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## The Status and Development of Policy Theory: A Reply to Hill

**Editor's Note:** This article continues a series intended to encourage the theoretical and empirical development of policy scholarship. The first salvo, by Kim Q. Hill, was published in the *Currents* in April, 1997 (Vol. 7, No. 1). As intended by Hill, his provocative essay stimulated a rejoinder by Edella Schlager, which appeared in the *Currents* in the June, 1997 issue (Vol. 7, No. 2), and the following article by Paul Sabatier. More will follow.

by Paul A. Sabatier, University of California, Davis

In the April 1997 issue of *Policy Currents*, Kim Quaile Hill lamented the current state of theory in policy studies and, more specifically, the status of positive theories of the policy process. He offered several explanations for the paucity of viable theories and suggested several ways to improve the situation.

I wish to thank Professor Hill for calling attention to the status of policy theory, since this has been a concern of mine for several years (Sabatier, 1991). While he has raised some interesting questions, several critical aspects of Professor Hill's essays are, unfortunately, quite flawed:

- 1) His assessment of the status of the field—as barren of any coherent theory or theoretical programs—ignores at least two viable theoretical programs and several promising approaches.
- 2) His understanding of what constitutes a theory is unclear, since he never provides either a definition or examples of viable theories.

- 3) His understanding of how theories develop is limited to inductive processes and thus of limited value, if not seriously misleading.
- 4) Given his flawed analysis of the status and causes of present theoretical development, his prescriptions for improving the situation are likewise misleading.

In the remainder of this paper, I first elaborate my conception of what constitutes theory and then briefly assess the state of theory in policy studies. The bulk of the paper presents some ideas—largely drawn from the personal experience of myself and, I think, Elinor Ostrom—about how theories develop and how that development can be fostered.

### I. Theories, Frameworks, and Models

I shall define a theory as a logically-related set of propositions that seek to explain a fairly general set of phenomena. The criteria by which a scientific theory should be judged are reasonably clear (Lave and March, 1975: 59-73; King et al., 1994: 99-113):

- 1) It should be logically coherent. The major terms should be clearly defined and the major relationships should be logically consistent. Without coherence, falsifiability is problematic, and the implications of a set of propositions are unclear.
- 2) It should have clear causal drivers and a sense of causal process. Scientific theories are causal theories that seek to explain how certain patterns of phenomena have come about. They should identify the critical causal drivers—what is

assumed to be fundamentally moving events within the system—and then the processes or mechanisms by which those drivers affect other variables. One of the fundamental shortcomings of many frameworks in policy studies—including Lowi's arenas of power and the stages heuristics of Jones (1970)—is that they fail to specify causal drivers and processes.

- 3) Some of the major propositions should be empirically falsifiable. This is what distinguishes science from other fields of human knowledge. To the extent those propositions are logically related to others, the validity of untested aspects of the theory can also be assessed.
- 4) The intended scope of the theory should be clear, although it can clearly change over time.
- 5) The theory should be "fertile." This means that it should (a) give rise to non-obvious implications, preferably beyond its intended scope, and (b) produce a relatively large number of interesting predictions per assumption.

Note that the third and fifth—and even the fourth—criteria are heavily dependent upon the first. To the extent that Propositions A, B, and C are logically related, invalidating Proposition A has serious implications for Propositions B and C. In addition, a logically coherent set of propositions is much more likely to give rise to non-obvious implications.<sup>1</sup> And, in the process, the scope of the theory is likely to be expanded or contracted. I stress logical coherence as a critical aspect of scientific theories because it is a point that Hill misses completely.

Elinor Ostrom has developed some very helpful distinctions between frameworks, theories, and models. A *conceptual framework* identifies a set of variables and relationships that should be examined in order to explain a set of phenomena. A framework can provide anything from a skeletal set of variables (or variable sets) to something as extensive as a paradigm. It need not specify direction to relationships or identify critical hypotheses, although it may do so. A *theory* provides a "denser" and more logically coherent set of relationships, including direction and hypotheses, that self-consciously seek to explain a set of phenomena. It applies values to some of the variables, and usually specifies how relationships may vary depending upon the values of critical variables. Numerous theories may be consistent with a general conceptual framework. A *model* is a representation of

a specific situation. It is usually much narrower in scope than the relevant conceptual framework and theory, and should contain quite specific assumptions about the values of critical variables and the nature of specific relationships. Ideally, it is mathematical. In my view, frameworks, theories, and models can be conceptualized as operating along a continuum of increasing logical interconnectedness and specificity of values and relationships, but decreasing scope.<sup>2</sup>

For example, the principal-agent literature in political science can be seen as a rather minimal conceptual framework identifying the relationships between principals and agents in institutional settings as its scope. There are also a number of models of the effects of specific interventions by principals on the behavior of specific sets of agents (e.g. Wood and Waterman, 1991, 1994; Jenkins-Smith et al., 1993). But, despite the early efforts of Moe (1984), there is nothing yet resembling a theory (at least in political science). Such a theory would have to identify the goal structure, information assumptions, and other resources available to both principals and agents, as well as identify the relevant institutional and other contextual variables, and provide hypotheses about what strategies by principals are likely to be effective, null, and counterproductive.<sup>3</sup>

Most theoretical constructions in policy studies would qualify as frameworks, and there are also numerous models. This essay seeks to identify a set of strategies for turning minimal frameworks into more extensive ones and eventually into theories.

