope, inclusiveness, and logical rigor of the best of that work. Yet, as many critics have observed, deductive modeling in political science has yet establish widely compelling evidence for many of its fundamental assumptions or sufficiently fruitful rapport with empirical scholars. The later rapport would benefit the discipline generally and even the deductive modelers themselves in ways many such scholars do not seem to appreciate. On the latter point Asimov's (1984, 9-10) observations about how a preoccupation how deductive theory stymied the scientific progress of the ancient Greeks is fascination reading.

Naturally, we should expect that the vast majority of early efforts at advanced theory will ultimately be judged failures. More specifically, they will either be found wanting in their ability to account for intended policy phenomena, or they will reveal gaps in our empirical knowledge that are unexplainable by any general theory. But these outcomes would themselves be milestones of progress for policy theory. As Popper (1959) argues, the route to advanced scientific knowledge is by way of the falsification of theory, rather than through its confirmation. Failure is success.

Strategies for Advancing Policy Theory

Unlike Sabatier (1991) I do not believe that only a few selected research approaches will be highly productive for engendering systematic theory. Indeed, I suspect that the history of most young sciences demonstrates that a diversity of approaches—with dialogue among their separate practitioners—is the most fertile avenue. Thus policy theory would likely benefit from continued diversity of research approaches. Doubtless, too, will profit most from concentrated research on subfields of policy study—an "islands of theory" approach, one might say. But more sustained and systematic research than we have typically seen in the past will be necessary to engender significant theoretical progress.

The suggestions below indicate what I mean by sustained and systematic research. What follows, then, in light of the preceding observations are my recommended, largely unoriginal notions about how we might help this subfield mature more quickly.

More Replication

I see little recognition in the policy sciences subfield of the common admonition that the generalizability of any particular social science research findings should typically be suspect because of the risk of sample-specific, measure specific, or other method-specific effects. The conventional, if little-practiced corrective for that problem is of course replication of research findings in different settings with different samples, measures, and methods. We do too little of such research.

Instead, the received wisdom of many subfields of policy research consists of a few landmark studies that have never been systematically replicated. Even where replication of any sort is carried out in our field, it typically results in a difficult-to-interpret confrontation of divergent results or, perhaps worse, a confirmation of a previous finding that still remains of uncertain generalizability. These problems arise in part because most replications are essentially single case-studies-just as are the studies they attempt to reproduce. The comparability of the cases is often uncertain, as well, producingequal uncertainty about the comparability of the theory tests and findings.

Witness as but one example of this problem some of the results of Waterman and Meier's (1995) critical evaluation of progress with principal-agent models in the study of legislative-bureaucratic relations. This seeming mini-industry of research might appear to some to be an exemplary case of the wide use of a particular conceptual model (regardless of whether one is sympathetic or not with the approach and its assumptions). Waterman and Meier's major criticisms of this scholarship are based on what they argue to be fundamental shortcomings of the theory in its applicability to political phenomena. For that reason their criticisms do not principally concern what we have learned from tests of the theory. Yet their arguments are based in good part on their reactions to the actual body of theoretical and empirical work with such models. And what's striking to me is how modest that body of work is.

Waterman and Meier cite four purely theoretical works and only seventeen empirical studies within the principal-agent research perspective that concern legislative bureaucratic relations. Further, some of the empirical works offer but the scantest of evidence for their arguments or pose only very elementary hypothesis or theory tests. Most of the others are unreplicated case studies, regardless of whether they offer qualitative or quantitative evidence. Thus we have here only the slenderest of empirical bases for reaching positive or negative conclusions about the utility of this theoretical paradigm.

Equally notable in policy research generally is the limited use of multiple measures, samples, or theory tests within a single piece of research. The most modest, although still quite laudable, version of such practice would be to use multiple measures of key concepts within a single study. A significant additional step would be to offer tests of hypotheses with more than one sample. Even more ambitious would be those analyses where several related, but different theory tests are executed with multiple measures within a single piece of research—much as Blalock (1969, 35-43) suggests by seeking inventories of theoretically expected causes and effects or as King, Keohane, and Verba (1994) suggest by testing “multiple implications of theory.”

Angela Hinton-Andersson and I have made an admittedly modest initial effort to employ several of these strategies to sort out some of the interdependencies in the state policy representation process (Hill and Hinton-Andersson 1995). We tested our primary causal model with numerous independent measures of the key concepts, and we replicated the model test, with appropriately distinctive theoretical expectations for the findings, with two different samples—in this case of state mass and elite groups.

