

April, 1997

# POLICY CURRENTS

Institute for Public Policy  
The University of New Mexico  
and  
Policy Sciences Center  
The Florida State University

NEWSLETTER OF PUBLIC POLICY SECTION APSA

## In Search of Policy Theory

**Editor's Note:** This essay is part of a series intended to express provocative points of view about the current state and direction of policy scholarship. Future issues will contain replies to this essay, as well as new essays that we hope will stimulate discussion about other theoretical and substantive issues of import to policy scholars. We invite submission of (short) letters in reply to these essays. Send them via e-mail to [hjsmith@unm.edu](mailto:hjsmith@unm.edu).

by *Kim Quaile Hill, Texas A&M University*

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 appears timely. Finally, the essay proposes some pedagogical practices that comport with, and might enhance in several ways, the preceding recommendations for research.

The principal motivation for this essay is my criticism of my own scholarship—largely concerning democratic and representational processes as they af-

fect 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 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, that many policy researchers are not interested in theory. Instead, perhaps the majority of academic students of the policy process are interested in contemporary policy issues or the immediate activities of policy making institutions. Nor would I equate such interests with history or journalism as some might. Informed consideration 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 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 believe 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 assume 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, and Riker 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. Many of us are seduced 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. 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. 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; (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.

The vast majority of us who do empirical work of any stripe are 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, Legee 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.

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 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 most presentations of chaos theory seem forbiddingly impenetrable, their root conceptions of the world should appeal to those who see few regularities or equilibria and many exceptions to every generalization about the policy process. Yet chaos theory allows one to develop certain kinds of generalized knowledge about seemingly chaotic phenomena. Thus even if one chose not to adopt the mathematical armaments of such theory, its basic assumptions might inspire certain kinds of policy theory. Even more appealing might be the possibility that political phenomena exhibit some combination of deterministic and chaotic attributes, as suggested by Huckfeldt (1990).

Despite the fact that one can readily identify these alternative views 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. 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-range niche.

Deductive research typically does not suffer this theoretical timidity, and such work is a model of sorts for the rest of us—at least in the theoretical scope, in-

clusiveness, and logical rigor of the best of that work. Yet, as many critics have observed, deductive modeling in political science has to 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 with deductive theory stymied the scientific progress of the ancient Greeks is fascinating 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. 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, it 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, producing equal 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 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 only the slenderest of empirical bases for reaching positive or negative conclusions about the utility of this theoretical paradigm.

Equally notable in policy research 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 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, 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, 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 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 a particular 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 paid modest attention 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 implications of 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 multiple audiences, in particular, those interested in agricultural policy per se.

Johnson (1988) 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, it still might contain a rich lode of underexplored information useful for theory building.

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 of other 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 observed earlier, 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 strategies common 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 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 case study 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 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 sci-

ence, 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?

The implications for doctoral education 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.

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.

### References

- Aberbach, Joel D., Robert D. Putnam, and Bert A. Rockman. 1981. *Bureaucrats and Politicians in Western Democracies*. Cambridge: Harvard University Press.
- Almond, Gabriel and Stephen Genco. 1977. “Clouds, Clocks, and the Study of Politics.” *World Politics* 29: pp. 489-522.
- Anderson, James E. 1994. *Public Policymaking*. Boston: Houghton Mifflin.
- Aronson, Elliot, Marilyn Brewer, and J. Merrill Carlsmith. 1985. “Experimentation in Social Psychology.” In *Handbook of Social Psychology*, ed. Gardner Lindzey and Elliot Aronson. New York: Random House.
- Asimov, Isaac. 1984. *Asimov’s New Guide to Science*. New York: Basic Books.
- Babbie, Earl. 1986. *The Practice of Social Research*. Belmont, CA: Wadsworth.
- Bauer, Raymond A., Ithiel de Sola Pool, and Lewis A. Dexter. 1963. *American Business and Public Policy*. New York: Atherton.
- Baumgartner, Frank R. and Bryan D. Jones. 1993. *Agendas and Instability in American Politics*. University of Chicago Press.
- Beck, Nathaniel and Jonathan N. Katz. 1995. “What to Do (and Not to Do) with Time Series-Cross-Section Data in Comparative Politics.” *American Political Science Review* 89: 634-647.
- Blalock, Hubert M., Jr. 1969. *Theory Construction*. Englewood Cliffs, New Jersey: Prentice-Hall.
- Blalock, Hubert M., Jr. 1991. “Are There Really Any Constructive Alternatives to Causal Modeling?” In *Sociological Methodology*, 1991, ed. Peter V. Marsden. Washington, DC: American Sociological Association.
- Braithwaite, Richard Bevan. 1953. *Scientific Explanation*. New York: Harper & Brothers.
- Brown, Courtney. 1995. *Chaos and Catastrophe Theories*. Thousand Oaks, Ca.: Sage.
- Browne, William P. 1988. *Private Interests, Public Policy, and Agricultural Policy*. Lawrence, KS: University of Kansas Press.
- Carmines, Edward G. and James A. Stimson. 1989. *Issue Evolution*. Princeton: Princeton University Press.
- Freedman, David. 1991. “Statistics and Shoeleather,” In *Sociological Methodology*, 1991, ed. Peter V. Marsden. Washington, DC: American Sociological Association.
- Greene, William H. 1993. *Econometric Analysis*. New York: Macmillan, second edition.
- Halpern, Nina. 1993. “Stalinist Political Economy.” In *Ideas and Foreign Policy: Beliefs, Institutions, and Political Change*, eds. Judith Goldstein and Robert O. Keohane. Ithaca: Cornell University Press.
- Heinz, John P., Edward O. Laumann, Robert L. Nelson, and Robert H. Salisbury. 1993. *The Hollow Core*. Cambridge: Harvard University Press.
- Hill, Kim Quaile and Angela Hinton-Andersson. 1995. “Pathways of Representation: A Causal Analysis of Public Opinion-Public Policy Linkages.” *American Journal of Political Science* 39: 924-935.