## II. Present Status of Policy Theory

Hill complains several times about the "limited theoretical development" in positive theories of public policy. While he doesn't explicitly say so, one gets the impression that he views it as a barren waste land—save for a few theoretically-informed case studies, such as Browne (1988) and Johnson (1992). I agree completely with Schlager's (1997:14) contention in the June 1997 issue of *Policy Currents* that the field is not a wasteland but rather a field characterized by "mountain islands of theoretical structure, intermingled with, and occasionally attached together by foothills of shared methods and concepts, and empirical work, all of which is surrounded by oceans of descriptive work not attached to any mountain of theory."

The most impressive mountain is, of course, institutional rational choice or "actor-centered institutionalism" (Shepsle, 1989; Scharpf, 1997). The critical arguments are (a) that humans are intendedly rational, (b) their behavior is strongly influenced by institutional rules, and (c) they seek to influence institutional rules in order to alter others' behavior. Adherents in-

clude Chubb and Moe (1990) and Schneider et al., (1997) on school choice, Kagan (1978) and Scholz (1984) on regulatory compliance, Scharpf (1997) on European macroeconomic policy, Shepsle and Weingast (1986) and McCubbins and Sullivan (1987) on Congressional policy-making, and a host of scholars on administrative decision-making (Bendor et al., 1987; Knott and Miller, 1987). At a minimum, these authors share a conceptual framework. In most cases, they have been developing theories of behavior in different institutional settings and several models within each of those theories and then testing them over a number of years. In short, they have been doing precisely what Hill calls for in a variety of policy areas and institutions.

Even more amazing is that Hill fails to recognize a particularly impressive research program within the institutional rational choice tradition, that of Elinor Ostrom and her colleagues (see Schlager 1997:14-15 for a summary). Her Institutional Analysis and Development (IAD) framework is probably as close to a “covering theory” as we have in the social sciences. Her theory applying the IAD framework to the management of common pool resources (Ostrom, 1990; Ostrom et al., 1993; 1994; Crawford and Ostrom, 1995) is clearly one of the most important theoretical developments in political science over the last 10-15 years. It has attracted a half-million dollars in funding annually from NSF and other agencies, resulting in an impressive series of empirical tests in both field and laboratory settings. Her achievements are recognized by the discipline as a whole—she is, after all, the past President of the APSA—and will probably gain her election to the National Academy of Sciences. How Hill could fail to recognize this very elaborate, extensively-tested, theory leaves me completely befuddled about his search procedures and/or his conception of a theoretically-based research program.

I also agree with Schlager that the Advocacy Coalition Framework (ACF) developed by Hank Jenkins-Smith and myself—and critically applied in at least 32 settings by different scholars—is a viable and coherent research program. In fact, I would contend the ACF has evolved over the past decade from a moderately complex theoretical framework in 1988 to a much denser and logically coherent framework and/or theory in which several of the major holes are in the process of being filled (Sabatier and Jenkins-Smith, 1988; 1993; 1998).

There are also several fairly broad frameworks that cover much of the policy process and have been applied in a sophisticated fashion in a number of settings. These include (1) the punctuated equilibrium framework of Bryan Jones and his colleagues (Baumgartner and Jones, 1993; Jones, 1994; True et

al., 1998), (2) the policy diffusion framework and models of Berry and Berry (1990; 1992; 1998), (3) the multiple streams framework of Kingdon (1996; see also Zahariadis, 1996; 1998), (4) Ken Meier’s framework of regulatory policy-making (Meier, 1985; Meier et al., 1998), (5) the comparative state policy frameworks of Hofferbert (1974; Mazmanian and Sabatier, 1980) and Flora (1986),<sup>4</sup> and, (6) the principal agent framework of Moe (1984; see also Wood and Waterman, 1994).

The reason to get the terrain straight is that well-developed research programs—particularly that of Ostrom—should provide clues about how to move from relatively simple frameworks to much more developed frameworks and theories.

### III. The Origins and Development of Theories

The development (or elaboration) of a theory needs to be distinguished from its verification. The first deals with the generation of a set of logically-interrelated set of propositions—whatever the source—whereas the latter deals with the empirical testing of the validity of some of those propositions. This discussion is primarily concerned with the former, although one of the critical arguments is that the two should, of course, be linked.

#### A. Scenarios of Theory Development

Traditionally, scholars have distinguished two processes of theory development: inductive and deductive (see, for example, Reynolds, 1971). Both of these pure types strike me as being of limited value.

According to the inductive conception of theory development, theories arise out of the accumulation of “facts” from a variety of empirical studies, which are then synthesized into a set of coherent, more abstract propositions. This is clearly the process that Hill seems to be assuming with his continuous injunctions for more replication and extension of existing studies, more comparative analysis, more ambitious case-study scholarship, etc. The central problem with this conception is that it starts from a positivist view of perception that assumes we can observe “facts” unmediated by prior beliefs or presuppositions. This view has been subjected to some devastating critiques over the past 25 years (Kuhn, 1970; Brown, 1977; Hawkesworth, 1992). At any rate, I shall assume that, when seeking to understand any reasonably complex set of phenomena—and public policy processes are clearly complex—the observer must begin with a set of presuppositions concerning the entities worthy of notice, their characteristics that are worth remembering, and the types of relationships among entities that are worth observing. In other words, I assume that perception of complex phenomena is mediated by a set

of presuppositions constituting at least a simple conceptual framework. The problem with much of policy research is that these conceptual frameworks are often implicit rather than explicit, and thus not subjected to any serious scrutiny by the author or many readers.

In a deductive (or axiomatic) conception of theory development, the author begins with a set of fundamental axioms and definitions, and logically derives from them a more elaborate set of propositions, some of which are falsifiable. This conception is certainly consistent with a presuppositionist philosophy of science. And it may occasionally happen, with game theory being a possible example (von Neumann and Morgenstern, 1944). But the pure form of axiomatic theory development assumes that theories are developed in a vacuum, unconstrained by perceived regularities in portions of the phenomena of interest. That strikes me as unlikely, since anyone proposing a theory typically has 20-50 years of experience under his/her belt.