Replication is, of course, not a highly valued practice in our discipline. We—and our journals—place a priority on new and distinctive research findings. The latter practice also induces most of us, myself included, to argue often for more newness and distinctiveness in some of our findings than is likely wise. Nonetheless, if we are clever about how we design and package replication studies, they may be highly salable in our academic marketplace. Of course, incorporating replication tests in our primary research endeavors will be the best way to make our work both salable and theoretically productive. Yet experimental efforts like that at the American Journal of Political Science to accept replications of previously published research should also yield notable theoretical knowledge.

Another rich opportunity for replication research lies in the ocean of masters theses and even doctoral dissertations produced annually on policy topics. It is a sobering exercise to read the occasional listing of dissertation topics published in PS and to see the enormous number of ad hoc and parochial research questions-seemingly little connected with anything but the author’s home town, home state, home country, or personal policy fetish—pursued in those works. What a gold mine of untapped energy and resources this is for the replication, even if in part alone, of leading works in the field.

More Comparative Analysis

As noted above, one strategy of replication is the testing of a single theoretical relationship in multiple samples or for multiple cases. That strategy deserves separate discussion. Again, we have a subfield built on the wisdom of a handful of landmark studies of single policy decisions, issues, and agencies. Yet the generalizability of such research findings across decisions, issues, decision-makers, institutions, and time is often unknown. The more the replication of research in such comparative terms the better, of course, but “more” often comes at a terrific price in research resources—money, graduate students, and your and my most precious resource, our all-too-limited time.

Here is where more ambitious collaborative research is called for. Consider some notable models—chosen broadly in political science because there are so few in the policy field alone. Important, but all-too-rare, examples of such research include classics like Bauer, Pool, and Dexter’s American Business and Public Policy (1963) and Wahike, Eulau, Buchanan, and Ferguson’s The Legislative System (1962) along with more recent studies such as Aberbach, Putnam, and Rockman’s Bureaucrats and Politicians in Western Democracies (1981), Heinz, Laumann, Nelson, and Salisbury’s The Hollow Core (1993), and Hrebenar and Thomas’ multi-scholar, fifty-state study of interest group systems (see Hrebenar and Thomas

1993 and related, region-specific compendia). These works feature laudable scholarly ambition of two kinds—in the scope of their theoretical conceptions and in the extensiveness of their data collection and theory testing. They make a strong case for scholarly strength in numbers.

Yet comparative analysis and replication of this kind is also possible without a host of immediate collaborators. Patricia Hurley and I are engaged in a research project to test a complex model of constituent-legislator representational linkages for three samples of members of the U.S. Congress—with both House and Senate legislative samples elected at different times across a thirty-year period. We rely heavily on data collected in a number of past scholarly investigations, and in that sense we have many "collaborators." But we have also supplemented each data-set for aparticular constituency-legislator sample with a number of newly created measures.

More Ambitious Case-study Scholarship

The policy subfield of political science has produced an abundance of case-study research, but the vast bulk of that scholarship has a considerably more applied than theoretical orientation. Even that case-study research which is concerned with theory has, in my judgment, paid modestattention at best to conventional scientific criteria for the development of generalized theory. Yet policy scholars in this methodological genre have an opportunity that few of them evidently recognize: they have the prospect of substantially shaping the theoretical agenda of the entire field.

The vast majority of policy scholars appears to recognize the depth, richness, and nuance that is featured in the best case-study research. And the proposition that especially meaningful hypotheses about policy processes can be derived from such scholarship is nearly equally accepted. Yet for case-study scholars to exercise such influence on the discipline they must regiment their work in more obviously scientific ways—with deliberate concern for the selection of representative cases, validity and reliability of evidence, conservative and skeptical assessment of the general implicationsof their intensive findings, and with explicit attention to articulating general propositions suitable for testing in more systematic, subsequent analyses.

The preceding goals for case-study research may sound intimidating or even preposterous to some, but they are ones for which the best methodological writings on the case method offer considerable practical advice. The most thorough-going discussion of how to employ such theory building strategies in case-study social science scholarship is that of King, Keohane, and Verba (1994). That work should be required reading for all policy scholars, and it should shape the ideas of those who pursue case study research in this field. Unfortunately, only a modest number of policy case-studies have employed such strategies to date. King, Keohane, and Verba themselves discuss two recent policy studies at some length because of their theoretical and methodological merits—Nina Halpern's (1993) analysis of the relevance of Stalinist theory for policy making in Eastern Europe and China and Helen V. Milner's (1988) examination of American trade policy in the twentieth century.

