
The Stages Approach to the Policy Process

What Has It Done? Where Is It Going?

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"I'm sorry Peter, but it seems that [policy research] has moved beyond the [policy process] stages heuristic."

—Participant at 1996 APSA meetings

More than forty-five years ago, Harold D. Lasswell articulated the first formal usage of the concept *policy sciences*. Although informal policy advice had been offered by advisers to rulers for centuries, Lasswell was the first to define in any coherent manner what composed this "new" approach to government and its characteristics (Lasswell, 1951; also Lasswell and Kaplan, 1950). Since then, the policy sciences—largely under the derivative rubrics of policy analysis and later public management—have made tremendous strides in terms of widespread acceptance, surely in the United States and increasingly in other nations. But as the policy sciences orientation approaches half a century, one can legitimately wonder what it has produced in terms of Lasswell's original vision, its everyday operation, and, most important, its capacity for future research, in short, its overall success. More pessimistic observers would agree with Donald Schön and Martin

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Rein (1994, p. xvi), who—although themselves sympathetic to the policy sciences—wrote that “the policy analytic movement begun by Harold Lasswell in the early 1950s has largely failed.”

In this essay, I deal with one particular aspect of Lasswell’s vision of the policy sciences. Lasswell operationalized—although rather abstractly—many of his ideas about improving the quality of governance by improving the quality of the information being rendered to government. He focused particular attention on the “policy process,” or the functional stages or phases that a given government policy (or program) would go through during its “policy life.” As we shall see, many observers have argued against the Lasswellian approach and have strongly suggested the shortcomings of the policy process/stages approach. In this context, we can examine Lasswell’s (and others’) policy framework to see if it has become as antiquated (some would claim dysfunctional) as its critics have charged. Alternatively, we can see if it still offers some utility as the art and craft of policy research continue to evolve as a tool to improve the quality of the information offered government.

KNOWLEDGE IN THE POLICY PROCESS

Lasswell gave special emphasis to what he termed “knowledge *of* the policy process” and “knowledge *in* the policy process,” the former being more substantive (e.g., How much CO₂ can be released into the atmosphere without evoking a disastrous global warming condition?) and the latter being more procedural (How does a democratic polity publicly intervene in reducing its CO₂ emissions?). He framed a “conceptual map [that] must provide a guide to obtaining a generalistic image of the major phases of any collective act” (Lasswell, 1971, p. 28) and nominated seven “stages” of what he was later to call “the decision process” (Lasswell, 1956):

- Intelligence
- Promotion
- Prescription
- Invocation
- Application
- Termination
- Appraisal

This listing reflects the origin of what has arguably been the most widely accepted concept of the policy sciences, that is, the policy process, the procedure by which a given policy is proposed, examined, carried out, and perhaps terminated (see Lasswell, 1956). Later, one of Lasswell’s students at Yale University, Garry D. Brewer (1974), proposed a derivative list (almost certainly with Lasswell’s specific

approval) that (with other very similar alternatives from other authors) has shaped much of the research agenda undertaken by policy scientists since the mid-1970s, in both substantive and practical terms:¹

- Initiation
- Estimation
- Selection
- Implementation
- Evaluation
- Termination

These stages are not simply divined from the heady atmosphere of the academy. Both individually and in combination, they offer a way to think about public policy in concept and, just as important, in operation. Although they certainly can merge with one another, each does have a distinctive characteristic and mannerism and process that give the individual stage a life and presence of its own. Without denying that the stages can (and often should) share information and procedures, few observers would confuse the distinguishing set of activities that defines program estimation with those dealing with (say) policy termination. Angela Browne and Aaron Wildavsky (1984, p. 205) made the point with great cogency as they distinguished between the mutually supportive duality of implementation and evaluation:

The conceptual distinction between evaluation and implementation is important to maintain, however much the two overlap in practice, because they protect against the absorption of analysis into action to the detriment of both.

The idea of a delineated, sequential policy process framework apparently was much admired, for, as stated above, numerous authors have availed themselves of the framework, either explicitly or implicitly. Charles Jones’s *An Introduction to the Study of Public Policy* (1970/1977/1984) and James Anderson’s *Public Policy Making* (1975/1979) were among the first “policy process” volumes; Anderson references both Lasswell and Jones in his description of the policy stages (although omitting termination). In 1983, Brewer and deLeon published their volume, which completely laid out the stages of and rationales for the policy process. All three volumes (and other analogous models, such as Judith May and Aaron Wildavsky, 1978, and Dennis Palumbo, 1988) focused the reader’s attentions on “knowledge *of*,” that is, the workings of the policy process as a process-oriented event.