- Hrebener, Ronald J. and Clive S. Thomas. 1993. *Interest Group Politics in the Midwestern States*. Ames, IA: University of Iowa Press.
- Huckfeldt, Robert. 1990. "Structure, Indeterminacy and Chaos: A Case for Sociological Law." *Journal of Theoretical Politics* 2: 413-433.
- Johnson, Cathy Marie. 1992. *The Dynamics of Conflict Between Bureaucrats and Legislators*. Armonk, NY: M. E. Sharpe.
- Kiel, L. Douglas. 1991. "Lessons from the Nonlinear Paradigm: Applications of the Theory of Dissipative Structures in the Social Sciences." *Social Science Quarterly*. 72: 431-442.
- King, Gary, Robert O. Keohane, and Sidney Verba. 1994. *Designing Social Inquiry*. Princeton: Princeton University Press.
- Lasswell, Harold D. 1951. "The Policy Orientation." In *The Policy Sciences*, ed. Daniel Lerner and Harold D. Lasswell. Stanford: Stanford University Press.
- Leege, David C. and Wayne L. Francis. 1984. *Political Research*. New York: Basic Books.
- Lowery, David and Virginia Gray. 1995. "The Population Ecology of Gucci Gulch, or the Natural Regulation of Interest Group Numbers in the American States." *American Journal of Political Science* 39: 1-30.
- Lowi, Theodore J. 1972. "Four Systems of Policy, Politics, and Choice." *Public Administration Review* 32: 298-310.
- McGregor, James P. 1993. "Procrustus and the Regression Model: On the Misuse of the Regression Model." *PS XXVI*: 801-804.
- Marini, Margaret Mooney and Burton Singer. 1988. "Causality in the Social Sciences." In *Sociological Methodology*, 1988, ed. Clifford C. Clogg. Washington, DC: American Sociological Association.
- Milner, Helen V. 1988. *Resisting Protectionism: Global Industries and the Politics of International Trade*. Princeton: Princeton University Press.
- Nagel, Ernest. 1961. *The Structure of Science*. New York: Harcourt, Brace & World.
- Popper, Karl R. 1959. *The Logic of Scientific Discovery*. New York: Basic Books.
- Riker, William H. 1980. "Implications from the Disequilibrium of Majority Rule for the Study of Institutions." *American Political Science Review* 74: 432-446.
- Ripley, Randall B. and Grace A. Franklin. 1987. *Congress, the Bureaucracy, and Public Policy*. Chicago: Dorsey.
- Sabatier, Paul A. 1991. "Toward Better Theories of the Policy Process." *PS XXIX*: 147-156.
- Wahl, John C., Heinz Eulau, William Buchanan, LeRoy C. Ferguson. 1962. *The Legislative System*. New York: John Wiley and Sons.
- Waterman, Richard W. and Kenneth J. Meier. 1995. "Principal-agent Models: A Theoretical Cul-de-Sac?" Paper presented at the annual meeting of the Midwest Political Science Association. Chicago, Illinois.
- Wolf, Fredric M. 1986. *Meta-Analysis: Quantitative Analysis Methods for Research Synthesis*. Beverly Hills: Sage.