Instead, the more likely scenario is that a person becomes dissatisfied with an existing conceptual framework or body of theory, develops an alternative framework (or initial theory) to address those shortcomings, and then progressively elaborates that framework until it becomes a more fully developed theory over time. This is, I think, the case for both Elinor Ostrom and myself.

Ostrom began with a general appreciation of the theoretical elegance and potential explanatory power of micro-economic theories applied to political behavior (e.g. public choice theory), but she was profoundly disturbed by proponents' general neglect of the role of institutions (Ostrom, 1986). Public choice theorists tended to implicitly assume a set of institutional arrangements without recognizing that those arrangements were subject to manipulation and that the same individual would behave differently in different institutional settings. With respect to the more limited case of the management of common pool resources, she was disturbed with Hardin's (1968) analysis of "the tragedy of the commons." It implicitly assumed a given set of institutional rules—particularly, that local herders could not communicate with each other and *themselves* reach a set of agreements to regulate access to the common pool resource—and thus that intervention by external agents was necessary to regulate and enforce access restrictions (Ostrom, 1990). The basic IAD framework was first elaborated in Kiser and Ostrom (1982) and first applied shortly thereafter to the management of common pool resources in Bill Blomquist's dissertation research on groundwater basins in Southern California (see Ostrom, 1990: Preface).<sup>5</sup>

Likewise, the advocacy coalition framework grew directly out of my dissatisfaction with (a) the bifurca-

tion of implementation studies in the early 1980s into "top-down" and "bottom-up" perspectives, (b) most policy scholars' neglect of the role of technical information in the policy process, and (c) the overly simplistic model of the individual in most rational choice approaches to policy (Sabatier, 1986). The ACF was an effort to develop a new synthesis combining the best features of both implementation perspectives, together with Carol Weiss's (1977) insights on the long-term "enlightenment function of policy research" and a model of perception drawn largely from social psychology. It was strengthened when my intuitions concerning the factors affecting the role of scientific information were independently confirmed by the experience of Hank Jenkins-Smith (1988) as a policy analyst in Washington. The ACF was initially presented at a Rotterdam conference in 1983, revised for several years, and then published for the first time in the late 1980s (Sabatier, 1987; 1988).

Both the IAD and the ACF have, however, undergone considerable revision since their original publication. Both started out as moderately extensive conceptual frameworks (Kiser and Ostrom, 1982; Sabatier, 1988). The IAD has evolved into both (a) a more elaborate framework for understanding virtually all of social behavior (Ostrom, 1998) and (b) a much more elaborate theory of the management of common property resources (Ostrom et al., 1993; 1994; Ostrom, 1998). The ACF has become a much more integrated framework and/or theory for understanding long-term policy change in modern societies with a significant division of labor and some capability for organized dissent (Sabatier, 1998; Sabatier and Jenkins-Smith, 1998).

## **B. From Modest Frameworks to More Extensive Frameworks and/or Theories**

What are some of the reasons why the IAD and ACF have evolved from fairly modest frameworks in the early/mid 1980s to much more extensive frameworks and/or theories today? Conversely, why has Kingdon's multiple streams framework—initially published at about the same time (1984)—attracted much less empirical testing and undergone much less elaboration?<sup>6</sup> Several preliminary conclusions are suggested below.

1) *Be clear enough to be proven wrong.* This applies both to concepts and to proposed relationships (hypotheses). The basic argument is that we learn from our mistakes. Vague concepts and propositions are never proven wrong, and thus little learning occurs. Without learning, there is little incentive to correct inconsistencies, to revise falsified relationships, or to elaborate the framework to fill serious voids.

For example, an advocacy coalition has been defined as "people from a variety of positions (elected

and agency officials, interest group leaders, researchers) who share a particular belief system. . . and who show a non-trivial degree of coordinated activity over time” (Sabatier, 1988:139). In a 1995 article, Edella Schlager observed that all the empirical tests of the existence of coalitions by Jenkins-Smith and myself had focused simply on shared beliefs and thus had implicitly assumed that shared beliefs are a *sufficient* condition for coordinated behavior. Anyone remotely familiar with the literature on collective action (Olson, 1965) realizes this is patently false. As a result, my students and I have spent a lot of time seeking to define and operationalize different levels of coordination (Zafonte and Sabatier, 1997), as well as addressing Schlager’s suggestions concerning the conditions conducive to greater coordination. In short, a clear definition of “advocacy coalition” plus its clear—and invalid—operationalization have led to a major effort to fill the void of coordinated behavior within the framework.

Clear, explicit hypotheses attract serious scrutiny by other scholars. Several people who regularly use the ACF in their graduate courses have told me that one of the major reasons is that the ACF has always identified a number of explicit hypotheses and the authors of the ACF seem willing to revise those hypotheses on the basis of solid empirical research. Along the same lines, in a review of Ostrom et al., (1994), Jonathan Bendor (1995:189) remarked that he found the evidence from testing game-theoretic hypotheses in laboratory settings to be more persuasive than the field studies precisely because the former involved quantitative predictions that could be more easily falsified (or supported) by the evidence. Conversely, the multiple streams framework has no explicit hypotheses and is so fluid in its structure and operationalization that falsification is difficult.<sup>7</sup> Given the paucity of tests by other scholars, it is not surprising that Kingdon (1996: Postscript) has found no need to make revisions.