In my judgment William P. Browne's Private Interests, Public Policy, and American Agriculture (1988) and Cathy Marie Johnson's The Dynamics of Conflict Between Bureaucrats and Legislators

(1992) are two of the few additional examples of case-study scholarship that demonstrate both serious ambitions for theory construction and deliberate research strategies designed to advance that goal. Browne offers an impressive, intensive examination of the agriculture policy community and policy process in part to advance the general study of interest group politics. Yet by Browne's (1988, 67) own admission the theoretical work is limited by his desire to write for multipleaudiences, in particular, those interested in agricultural policy per se.

Johnson proposes a theory of the relationship between Congress and bureaucratic agencies that she argues is contrary to the conventional scholarly wisdom on that subject. She then derives a series of testable hypotheses from her theory, and she offers evidence for tests of the hypotheses from intensive case-analysis of the histories of four agencies. Johnson's work is admirable, then, for its theoretical foundation and its deliberate tests of theoretical propositions. If she is to be faulted on theoretical grounds, however, it is for the kind of timidity I mentioned above. She fails to seize the opportunity provided by her work to propose future research directions that might advance theory still further.

More Ambitious Use of Case-study Scholarship

Despite the limitations of existing case-study research, I suspect that it still might contain a rich lode of underexplored information useful for theory building. And I am frankly surprised that no recent scholarly work has attempted to develop theoretically informed generalizations by the systematic, secondary examination of a host of policy case studies. In effect, we have not attempted to use that body of knowledge in any fashion like one would with a meta-analysis (Wolf 1986). Perhaps, most of this scholarship is too particularistic for such an effort. Yet I suspect that there is considerable wisdom for some kinds of theory development that could be gleaned from existing policy case studies and then used to form tentative generalizations and to direct more systematic research.

A classic, if seemingly long-forgotten example of such work is Lowi's effort (1972) to find evidence for policy arenas generalizations in a number of separate policy case studies. In describing this work Lowi (1972, 304) observes that "our task was essentially to 'interview' each author by addressing certain questions to his case study." Both the theoretically informed method and the results of this analysis might serve as models for contemporary policy researchers.

More Commitment to "Programmatic Research"

The diversity of interesting research questions in the policy studies field encourages scholarly eclecticism. At least that is the most positive face I can put on my own tendency—and that of a host ofother scholars in the field—to examine many different questions rather than pursue a single line of research guided by a deliberate theoretical agenda. Unfortunately, we seem not to work any better collectively than individually in this regard. As I observedearlier, limited attention to replication and comparative research inhibit the systematic pursuit of most research questions to their theoretically logical end.

Doubtless, more theoretically directed research would produce greater scholarly progress. A model for such research in social psychology is proposed by Aronson, Brewer, and Carlsmith (1985, 480-481), explicitly because of the typically uncertain validity of measurement operations in that discipline as in all the social sciences. As Aronson, Brewer, and Carlsmith (1985, 480) observe:

> "...the solution to the problems of conceptual ambiguity in social psychological research lies in programmatic research efforts in which different experimental procedures are used to explore the same conceptual relationship. Essentially, there are two properties that we demand of a series of experiments before we are convinced that we understand what the conceptual interpretation should be.
>
> "First, we ask for a number of empirical techniques that differ in as many ways as possible, having in common only our basic conceptual variable. If all these techniques yield the same result, then we become more and more convinced that the underlying variable that all techniques have in common is, in fact, producing the results.
>
> "Second, we must show that a particular empirical realization of our independent variable produces a large number of different outcomes, all theoretically tied to the independent variable."

If one substitutes an alternative word indicating the variety of research strategiescommon to policy studies for the exclusive term "experimentation" in the preceding quotation, the sentiment there is entirely appropriate to our subfield of political science. Yet too few policy scholars with an interest in theory development are sufficiently disciplined to follow this programmatic path. And collective disciplinary practice, as I argued above, does not compensate for our individual failures in this respect.

More Deliberate Attention to the Requirements for Causal Inference.