# APSA Public Policy Panels

## Panel: 18-1

**Title:** POLICY NETWORKS, COMMUNITIES  
AND COALITIONS

**Chair:** Paul A. Sabatier  
Division of Environmental Studies  
University of California, Davis

**Papers:** *Hearts of the Body Politic Resource  
Exchange as Predictors of Power in  
Japanese Politics.*  
Jeffrey Broadbent  
University of Minnesota  
Department of Sociology

*Macro-Stability and Micro-Change: German  
Policy Networks Before and After Unification.*  
Thomas Koenig  
University of Mannheim  
Mannheim Center for European  
Social Research

*Policy Networks and Urban Governance:  
Economic Development, Culture, and  
Housing in Rennes.*  
Patrick LeGales  
University of Rennes  
Centre de Resherches Administratives  
Institut d'Etudes Politiques

*Interest Groups, Multi-Arena Politics, and  
Policy Change.*  
Jeremy J. Richardson  
University of Essex  
Dept. of Government Colchester

*Beliefs and Function as Determinants of  
Network Structure in San Francisco Bay  
Water Policy.*  
Paul Sabatier and Matt Zafonte  
University of California, Davis  
Division of Environmental Studies

**Discussant**  
Edella Schlager  
University of Arizona  
School of Public Administration

## Panel: 18-2

**Title:** POLITICS OF MORALITY POLICY IN THE  
U.S. STATES

**Chair:** Christopher Z. Mooney  
West Virginia University  
Department of Political Science

**Papers:** *The Rising Agenda of Physician Assisted  
Suicide: Explaining the Growth and Content  
of Morality Policy.*  
Henry R. Glick, Amy Hutchinson,  
and Scott Lamothe  
Department of Political Science  
Florida State University

*Morality Politics and Public Education:  
Conflicts Over Rights and Values.*  
Sandra Vergari  
The University of Northern Iowa

*Drugs, Sex, Rock and Roll: A Theory of  
Morality Policy.*  
Kenneth J. Meier  
University of Wisconsin-Milwaukee  
Department of Political Science

*Clean Thoughts and Dirty Minds: The  
Politics of Porn.*  
Kevin B. Smith  
University of Nebraska  
Department of Political Science

*Morality Politics and Presidential Power:  
Pornography, the Issue.*  
Byron W. Daynes  
Brigham Young University

**Discussant:** Clyde Wilcox  
Georgetown University  
Department of Government

**Panel: 18-3**

**Title:** ROUNDTABLE ON: "WILL SOCIAL SECURITY SURVIVE THE 21ST CENTURY? LOOMING CHALLENGES AND FUTURE PROSPECTS

**Chair:** Eric Patashnik  
Yale University  
Department of Political Science

**Presenters:**

Paul Light  
Pew Charitable Trusts

Larry Jacobs  
University of Minnesota  
Department of Political Science

Estelle James  
The World Bank

Theodore Marmor  
Yale University  
Yale School of Management

Marilyn Moon  
The Urban Institute

**Discussant:** Julian Zelizer  
University at Albany  
Department of History

**Panel: 18-4**

**Title:** ROUNDTABLE ON: "ECHOES OF CALIFORNIA'S PROPOSITION 209: WILL ENDING A MENDING AFFIRMATIVE ACTION BECOME A NATIONAL ISSUE?"

**Chair:** Frederick R. Lynch  
Claremont McKenna College  
Department of Government  
Bauer Center

**Presenters:**

Sam Fulwood, III  
Los Angeles Times

Harry Pachon  
The Claremont Colleges  
President/Professor  
Tomas Rivera Center

Bob Zelnick  
ABC News Legal Correspondent

Glynn Custred  
Professor of Anthropology  
Co-Author, Prop 209  
California State University, Hayward

Kevin Merida  
National Affairs Correspondent  
The Washington Post

**Discussant:** Christopher Edley  
Harvard Law School

**Panel: 18-5**

**Title:** ROUNDTABLE ON: INSTITUTIONS AND POLICIES TO STRENGTHEN DEMOCRACY

**Chair:** Helen Ingram  
University of California at Irvine  
Dept. of Urban and Regional Planning

**Presenters:**

Anne Schneider  
Arizona State University  
Dean, College of Public Programs

Jeffrey Henig  
George Washington University  
Center for Washington Area Studies