2) *Make the concepts of the framework/theory as abstract as possible.* The more abstract the concepts, the broader the scope of the framework or theory. Breadth is inherently desirable, *ceteris paribus*. More importantly, broader propositions are more likely to be falsified in some situations and confirmed in others. That, in turn, should lead to the identification of intervening variables and/or conditional relationships, i.e. to an elaboration of the theory. Ambition plus clarity lead to error which, in turn, produces revision and elaboration (Lave and March, 1975:42; King et al., 1994).

3) *Think causal process.*<sup>8</sup> What exactly are the mechanisms by which A affects B, which, in turn, affect C, etc. (Lave and March, 1975:40)? Thinking carefully about the steps in a causal process is one of

the principal steps in going from general frameworks to denser, more logically-interconnected theories. The failure to develop clear chains of causal relationships is probably one of the reasons that several policy frameworks that were popular in the 1970s—including Lowi’s arenas of power and the large-n comparative studies of Hofferbert, Dye, et al.—no longer attract much attention.

4) *Develop a coherent model of the individual.*

One of the major reasons that neither Lowi nor Hofferbert et al. ever developed clear chains of causal processes is that neither framework ever really developed a model of the individual actor. Such a model should include the goals or rules fundamentally driving actors’ behavior, their capacity to acquire and process information, their decision rules, and their politically-relevant resources. Since policy-making is fundamentally done by human beings, it is extraordinarily difficult to develop much of a sense of process if the linchpin of the entire process—the individual (or corporate) human actor—is a black box.

One of the fundamental tasks confronting several frameworks of the policy process—including Kingdon’s multiple streams framework and the diffusion framework of Berry and Berry—is to develop a much more explicit and coherent model (or models) of the individual. Conversely, some of the most interesting differences between the IAD theory of common property management, the ACF, and the punctuated equilibrium framework of Jones et al. concern differences in their models of the individual. For an excellent comparison, see Schlager (1998; also Schlager 1995; Schlager and Blomquist, 1996).

5) *Work on internal inconsistencies and interconnections.* This is another of the fundamental tasks in going from minimal frameworks to much denser, internally consistent frameworks and theories. It usually involves both empirical work that identifies inconsistencies and anomalies, and then logically thinking about how to resolve them.

In the advocacy coalition framework, for example, the delineation of policy core beliefs—as opposed to deep core and secondary aspects—is critical because (a) policy core beliefs are one of the essential means of defining a coalition, (b) they are critical to distinguishing major (policy core) from minor (secondary aspects) of policy change, and (c) most of the original ACF hypotheses hinged on the distinction between policy core and secondary aspects. Yet the original versions of the ACF were unclear whether the critical component of a policy core belief was degree of abstraction or scope. This ambiguity became critical when some of the research by Jenkins-Smith et al., (1993) on OCS leasing identified very concrete beliefs—material self-interest operationalized as ex-

panded leasing—to be the fundamental glue holding the Pro-Leasing Coalition together. This led us to select subsystem-wide scope dealing with a specific set of topics as being the defining characteristic of a policy-core belief.<sup>9</sup> That, in turn, led to query: “What are the defining characteristics of a subsystem?” Again, this question was precipitated when several empirical research projects sought to apply the ACF to “subsystems” that were narrower in scope than traditional ones: landsat within science and technology, eutrophication within water pollution, and automotive pollution control within air pollution control (Thomas, 1996; Loeber and Grin, 1998). These efforts led to a series of discussions resulting in a relatively clear set of necessary and sufficient conditions for the existence of a subsystem (Sabatier and Jenkins-Smith, 1998). That, in turn, led to empirical research which identified a set of partially-overlapping subsystems in San Francisco water policy and examined the interactions among them (Zafonte and Sabatier, 1997). The end result is that the relationships among policy core beliefs, subsystems, advocacy coalitions, and policy change are much more extensive and much clearer today than they were in either the 1988 or 1993 versions of the ACF.

6) *Develop a long-term research program involving both theoretical elaboration and empirical testing among a network of scholars.* This is probably the key. As we saw above, theoretical elaboration and empirical testing go hand in hand. Empirical studies identify inconsistencies, areas in need of elaboration, and propositions that are probably invalid (at least for a set of cases). These should stimulate revision and elaboration of the theory. All of this takes time—at least a decade. It helps enormously if a group of scholars working in a variety of field settings become involved in the empirical applications and contribute to the theoretical revisions. But it is probably also desirable if the original proponents of the framework continue to guide the overall research program so that the internal coherence of the theoretical framework is maintained over time.

Both Elinor Ostrom and I have been self-consciously pursuing such a strategy since the early 1980s (Ostrom et al., 1994; Sabatier and Jenkins-Smith, 1998). My perception is that John Kingdon has not. In both the IAD and ACF cases, the strategy has involved: (a) the initial publication of the framework; (b) empirical research by the authors to critically apply the framework in a variety of settings; (c) explicit encouragement to other scholars to do the same in settings where they are expert; (d) a clear willingness to revise the framework on the basis of empirical research and logical analysis; and (e) the fostering of a network of scholars involved in a shared research program.<sup>10</sup>

7) *Use multiple theories if possible.* This is pretty standard advice (Platt, 1964; Stinchcombe, 1968). It involves both being knowledgeable about multiple theories and, when possible, applying several theories in empirical research. The advantages are, first, that this strategy provides some guarantee against assuming that a particular theory is THE valid one. Instead, it leads to an appreciation that different theories may have comparative advantages in different settings. Second, knowing other theories should make one much more sensitive to some of the implicit assumptions in one’s favored theory. For example, much of the elaboration of the theory of the individual in the ACF has been the result of Edella Schlager’s explicit comparisons between the IAD and ACF.