Perhaps no topics are as controversial in the philosophy of science as the possibilities for and necessary conditions to support causal inference. In glaring contrast to that controversy, it is remarkable how quick many social scientists are to make causal inferences from even the most casually developed empirical evidence. And the casestudy literature is as prone to this tendency as is highly statistical scholarship. Indeed, a good deal of our research would be vastly improved if the words cause, influence, and impact were struck from its lexicon.

I suspect that all of us would also profit greatly by periodic re-exposure to an elementary review of the requirements for causal inference such as that offered by Babbie (1986, 49-66), an intensive review of such requirements as that in Marini and Singer (1988), and any portion of the extended debate about this topic in the philosophy of science literature. At a minimum, we'd be more conservative and skeptical about what causal inferences we would be willing to make. We might also be more imaginative about how to carry out research that would support such conclusions.

More particularly, we should remember that the character and quality of our research designs are far more important to our prospects for causal inference than is the method—be it a verbal or statistical one—by which we summarize and evaluate our empirical evidence. And greater attention to the ideas discussed by Babbie, Marini and Singer, and similar works could lead to substantial improvements in research design and thus the plausibility of our causal inferences.

The Relevance of Theory for our Educational Mission

Taking theory seriously means taking our science seriously. if we are to take on the latter obligation, we must do so in our classrooms as well as in our "laboratories." Yet I see scant evidence that we do so. And considerable scrutiny of undergraduate textbooks and course syllabi supports the latter conclusion. First, I know of no undergraduate public policy textbook that adopts a systematic approach to explaining the scientific goals of policy research, the most prominent scientific approaches to the study of the policy process, what we know today from such research, and what we do not know. At best, there exist a few partial efforts in texts that employ a single theoretical perspective to organize their discussions of the policy process—such as Ripley and Franklin's use of policy arenas theory in Congress, the Bureaucracy, and Public Policy (1987) or Anderson's use of the "stages" model of the policy process in Public Policymaking (1994).

The preceding works and a few others are only partial models of what a "science of the policy process" text might include, and they are remarkable exceptions, too, in a sea of textual materials with far different ambitions. The overwhelming majority of policy texts offers extended discussions of contemporary policy controversies and policymaking episodes with little or no attention to theoretical concerns. Similarly, none of the undergraduate policy studies course syllabi distributed by the American Political Science Association through various channels indicates an educational program organized around the scientific study of policy processes. Such courses may well exist, but they evidently are rarities. Instead, we appear to teach civics and current events.

If more of our teaching was centered explicitly on the scientific basis for and progress of our discipline, we would reap considerable benefits. I have tested those relatively uncharted instructional waters recently at both the graduate and undergraduate levels, and I've found the experience both humbling and quite educational for my own view of the discipline. I was literally forced to see the field in a different way than I had before. My students also claimed a benefit that I had not entirely anticipated. They said that they had only rarely encountered a course where the science of political science, warts and all, was so deliberately explained and discussed. And they admitted considerable prior confusion and skepticism about how the discipline merited the "science" in its name. Should we be surprised, then, that few of our students—or of the general public for that matter—can grasp why it is we are justifiably called a science? Should we be surprised, either, that we enjoy so modest a scientific reputation outside our own numbers?

For the sake of space I will not extend the preceding remarks at length as they apply to doctoral education, but their implications for that topic should be evident. In my view we should invest far more time than we do in ensuring that our doctoral students are well-educated scientists—with as much understanding of science generally as of politics particularly.

Is Advanced Theory Possible?

Obviously, many policy scholars are skeptical about our prospects for developing any notable degree of general theory, regardless of the assumptions of that theory. Such skepticism has been widespread in many fields of the social sciences in the last decade or so. At the same time a considerable volume of policy scholarship being published in all levels of our journals and scholarly books expresses a desire to advance general theory. I find a remarkable anomaly in this contrast, but I do not have space to explore it in this essay.

The specific question of how much theoretical progress is possible, however, is currently unanswerable in my opinion. We will only have an answer after we have worked hard at the task for a considerably longer time. Yet I think that we are not presently working hard enough—hard enough at sustained and systematic scientific research, that is—to achieve whatever progress might be possible.

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Book Reviews

Cultivating Congress: Constituents, Issues, and Interests in Agriculture Policymaking. By William P. Browne. Lawrence, KS: University Press of Kansas, 1995. 295 p. $29.95 (HC), $14.95 (P).