Just as important, these volumes and their advocacy (or at least their utilization) of the policy process model directed an entire generation of research by noted policy scholars, as they studied stages as stages (e.g., policy initiation) rather than as specific issue areas (e.g., energy resources).² These works include such unquestionable policy classics as:³

- Initiation: Nelson Polsby's *Political Innovation in America* (1984), John Kingdon's *Agendas, Alternatives, and Public Policy* (1984/1996), and Barbara Nelson's *Making an Issue of Child Abuse* (1984).
- Estimation: Alice Rivlin's *Systematic Thinking for Social Action* (1971), Edward Quade's *Analysis for Public Decisions* (1983), and David Weimer and Aidan Vining's *Policy Analysis* (1989).
- Implementation: Jeffrey Pressman and Aaron Wildavsky's *Implementation . . .* (1973), Eugene Bardach's *The Implementation Game* (1977), and Daniel Mazmanian and Paul Sabatier's *Implementation and Public Policy* (1983).
- Evaluation: Edward Suchman's *Evaluation Research* (1967) and Richard Titmuss's *The Gift Relationship* (1971).
- Termination: Herbert Kaufman's *Are Government Organizations Immortal?* (1976) and Fred Ikle's *Every War Must End* (1971/1991).

In his *Advice and Consent* (1988), deLeon compared the relative strengths and weaknesses of the segmentation of the policy stages/process framework as it affects the policy sciences research agenda. On the one hand, these works brought a new richness to the policy sciences, as Polsby and other policy scholars emphasized the intense complexity that theorists in political science and economics, in search of more rigorous, hypotheses-generating-models, might have overlooked. For instance, Pressman and Wildavsky's detailing of the high drama performed by the Economic Development Administration (EDA) and its incredibly cumbersome ballet with the city of Oakland, partially initiated to ward off potential urban violence (that surely was not part of the EDA's initial mission), demonstrated just how involved and actually convoluted policy implementation could be. Similarly, Titmuss's normatively oriented evaluation of comparative blood transfusion policies in *The Gift Relationship* forcefully argues against a reliance on standard benefit-cost analyses that were the growing standard of program evaluation.

Moreover, an emphasis on the policy process moved research away from a strict adherence to the study of public administration and institutions, which was increasing in political science, and of quasi-markets, which was the predilection of economics. Thus, it helped to rationalize a new problem-oriented perspective markedly different from its disciplinary predecessors. The cumulative analyses of the various stages clearly demonstrated Lasswell's insistence on a multidisciplinary approach to the policy sciences, as well as the interactive effects among the different stages. Finally, the policy process framework readily permitted the explicit inclusion of social norms and personal values, a component too often neglected or ignored in contemporary political and economic examinations.

But at the same time, these analyses of specific stages in the policy process model had a clear downside in that they oriented scholars toward looking at just

one stage at a time (deLeon, 1988), thereby neglecting the entire process. Ultimately, many policy researchers (and policymakers⁴) came to view the process as a sharply differentiated set of activities: First, you define the problem; then, a completely different set of actors implements the chosen policy option; a third stage defines the evaluation; and so on. Likewise, they portrayed a disjointed, episodic process rather than a more ongoing, continuous one, as well as a policy phenomenon that seemingly took place in the relatively short term, one more suitable to the policymaker's rapidly changing schedule than the life span of a given policy. Finally, to many, the policy process/stages image implied a certain linearity—for example, first initiation, then estimation. . . . then (possibly) termination—as opposed to a series of feedback actions or recursive loops (e.g., estimation can lead back to initiation rather than the next step, selection, and implementation and evaluation insistently feed back and forth on each other) that characterize the operations and politics of the policy process.

Nevertheless, most (even subsequent critics) agree that the framework of the policy process and its various stages held center stage for at least the better part of the 1970s and 1980s. It was, for many, the “conventional wisdom” (Robert Nakamura, 1987, referred to it as “the textbook policy process”) that forced itself upon an emerging discipline, largely in disregard of Albert Hirschman's (1970) prescient warning that paradigms, unless closely considered, can become a hindrance to understanding. And arguably, that is exactly what happened as policy scholars began to inform their own interpretations of the policy process framework as if it were the target rather than the condition it sought to describe. Although certainly none would argue against a new statement of perspectives, one can openly question its basic assumptions. Let us therefore examine the thrust of these criticisms.

IN SEARCH OF A THEORY . . .

It was not until the late 1980s that Robert Nakamura (1987) began to question the conventional wisdom, asking if its “widespread use” suggested that the stages were anywhere near as precisely defined as their proponents proposed; if not, he claimed, the process/stages image could not be used as a “paradigm.” Later, Paul Sabatier (see Sabatier, 1988, 1991), often in cooperation with Hank Jenkins-Smith (1993), proposed that the policy process “heuristic” (their term) has “serious limitations as a basis for research and teaching” (Jenkins-Smith and Sabatier, 1993, p. 3), and, more specifically, that the policy process neglects “the role of ideas—particularly ideas involving the relatively technical aspects of policy debates—in policy evolution” (Sabatier, 1993, p. 15).