#### IV. Conclusions

Kim Quaile Hill (1997) has initiated a useful dialogue concerning the status of theories of public policymaking. However, his view of the field is much too pessimistic. There are at least two viable bodies of theory out there—the IAD and ACF theoretical frameworks—and several more rudimentary frameworks whose further elaboration and development should be encouraged. These include punctuated equilibrium, policy innovation/diffusion, Ken Meier’s framework of regulatory policymaking, the multiple streams framework, Moe’s principal-agent framework, and Hofferbert’s “funnel of causality” framework.

I also disagree with Hill’s prescriptions about how to improve the status of theory in our field. He seems to assume that theory-building is almost entirely a process of accumulating and synthesizing findings from a variety of studies. In so doing, he completely misses the logical side of theory building, i.e. the development of a set of clear and logically consistent set of propositions. Both are necessary.

Finally, I would like to second Edella Schlager’s (1997:15) contention that argument and exhortation are not sufficient to improve the status of theory. We need to work on the institutional incentives affecting behavior. When it comes to research, there are at least two major types of incentives: funding and publication.

*Funding.* I’m not sure this is a problem. NSF usually requires that funded proposals pay some serious attention to theoretical development. My experience has been that this is also the case with some more applied programs, such as EPA’s exploratory grants program. To the extent this is not the case in other policy areas, attempts should be made to alter funding criteria. And I strongly applaud EPA’s recent decisions to run most of their grants programs through NSF, i.e. NSF is responsible for selecting the grantees and then EPA administers the funded grants.

*Publication.* With respect to books, there is one series—the one I edit for Westview Press on “Theoretical Lenses on Public Policy”—that is explicitly devoted to the improvement of theory. I would encourage the editors of other policy series (Ken Meier at M. E. Sharpe, Joe Stewart and Anne Schneider at SUNY, and Bert Rockman at Pittsburgh) to perhaps accord a little higher priority to the quality of theory in their publications. The problem is probably more serious with respect to journals. While most of the general policy journals—*JPP*, *JPAM*, *Policy Sciences*, and the PSO journals—sometimes publish theoretical articles, none of them require that articles be theoretically grounded. Given institutional inertia, I doubt that will change. Thus I think there is a strong argument for the Public Policy Section of the APSA to develop a new journal explicitly devoted to encouraging theoretically-relevant and methodologically-sophisticated scholarship on the policy process. This is an argument I have made before (Sabatier, 1991:153-155). Perhaps persistence will pay. If it does, Hill’s article—for all its problems—will have served a very useful purpose indeed.

## Notes

1. For example, scholars like Popkin (1979) and Becker (1976) have taken the basic principles of micro-economic theory and derived implications far beyond the original scope of the theory. Some of those implications have been empirically verified, while others have not. It was the logical coherence of the theory, however, that allowed creative minds to wonder: If it applies to market transactions in Western countries, why not other aspects of human behavior?

2. This conclusion grew out of an email exchange that I had with Lin Ostrom in the winter of 1996-97, but I’m not entirely sure that she would agree. For informative, and generally consistent, discussions of the distinctions among frameworks, theories, and models, see Ostrom (1998), Ostrom et al., (1994), Schlager (1998), and Scharpf (1997).

3. One of the most serious limitations of the principal-agent literature is its models of the agent. Much of this literature—e.g. Wood and Waterman (1994)—have essentially *no* model of the agent. The agent is simply a passive receptor responding to stimuli from principals. Another strain follows Niskanen’s overly simplistic view that agents are simply budget maximizers. Using Downs’ (1966) terminology, however, the same stimuli (incentive system) may have quite different effects on conservers (classical shirkers) than on policy advocates. See also Miller (1992) and

Worsham et al., (1997). At a minimum, any principal-agent analysis needs to start with clear and reasonably valid models of (a) the principal (s) and (b) the agents.

4. For a superb critique of this literature, see Blomquist (1998).

5. Interestingly, both Ostrom’s IAD framework and my ACF emerged out of the 1981-82 academic year we spent together at the Center for Interdisciplinary Research (ZIF), University of Bielefeld. The year at ZIF essentially marked the end of the initial phase of development of the IAD, following a decade of empirical research on urban service delivery systems. It marked the origins of the ACF, however, because of the exposure it gave me to “bottom up” implementation theorists like Ostrom, Benny Hjern, Ken Hanf and Fritz Scharpf.

6. While Kingdon (1984) is cited by many people, I am aware of only one scholar—Nikolaos Zahariadis (1992; 1996; 1998)—who has actually critically applied the multiple streams framework in a serious fashion. The framework itself has undergone only minor revision. Kingdon’s postscript in the 1996 edition contains no serious revisions. Zahariadis’ work (a) provides evidence that it can be extended outside the U.S. and to situations that are less “ambiguous” and (b) suggests a couple of related hypotheses: crises in the problem stream are conducive to searches for solutions specific to the problem while electoral mandates produce a search for doctrinal (general) solutions. In contrast, the ACF has been seriously applied by scholars other than Sabatier and Jenkins-Smith in at least 26 cases in ten countries and undergone at least a dozen significant revisions since 1988 (Sabatier, 1988; Sabatier and Jenkins-Smith, 1998). The IAD theory of common property management has also been seriously applied by several dozen scholars in numerous countries (see Schlager, 1997 for a partial list) and undergone quite substantial revision and elaboration (see Ostrom et al., 1994; Schlager, 1998). As for citations, in 1996 multiple streams received about 58 citations in the SSCI, the ACF about 40, and the IAD well over 100.