Carmen Sirianni  
Brandeis University  
Heller Graduate School

Steven Rathgeb Smith  
The University of Washington  
Graduate School of Public Affairs

Paul Light  
Pew Charitable Trusts

**Panel: 18-6**

**Title:** THEORETICAL ISSUES AND THE POLICY PROCESS

**Chair:** Peter deLeon  
University of Colorado  
Graduate School of Public Affairs

**Papers:** *APB For the Missing Link the Strange Case of Implementation in Public Policy Research.*

Peter deLeon  
University of Colorado  
Graduate School of Public Affairs

*Public Policy: The Search for a Paradigm.*  
Matthew Cahn  
California State University, Northridge

*From Policy Change to Institutional Change: Persistence, Change and Policy Frameworks.*  
Christopher Mazzeo  
Stanford University

*Political Power in the Commons: Institutional Heterogeneities and Resource Management Prospects.*

William Blomquist and Edella Schlager  
Indiana University

*Unexpected Arrivals (and Departures): Reconceptualizing Intermediary Policy Structures in a Globalizing Era.*

Anthony Perl with William Coleman  
University of Calgary

**Discussant:**

Toddi A. Steelman  
Duke University

**PANEL: 18-7A**

**Title:** WELFARE POLICY

*Note: Cross Reference with panel 17-6, Division 17 as primary sponsor*

**Chair:** Robert Albritton

**Papers:** *State Welfare Policy in the United States: 1960-1990.*

William D. Berry with Russell Hanson and Richard Fording  
Florida State University  
Department of Political Science

*Income Support for the Poor: Policy Making in the American States.*

Christopher Howard  
College of William and Mary  
Department of Government

*Welfare Magnets and the Race to the Bottom: Empirical and Political Realities.*

Scott W. Allard and Sheldon Danziger  
University of Michigan  
Poverty Research and Training Center

*The Effects of Party Competition, Party Control and Partisan Change on State Policymaking: Evidence from Welfare Spending in the American States.*

Charles Barrilleaux  
Florida State University

and Thomas Holbrook  
University of Wisconsin-Milwaukee  
Department of Political Science

and Laura Langer  
Florida State University  
Department of Political Science

**Discussants:**

Colleen Grogan  
Yale University  
Institution for Social and Policy Studies

and Gerald Wright  
Indiana University  
Department of Political Science

**Panel: 18-7b**

**Title:** POLICY IMPACTS AND INNOVATIONS IN THE STATES.

*Note: Cross Reference with panel 17-3, Division 17 as secondary sponsor.*

**Chair:** Kim Quaile Hill  
Texas A&M University  
Department of Political Science

*State Appropriations for Higher Education and Economic Development.*

Ruth Storm  
House Appropriations  
Florida Legislature

*Assessing the Impact of Solid Waste Management Initiatives in the American States.*

Christopher P. Borick  
St. Norbert College  
Political Science Department

*Small Business, Big Politics: The Story of Health Insurance Small Group Market Reforms.*

Christopher Stream  
Florida State University  
Askew School of Public Administration and Policy

*Decisions to Adopt and Reinvent Right-to-Die Policies in the American States: Three Event History Analyses.*

J. Donald Smith  
Rice University  
Department of Political Science

**Discussant:**

Charles Barrilleaux  
Florida State University  
Policy Science Center

**Panel: 18-8**

**Title:** PUBLIC OPINION AND POLICY FORMATION

**Chair:** James P. Lester  
Colorado State University  
Department of Political Science

**Papers:** *Public Assistance and Public Opinion: Is There a Culture of Dependency?*  
Saundra K. Schneider and William G. Jacoby  
University of South Carolina  
Dept. of Government & International Studies

*Regulation and Representation: Opinion-Policy Congruence and Regulatory Policy in America.*

Brad R. McKay  
University of Houston

*Immigration Policy Preferences and Anti-Immigrant Opinion.*  
Max Neiman and Kenneth Fernandez  
University of California, Riverside  
Center for Social and Behavioral Sciences Research

*Dimensions of Internationalism and Isolationism: An Analysis of the Structure of Policy Alternatives.*