Sabatier offered six very concrete complaints about the policy process as a unifying concept within the policy sciences (Jenkins-Smith and Sabatier, 1993, pp. 3–4; emphases in original):

1. "The stages model is not really a *causal model at all*." That is, it did not lend itself to prediction, or even to indicating how one stage led to another.
2. "The stages model *does not provide a clear basis for empirical hypothesis testing*." Hence it is not amenable to confirmation, amendment, or fabrication.
3. "The stages heuristic suffers from *descriptive inaccuracy* in posing a series of stages. . . ."
4. "The stages metaphor suffers from a built-in *legalistic, top-down focus*."
5. "The stages metaphor inappropriately *emphasizes the policy cycle as the temporal unit of analysis*." In other words, it neglects the concept of a system of intergovernmental relations.
6. "The stages metaphor fails to provide a good vehicle for integrating the roles of policy analysis and *policy-oriented learning throughout the public policy process*."

Sabatier's criticisms were well couched and thoughtful, even though not always to the point of those who used the policy process/stages metaphor.⁵ The primary shortcoming, according to Ronald Brunner (1991), is that Sabatier's criticisms reflected a worrisomely narrow use of *empirical* (e.g., a use conducive to specific hypothesis creation and prediction) theory; it overlooks the presence of what Lasswell called a *central* theory, which helps integrate (N.B.: not necessarily predict) policy events. As Brunner (1991, p. 70) posited, "An adequate body of central theory—composed of concepts as well as normative and empirical propositions—has been available for some time." Later, Brunner (1991, pp. 80–81) was even more explicit: "The purpose of the policy sciences as 'science' is to realize more of the potential for free choice through the sharing of insight [i.e., central theory]. The purpose is *not* prediction" (emphasis in original).

This narrowness is also present in more functional uses (to which Lasswell gave equal footing) of the policy process paradigm. For example, Brewer and deLeon (and, by implication, Lasswell) never proposed that the policy process comprised a theoretic *model* as ascribed by Sabatier, for they certainly realized that it was not suitable to formal hypothesis testing or prediction with much precision. Rather, they viewed the policy process as a device (a heuristic, as it were) to help disaggregate an otherwise seamless web of public policy transactions, as was too regularly depicted in political science. They proposed that each segment and transition were distinguished by differentiated actions and purposes. For instance, policy estimation was primarily an analytic activity pursued by (usually) staff analysts within an agency; on the other hand, implementation was performed by an entirely different set of actors, generally acting outside the agency, having to interact with a defined set of external clients, and occasionally having to alter literally the policy purposes as a matter of local necessity (Groggin et al., 1990).

Still, the overall policy process metaphor implied a system. In Brewer and deLeon's (1983) simile, the policy process "model" was likened to that of a medical doctor; a physician might well examine a patient's blood circulation or hormonal balance but would never lose track of the fact that the body's circulation or biochemistry is contained within and vital to a system (i.e., the body). Nor did Brewer and deLeon ever claim that the stages are unidirectional or lacked feedback capabilities; indeed, quite the opposite. To claim that the policy process heuristic lacks *empirical* theoretic constructs and characteristics and is therefore empty, or even "dysfunctional," is somewhat akin to claiming that Tom Cruise's reputed lack of serious acting ability disqualifies him as a matinee idol and box office cash cow.

However, these reservations or rejoinders are not meant to diminish the importance of Sabatier and Jenkins-Smith's research agenda. Indeed, at its base, it is nowhere near as dismissive of the policy process/stages heuristic as the authors would have us believe. Rather, one could justifiably argue that in the articulation of their advocacy coalition framework (Sabatier and Jenkins-Smith, 1991), they were talking implicitly about a perceived lacuna in the policy process, in this case, policy initiation (or what others, such as David Dery, 1984, have termed "problem definition"). Their very title—*Policy Change and Learning*—speaks directly to their goals, that is, to explain how new (or seriously revised) programs are brought into being, sometimes over at least a decade and despite any number of opposition parties, which are not so much defeated as coopted or persuaded or cajoled into what Sabatier and Jenkins-Smith called an "advocacy coalition." Moreover, these authors' particular contributions to policy research—as opposed to, say, Kingdon's on agenda setting—are significant, as they focus explicitly on differences between dynamic and static policy elements. In this way, Sabatier and Jenkins-Smith provide greater awareness between (what they call) secondary versus core issues and try to incorporate changes in such "values" as they permit norms to become a formal part of the policy considerations.