7. In my view, the fundamental problems with the multiple streams framework are that: (1) it’s unclear whether the dependent variable is the set of viable policy alternatives or the selection of an alternative; (2) the critical assumption of the independence of streams cannot be falsified because Kingdon never tells us how to identify which actors are in which streams; and (3) the causal drivers are underspecified,

in part because there are no clear models of the individual (except perhaps for legislators).

8. This is another place where I strongly disagree with Hill (1997), particularly his advice to be very cautious about attributing causation. He's correct in the context of *verification*. Establishing causation in a specific situation is, in fact, difficult. But, in *elaborating* a theory, one should always posit causal relationships and then worry later about how to verify them. Be bold rather than timid (to use Hill's terms).

9. Unfortunately, this definitional issue was still ambiguous in the concluding chapter of the 1993 book and was not really clarified until we began working on the ACF assessment paper (which eventually became Sabatier and Jenkins-Smith, 1998).

10. Specific techniques for fostering such a network include (a) explicitly encouraging other scholars to critically apply the framework; (b) reviewing dissertations and conference papers of young scholars interested in the framework; (c) providing incentives, e.g. grant funds or publication outlets, to stimulate such interest; and (d) establishing newsletters, conferences, and other mechanisms as communication outlets for scholars interested in the network. In virtually all of these areas, Elinor Ostrom is the master and I'm the apprentice.

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### A Perspective: Misunderstanding the ADA

by David Pfeiffer, University of Hawaii at Manoa;  
and Suffolk University, Boston

In the September, 1997 issue of *Policy Currents* (Vol. 7, No. 3) Eric Patashnik presents what he sees as "A New Look in Welfare State Research." It is an interesting article which describes recent approaches to social policy in the US. While I will leave to other policy analysts the task of commenting on the overall perception of changes in social policy research, I must point out a misunderstanding which Patashnik presents: that the Americans with Disabilities Act (ADA) is something other than a civil rights statute.

Patashnik (1997) cites and relies upon Burke (1997) for his argument. Professor Burke was kind enough to send me an early copy of his chapter some months before it was published. He is aware of my contentions and I will not address this note to him. My analysis of Professor Burke's work will appear elsewhere. At the same time it appears that Patashnik is relying upon Burke for the interpretation of the ADA and may or may not agree with him. Therefore, I will address this note to the misunderstanding of the ADA of which Burke (1997) is a clear example and with which Patashnik (1997) may agree.

Those persons who misunderstand the ADA usually make some or all of the following points. (1) Instead of focusing on needs and problems of people with disabilities, the ADA forces a rights orientation which prevents their needs from being met. The rights approach is "less effective and equitable than the professional treatment approaches to disability policy used in some other advanced democracies" (Patashnik, 1997, 5). (2) The ADA is an unfunded mandate forced upon the states (Burke, not Patashnik) and the private sector (Patashnik). (3) People with disabilities are deserving of public assistance. (4) The ADA promises to bring more and more people with disabilities into the work force, but it has failed to do so. (5) The ADA forces changes to be made such as accommodations and barrier removal. And (6) that an adversarial process is bad for social policy. This last contention usually accompanies the misunderstanding of the ADA, but strictly speaking it is not a misunderstanding of the ADA. It is a misunderstanding of our governmental system.

A reading of the statute itself (42 USC 12100) will show that none of these misunderstands is correct. However, let me discuss them individually.

(1) First, in regard to the rights orientation versus "professional treatment" which is directed toward the needs (mainly thought to be medical needs) of people with disabilities, allow me to point out that disability is

not a health question, but rather it is a political/policy question. I am a person with a disability using a wheelchair from a childhood bout with polio.

I am disabled, but I am not sick. Although as I enter my sixties I find it is not as easy to throw off a cold as it was 40 years ago, I am quite healthy. I usually see my physician once a year for a check up. When I do develop a medical problem, I never hesitate to consult my physician.

On the other hand, being a wheelchair user I encounter stairs, a lack of elevators, a lack of curb cuts, and inaccessible public transportation. Other people with different disabilities encounter other barriers. All of these barriers (and many more) are (or were in some cases) allowed by public policy. Building codes did not require wheelchair access and allowed expansive marble steps leading up to many buildings. Building codes did not require an elevator if the building had more than one floor. Curb cuts and lifts on busses were not required by local policy and by government agencies (like the US Department of Transportation) which funded them. It is especially rude and annoying if the building is a public one to which all taxpaying citizens have access, except for mobility limited persons.

I grew up in the South and daily witnessed places where African Americans were excluded. Segregation was the policy of the day. In the past women were routinely excluded from occupations because (it was said) they were not able to do them. For a while providing a women's restroom was considered too high of an expense in an all male office and was a reason not to hire a woman. In the same way public policies have kept people with disabilities out of public buildings, out of public accommodations, and off public transportation effectively segregating them and leading to high unemployment. The ADA simply says that discrimination (such as that cited) based upon a disability is a violation of a person's civil rights.

The other part of the rights orientation position - that people with disabilities have problems which need professional intervention - is so paternalistic as to be bigotry. To say that everyone has problems requiring professional intervention is too broad. But we all get by with a little help from our friends. When a non-disabled person's car needs work, he or she takes it to a mechanic. Or when the computer does not work, a consultant is called. When we have money to invest, we talk with financial advisors. The list can go on for quite a while showing that all people have problems which need professional intervention.

The problems usually faced by people with disabilities are the same as those faced by people without disabilities. The ADA requires that equal protection and due process (the ways in which civil rights are en-

forced) be provided to people with disabilities in the same way that they are provided to people without disabilities.