Valerie A. Sulfaro  
James Madison University  
Department of Political Science

*In a Different Choice?: Gender Gaps in Public Policy Assessment.*

Mark Schlesinger  
Yale University

and Caroline E. Heldman  
Rutgers University

**Discussant:**

Lonna Atkeson  
University of New Mexico  
Department of Political Science

**Panel: 18-9**

**Title:** AGENDA SETTING AND PROBLEM DEFINITION

**Chair:** Frank R. Baumgartner  
Texas A&M University  
Political Science

**Paper:** *Structure and Performance in a Multiple Streams Model.*  
Nikolaos Zahariadis  
State University of New York

*The Politics of Agenda Denial.*  
Marc Howard Ross  
Bryn Mawr College  
Department of Political Science

and Roger Cobb  
Brown University  
Department of Political Science

*The New York Times, Health Care Reform and the Public Agenda.*  
Stella Z. Theodoulou  
California State University, Northridge  
Department of Political Science

*Policy Attributes, Congressional Committees, and Legislative Attention in Telecommunications Policy From 1945 to 1996.*  
Michael Rosenstiehl  
Texas A&M University  
Department of Political Science

**Discussant:**

James P. Lester  
Colorado State University  
Department of Political Science

**Panel: 18-10**

**Title:** PUBLIC POLICY AND BUREAUCRATIC CONTROL

**Chair:** Dave Hedge  
University of Florida  
Department of Political Science

**Papers:** *Beyond Authority: The Politics of Public Policy Implementation at the Department of Housing and Urban Development 1976-1989.*  
Kimi Lynn King  
University of North Texas

*Political Control of Bureaucracy and the Multiple Principal Problem.*

B. Dan Wood  
Texas A&M University

*Public Input: The Interaction of Public Opinion and Political Control Over Environmental Policy.*

Joseph Hinchliffe  
University of Illinois at Urbana-Champaign

*Mixed Institutions and the Governmental Process Groups: The Case of Fannie Mae and Freddie Mac.*

Jonathan GS Koppell  
UC-Berkeley  
Institute of Governmental Studies

**Discussant:**

George Krause  
University of South Carolina  
Department of Government and International Studies

**Panel: 18-11**

**Title:** POLICIES AS INSTITUTIONS AND INSTITUTIONS AS POLICIES

**Chair:** William Gormley  
Georgetown University  
Public Policy Institute

**Papers:** *The Rationality of Policy Institutions.*  
Dennis Coyle  
Catholic University of America

*Heuristics, Biases, and the Policy Process: Insights from Risk Regulation.*

Dr. Rajeev Gowda  
University of Oklahoma  
Science and Public Policy Program

*American Core Values and Policy Problem Definition.*

Sandra M. Anglund  
University of Connecticut

*The Institutionalization of Public Policy.*

Peter Yacobucci  
University of Arizona  
Department of Political Science

**Discussant:**

Royce Hanson,  
University of Texas, Dallas  
School of Social Sciences

**Panel: 18-12**

**Title:** AIDS AND PUBLIC POLICY

**Chair:** Deborah McFarlane  
University of New Mexico  
Department of Political Science

**Paper:** *The Politics of AIDS in Mexico: The Role of Civil Society.*

Robert McNamara  
Sonoma State University  
Department of Political Science

*Agendas, Policy Windows, and the Politics of Providing Services for Persons with HIV.*

Gregory S. Thielemann  
University of Texas, Dallas

*Debate and Deliberation of Multi-dimensional Health Care Issues: Evidence From Cancer and AIDS.*

Shalini C. Vallabhan  
Texas A&M University  
Department of Political Science

*The Politics of Health in the United States: Government and Social Movement Responses to Diseases.*

Abigail Riggs Spangler  
Columbia University

*Target Population in the Policy Process.*

Mark C. Donovan  
University of Washington  
Department of Political Science

**Discussant:**

James Slack  
California State University, Bakersfield  
Department of Public Policy and Administration

**Panel: 18-13**

**Title:** THE CROSS NATIONAL TRANSFER OF  
POLICES AND POLITICAL INSTITUTIONS

**Organizer:** David Dolowitz  
University of Birmingham  
Department of Political Science

**Chair:** Colin Hay  
Harvard University  
Center for European Studies

**Papers:** *The Mouse That Roared: Canadian-United  
States Interaction on Tobacco Regulation.*  
Donley T. Studlar  
West Virginia University  
Department of Political Science

*The Allure of the Modern and the Taint of  
Foreignness: Legitimizing Transferred  
Institutions.*  
Wade Jacoby  
Grinnell College  
Department of Political Science

*The Logic of Liberalism: Welfare Reform in  
Historical and Cross-National Perspective.*  
Robert H. Cox  
University of Oklahoma  
Department of Political Science