The agriculture policy domain is often singled out as an issue arena in which organized farm interests and farm-state legislators play an inordinately influential role in Congressional policymaking, enabling a relatively small proportion of the nation's citizens to benefit despite an ever-shrinking rural constituency. The "iron-triangle" metaphor has dominated explanations about agriculture politics, and reform efforts have experienced great difficulty in challenging farm-income protection programs.

William Browne has written a volume that challenges much of the conventional wisdom about this policy domain, in the process making us rethink our assessment of the impact of organized interests in the contemporary political climate in Washington as well as our views concerning how Congress makes policy generally. The picture he paints is one emphasizing the individualism in the post-reform Congress, the widespread, though ad hoc, concern with rural policy issues that extends far beyond farm-state representatives, and the much less influential role for organized farm interests and executive branch personnel in agricultural policy decisions than is commonly thought.

In large measure, Browne's work is a extension of his previously-published volume, *Private Interests, Public Policy, and American Agriculture*, which emphasized the strategic role played by interest groups in setting the agriculture policy issue agenda and providing selective information to Congress. The new volume examines agriculture policy making from the perspective of the congressional actor, focusing upon how legislators and their staffs decide what information to use in their decisions dealing with rural issues, where they get such information, how strategies to influence agriculture policy are determined, and the representational consequences of member behavior.

Data derived from in-depth interviews conducted from 1991 to 1993 with House or Senate members and/or their staff personnel in more than 100 congressional offices provides an impressive empirical basis for Browne's interpretations and conclusions. His sample includes not only members from the agriculture and agriculture subcommittees of both houses, but also legislators from the nine House and six Senate committees most likely to impact agriculture policies, as well as forty members of Congress who lacked any of the above specialized assignments. Fifty-four of the interviews were with members themselves, while 133 were with staff personnel with at least some responsibility in the agriculture and rural policy arenas. A number of other individuals were talked to as well, ranging from lobbyists, corporate officials and executive branch personnel to grassroots activists and consultants, to fill in background information. Much of the presentation of the findings is in easy-to-understand tabular form, enhanced by the skillful use of relevant, open-ended comments.

Browne clearly enjoys unusual access with policy makers, a fact that pays dividends reflected in the depth and thoughtfulness of some of the interviewees comments.

What Browne finds is that the agriculture policy making is driven more by individual member concerns than institutional ones, reflecting both a decline in the rules and norms that once constrained the behavior of members, as well uncertainty over who exercises authority, be it the leadership, caucuses, or committee/subcommittee referents. Additionally, the structure of agriculture domain is fluid and flexible, composed of increasingly more players, collectively holding contradictory positions, with many participants only becoming involved on an ad hoc basis. With little pressure from congressional authorities for comprehensive agriculture policy, nor policy agenda control by a well-defined network, there is a far greater capacity for individual members to act on their own strategic choices than previously.

In such an environment, legislators find it relatively easy and advantageous electorally to pursue issues for purely back home, district reasons. Indeed, Browne finds that large number of legislators, not just those representing large farm constituencies, at various times intrude in the agriculture domain on behalf of individuals in their constituency, usually with great success. The issues are typically very narrow and conflict is very low. Rather than challenging existing programs, new initiatives simply add to them. One of the reasons agriculture policy has resisted fundamental changes for so long is that so many members benefit from policy successes in the domain. For most legislators involvement in agriculture policy is not a zero-sum game. They can aid members of their constituency without threatening others.

One of Browne most intriguing findings concerns where congressional offices get their information and the attention paid to various organized interests in the agriculture policy arena. Members and their staffs have learned to distrust government agencies as well as interest groups, organizations they regularly find to be internally divided and having policy positions that do not reflect the interest of their own rural constituents. Browne finds that members typically come to pay inordinate attention to district confidants, individual constituents not connected to national interest groups, whom legislative offices have come to rely upon as both working farmers and barometers of district opinion. Ironically, the proliferation of organized interests and orchestrated group attempts to influence legislators on agriculture and rural issues may so overwhelm and confuse legislators that they largely discount them, preferring local sources of information.

Browne's volume is must reading for those interested in agriculture policy, and should be high on the list of scholars interested in congressional and interest group politics. It is appropriate for graduate courses, but undergraduates would have difficulty unless they are unusually well-grounded in both congressional politics and agriculture politics. Hopefully, the volume will encourage other researchers to study various policy domains in a similarly detailed and comprehensive manner.

Allan J. Cigler
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