(2) The contention that the ADA is an unfunded mandate forced upon the states and the private sector rests largely upon ignorance. The requirements of nondiscrimination in the ADA are the same as those in the Rehabilitation Act of 1973 (29 USC 700) for recipients of federal funds including private employers with contracts over \$50,000. Twenty years ago there were funds available for barrier removal, but cities and towns who failed to use the funds and continued the discriminatory practices now find that the law will be enforced and they complain. In addition there are tax credits and deductions available to private sector employers and privately owned public accommodations which will pay for the removal of barriers. But the overarching argument against this misconception is that civil rights are not optional and the ADA is a civil rights statute.

(3) The argument that people with disabilities are deserving of public assistance is demeaning and devaluing. The ADA was not passed because we deserved some federal largesse. The ADA was passed and signed into law because we proved that people with disabilities suffered discrimination continually even when such discrimination was otherwise considered illegal. The ADA commits the United States government to ensure that the civil rights of people with disabilities are respected in the same way that the civil rights of persons of different races, genders, and nationalities are respected.

(4) There are many persons who say that the ADA promises to bring more and more people with disabilities into the work force, but it has failed to do so. There are two responses to that misunderstanding. First, it is the labor market which has the major impact upon employment. Since 1990 the US economy has not been robust. Second, ten years after passage of the Civil Rights Act of 1964 there was continued unemployment of African Americans in this country and no one blamed the statute. Instead, it was said that the continued unemployment of African Americans demonstrated that bigotry was still around. The continued unemployment of people with disabilities demonstrates the same thing.

(5) Probably the most extreme misunderstanding of the ADA is that it forces changes to be made such as accommodations and barrier removal. All the ADA does is to forbid discrimination based upon a disability. If a person files a complaint alleging discrimination, then the ADA gives the a rather large number of

defenses. If, for example, an employer offered a reasonable accommodation which was turned down, then the person claiming discrimination will not prevail. If an employer, a public entity, or a public accommodation is sued and it is shown that the requested change is an undue burden or not feasible, then the person claiming discrimination will not win.

Once the policy of non-discrimination is stated, the ADA primarily provides defenses against a charge of discrimination. If you do not believe me, go read the statute. The regulations dealing with implementation of the ADA give examples of barrier removal and reasonable accommodations and probably are the source of much misunderstanding of the ADA. If the regulations suggest that policies and buildings be reviewed in order to find discriminatory policies and barriers, it is to assist companies and governmental entities and help them avoid a discrimination complaint. But the regulations are not the statute.

(6) Finally, the charge that "using litigant activism as a substitute for bureaucratic provision" (Patashnik, 1997, 5) is bad comes from a misunderstanding of the system of government which we have in the US. We do not have a monarchy, an aristocracy, or a theocracy to resolve disputes. We have a judicial system which attempts to be impartial. Of course mistakes are made, money buys better litigators, and judges have their own value systems which influence decisions, but that is the price we must bear unless we surrender our rights to some professional who will dictate what we should or should not do with our lives. Professionals can be of great assistance, but it must be up to us to accept or reject their recommendations regardless of our race, gender, nationality, or disability.

All of these arguments can be greatly expanded with numerous references. However, the passages in Patashnik (1997) consisted of only two paragraphs in five pages of text. The more expansive article in response to Burke (1997) awaits completion and publication. In the meantime the ADA must be viewed as what it is: a civil rights statute.

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## Selected Abstracts:

**Keeok Park. "Friends and Competitors: Policy Interactions between Local Governments in Metropolitan Areas." *Political Research Quarterly*, (December 1997).**

This article explores intergovernmental policy interactions between central city, county, and suburban city governments in 186 metropolitan areas. Two hypotheses are developed to test the existence and shape of intergovernmental policy interactions between those governments. A competition hypothesis, based on Tiebout's 1956 concept of interjurisdictional competition, posits that counties and cities compete in the developmental policy area by matching what other cities and counties do. A cooperation hypothesis, based on the notion of functional arrangements between local governments, posits that counties and cities cooperate in the public safety and education policy areas by supplementing others' policies or by substituting their policies with others' policies. The results of an analysis of local government expenditures indicate that local governments compete as well as cooperate at the intergovernmental level and that intergovernmental policy competition is weaker than interjurisdictional policy competition. The article ends with a series of discussions on possible changes in the current local government system to mitigate the competition between cities and their counties.

**Isaac Unah. "Specialized Courts of Appeals' Review of Bureaucratic Actions and the Politics of Protectionism." *Political Research Quarterly*, (December 1997).**

Specialized courts are historically thought to serve only policy-neutral objectives. In the research presented here, I reexamine this widespread belief. The article analyzes the interaction between the Court of Appeals for the Federal Circuit, a specialized circuit court, and bureaucratic agencies to attempt to determine the Court's impact on U.S. Trade policy outcomes. Using dumping and foreign government subsidization cases brought to court in the 1980s, I develop and test an attribute model of judicial protectionism. Encapsulating economic, political, and contextual legal circumstances surrounding international trade cases. The most notable findings are that the Federal Circuit shows a significant proclivity toward protecting U.S. industries against unfair trade practices of foreign competitors, suggesting that, contrary to previous thinking, specialized courts have far more than policy-neutral effects. Also, the International Trade Commission performs remarkably better in court than does the Department of Commerce due to the Commission's

court-like decisional strategy and independent political status. Finally, my model offers strong support for both political and case-related explanations of regulatory activity, showing, e.g., that industry political power and organizational ability condition trade judges' decisions to dispense regulatory protection.