*The Failure of the British Child Support Agency:  
Its American Origins?*  
David P. Dolowitz and David Marsh  
University of Birmingham  
Department of Political Science  
and Intonation Studies

**Discussant:**

Guy Peters  
University of Pittsburgh  
Department of Political Science

**Panel: 18-14**

**Title:** THE ROLE OF ENVIRONMENTAL  
ETHICS IN RESTRUCTURING  
ENVIRONMENTAL POLICY AND  
LAW FOR THE NEXT CENTURY

**Chair:** John Martin Gillroy  
Bucknell University  
Environmental Studies Program

**Presenters:**

Steven Kelman  
Harvard University  
JFK School of Government

Joel J. Kassiola  
San Francisco State University  
College of Behavioral & Social Science

Mark Sagoff  
University of Maryland  
Institute for Philosophy and Public Policy

Joe Bowersox  
Willamette University  
Department of Politics

Bob Paehlke  
Trent University  
Department of Environmental Studies

Bob Peperman Taylor  
University of Vermont  
Department of Political Science

**Discussants:**

Gary Woller  
Brigham Young University  
Marriott School of Management  
Institute of Public Administration

Susan Buck  
University of North Carolina, at Greensboro  
Department of Political Science

and John Martin Gillroy  
Bucknell University

**Posters:**

*Welfare Reform: Some Evidence on its Assumptions.*

Gregoary R. Weiher and Roger Durand  
University of Houston-Clear Lake

*The United States Terrorism: The Domestic Aspects.*

Jorg Brechtefeld  
Christian-Albrechts-University  
Institute for Political Science

*Toward a Less Hostile Cyberspace: A Critical Analysis of the 'Federal Guidelines for Searching and Seizing Computers' and the Fourth Amendment.*

Charles L. Mudd Jr.  
Quinnipiac College

*The Growth of the American Health State in the 20th Century: The Legacy of Breakthrough Legislation.*

Grace Roegner Freedman  
Columbia University

*Do Grant Agencies Include Political Considerations in the Grant Award Calculus?*

David G. Frederickson and Bill Laverty  
Indiana University

*Aggregation Games: Understanding Political Bargains in Polycentric Systems.*

Margaret M. Polski  
Indiana University

*Mixing and Matching: A Formal Assessment of Combining Regulatory Policy Instruments*

Ken Meier and Michael J. Licari  
University of Wisconsin-Milwaukee  
Department of Political Science

*Campaign Finance in New York City: The Limits of Reform.*

Jeffery Kraus  
Wagner College

# Book Review

*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 an 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 policymakers, 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 as 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 fundamen-

tal 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  
University of Kansas

## ANNOUNCEMENTS

### **Request for Proposals for the Editorship of *POLICY CURRENTS***

This journal has recently been moved to an interim home at the University of New Mexico's Institute for Public Policy. In order to find the best possible editor, we seek proposals from those who would like to become editor of the *Policy Currents*. Of importance are the willingness and ability of the prospective editor(s) to undertake the considerable effort involved in soliciting, editing and producing manuscripts, book reviews, and other materials of interest to the members of the APSA Public Policy Section. Also of importance is the commitment of resources by the editor's home institution, including (but not limited to) office space, graduate student support, and released time for the editors. While the costs of printing and distribution of the journal are reimbursed by the Public Policy Section, the production and printing are the responsibility of the editor.

Those interested in seeking the editorship should submit a proposal to Editor Selection Committee Chair:

Professor Paul A. Sabatier  
Division of Environmental Studies  
University of California, Davis  
Davis, CA 95616  
Phone: (916) 752-3074  
Fax: (916) 752-3350  
E-mail: pasabatier@ucdavis.edu

Include in the proposal the names and vitae of the prospective editor(s); letters of commitment from the supporting institution; and a plan for the future evolution of the journal. Please submit three copies of the full proposal by July 1, 1997.

**Editors**

Charles Barrilleaux,  
Richard C. Feiock,  
Hank Jenkins-Smith

**Managing Editor**

Kristin Kenyon

**Senior Art/Publication  
Production Specialist**

Carol Brown

Abstracts, announcements, and other information to be printed in the June issue of *Policy Currents* should be sent on 3-1/4 diskette by May 10, 1997 to:

Editors, *Policy Currents*  
Institute for Public Policy  
University of New Mexico  
1805 Sigma Chi Road  
Albuquerque, NM 87131-1121  
Phone: (505)277-1099  
Fax: (505)277-3115