**Lee Sigelman, Susan Welch, Timothy Bledsoe and Michael Combs. "Police Brutality and Public Perceptions of Racial Discrimination: A Tale of Two Beatings." *Political Research Quarterly*, (December 1997).**

This note uses data from a national and local opinion survey that were underway when highly publicized police beatings of African American citizens occurred in two American cities—the beating of Rodney King in Los Angeles and the killing of Malice Green in Detroit—to probe the impact of these dramatic events on public perceptions of racial discrimination. The incidents appear to have had their greatest effect on specific perceptions of the way local police treat blacks, and markedly less effect on broader perceptions of the extent of discrimination against them.

**Nayda Terkildsen and Frauke Schnell. "How Media Frames Move Public Opinion: An Analysis of the Women's Movement." *Political Research Quarterly*, (December 1997).**

We examine the weekly print media's coverage of the women's movement and ascertain the presence of five unique frames from the 1950's through the 1990s; a sex roles frame, a feminism frame, political rights frame, economic rights frame, and an antifeminism frame. After describing the frames we discuss an experimental test of four of the media packages on voters' political attitudes using a nonrandom sample of adults. Experimental results indicate that the economic rights and antifeminism frames had a strong, negative impact on subject attitudes toward gender equality, support for women's rights, support of non-traditional gender roles and the frequency with which subjects mentioned "women's issues" as among the most important issues facing the U.S. In addition, the feminism frame also exerted negative effects while, conversely, the political rights frame had a positive influence on similar gender attitudes. Results were moderated by respondent gender with men demonstrating greater susceptibility to issue framing than women.

**John A. Clark and Jerome S. Legge, Jr.** "Economics, Racism, and Attitudes toward Immigration in the New Germany." *Political Research Quarterly*, (December 1997).

A number of different theories have been advanced to explain support for policies which may benefit minority groups. This paper utilizes economic-self interest, traditional racism, and conservative values to explore attitudes of German citizens toward granting political asylum and residency to immigrants. The linear structural relations (LISREL) analysis on a 1991 sample of German citizens suggests that the structure of attitudes is different depending upon which section of Germany (the West or the former German Democratic Republic [GDR]) is considered. East Germans tend to "fuse" traditional racism with conservative values to express a racism by argued by "symbolic" theorists while the West Germans tend to separate the concepts, possibly because of the more mature democratic development of West German parties and institutions. In explaining immigration policy, traditional racism tends to dominate the West German model, while economic self-interest partially explains policy attitudes in the former GDR. Still, even in the East, ethnocentrism is a very strong factor in understanding support for restrictive immigration policies. We discuss the implications of analysis in terms of the literature on American racism and policy reasoning.

**Alvin Richman and David B. Nolle.** "Testing Foreign Policy Belief Structures of the American Public in the Post-Cold War Period." *Political Research Quarterly*, (December 1997).

The basic attitudinal structure underlying the American public's foreign policy preferences is assessed by using both exploratory and confirmatory factor analysis on two major post-cold war surveys containing many similarly worded questions-by the Chicago Council of Foreign Relations (CC, 10/94) and the Times Mirror Center (TM, 9/93). Although previous studies had stressed only two or three primary attitudinal factors and usually ignored factors interrelations, our exploratory analyses of these data sets consistently yielded at least four distinct and readily interpretable factors, including two correlated "outwork-focused" factors (Global Altruism and U.S. Global Interest), a U.S. Domestic issues factor bearing on foreign policy (e.g., jobs protection), and a Military Security factor model provided probabilistically close fits to both the CC and TM data sets and that accuracy of fits declined with various simpler models.

## Notices:

### Public Policy Syllabus Collection Call for Materials

by *Thomas Birkland*

I am preparing a collection of Public Policy syllabi for the APSA Syllabi Collection. We expect the collection to be available as a bound volume in mid 1998. We solicit your contributions to the collection. We seek syllabi and other materials from graduate and undergraduate public policy courses, from courses on substantive policy areas, and on course units on substantive policy areas, such (but by no means limited to) as environment or health policy.

Feel free to send me any materials you would like to contribute as attachments to e-mail (documents in HTML or Microsoft Word for Windows version 6.0 or lower), via fax at 518-442-5298, or via the mail to:

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Submissions will be reviewed by an advisory committee of senior scholars in public policy. If we select your materials for inclusion in the collection, we will contact you and may ask for additional camera-ready copies of materials, particularly if they were originally faxed to us.

I thank you for your interest and your contributions to what I hope will be an exciting and useful project.

## Skewered

by *Joseph Stewart, Jr.*

In the September, 1992 *PS*, Ken Meier and I published what is probably our most widely read work—"Rotisserie Political Science." (Other works are more widely cited, but just because they are cited does not mean they have been read.) In that work we attempted to alter behavior within the discipline, e.g. penalizing an individual who takes over 15 minutes to present his/her convention paper, and, at the same time, inject some intentional humor into the profession. (There is already sufficient unintentional humor.)

To extend those efforts the editor has drafted me to edit a column titled "Skewered," (on a Rotisserie, get it?) to appear in *Policy Currents*. (Could we reconsider the vote on the editorship?) Send suggested additions to the Rotisserie game to me at joestew@unm.edu. Either individual items or whole sections of the game are welcome. They should be in the format used at the top of this column—a suggested point value followed by the item/activity/inactivity. Also note how you would like the material to be credited. You might want to take credit for your contribution or you might want to blame someone else, say Paul Sabatier. I have always found that by opening a sentence with "If Ken Meier were here he would say ..." gives me infinite license to say whatever I wish.

Remember, however, libel laws have not been suspended, and the section's treasury does not allow for a legal defense fund.

Submit! I shall review it, so if your contribution makes it into print, that makes it refereed. You too could finally have something widely read.

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