We can identify analogous contributions on the part of Frank Baumgartner and Bryan Jones (1993), as they described events in terms of activities that they called regular "triggering events," resulting in "punctuated equilibria," leading naturally to the establishment of a new political status quo. They, too, fall easily into the area encompassed by issues of policy-initiation—specifically, how the media serve as a surrogate for emerging policy issues.

Much the same set of arguments might be made about program evaluation. Although new approaches to program evaluation are constantly being proposed and tested (see, e.g., Fischer, 1995, for evaluation from a postpositivist perspective), these do not destroy the utility of the policy process framework or undermine the necessary role of program evaluation.

All of these areas have historically been under-attended by policy analysts (see Schön and Rein, 1994; also deLeon, 1994a), and this neglect has adversely affected the insights offered by the consensual policy framework. But Sabatier and

Jenkins-Smith do not necessarily undercut the legitimacy or viability of the policy process approach.

ON BALANCE

Regardless of the contributions of Sabatier and Jenkins-Smith, one still needs to ask if their charges regarding the, at best, marginal improvements for further research results in the policy process are commensurate with reduced research efforts in that vein. I propose a rather more positive response than Sabatier and Jenkins-Smith's gloomy prognostication of reduced research efforts, that the policy process framework will continue to serve as a valuable heuristic in both policy research and programmatic operations. First, as I have suggested, and despite Sabatier and Jenkins-Smith's repeated protestations, there is some doubt as to whether they and the advocacy coalition framework (ACF) have broken out of the paradigm created by the policy process orientation.⁶ And to be fair, it is not clear that we should want them to, for it is apparent that a great deal of pivotal research is still to be done within that framework as long as one can admit that the policy process is *not* a model in the formal sense of the word.

Brewer and deLeon (and other "policy processers," I suspect) prefer to reflect upon the policy process/stages heuristic as a basis for viewing and categorizing actors and actions in ways that help unravel and elucidate given policies, both in retrospect (always, of course, the clearer view) and—more cautiously—in the future. As most observers fully know, these benefits are no small accomplishments, even if they do not create a clear view over the next policy mountain, let alone anticipate it. To argue over whether policy process represents a "model," a "metaphor," or a "heuristic" serves little purpose as long as we recognize its main strengths (i.e., that it is a means for categorizing policy actions as they vary from stage to stage) and attendant weaknesses (e.g., that it has a lack of predictive capabilities) and act accordingly. For instance, Steven Waldman's (1995) masterly account of the AmericaCorps legislation is perfectly clear in using the concepts developed within policy formulation, even though Waldman made no conscious appeal to the policy stages framework.

One can make the case that many of the more radical iterations of policy research—I mean the postpositivist themes, including research in hermeneutics and critical theory—could also be easily incorporated into the policy process paradigm (see Hawkesworth, 1988, and deLeon, 1997). Marie Danziger (1995) made the case (drawing from Foucault and others) that the "objective" basis of policy analysis is little more than a subjective judgment and cannot be used as if it were scientific "fact." Critical theory, as an example, makes the case that "systematically distorted communications" threaten the foundations of good policy and social legitimacy, that is, according to Jürgen Habermas, "communicative rationality" (see, e.g., Forester, 1985, 1993). It would be an easy transition from crit-

ical theory to describe a movement encompassing greater subjectivity or going toward greater communicative rationality in terms of improved problem definition. Lasswell originally called this stage the "intelligence" function, and later scholars (e.g., Brewer and deLeon, 1983) referred to it as the initiation stage. Other postpositivists, such as Fischer and Forester (1993), could be similarly located. A model as carefully structured as Sabatier and Jenkins-Smith's ACF would be unable to encompass these newer policy approaches, such as communicative rationality and postpositivism.

Likewise, new contributions to policy research, such as ethnography or mediated negotiation, can also be fitted into the policy process model without undermining—in fact, enhancing—its validity for understanding, working on, or, more to the point, improving the quality of information provided to government. This last task, of course, was one of Lasswell's original and enduring charges. In the above examples, if we attribute any credibility to the cited research approaches (and I propose few would entirely disown them), policy scholars such as Sabatier could be seen as possibly inhibiting the advancement of the policy sciences by clinging tenaciously to the problematic tenets of positivist thought and procedures. Conversely, these alternative concepts can readily be captured by the policy process framework.

The more pressing question is not "whither the policy process" but whether the policy process framework (or heuristic) can be useful in moving the policy sciences toward a set of policy-oriented theories. The quest for a policy theory was, after all, the clear intention of Sabatier and Jenkins-Smith and of Elinor Ostrom, not the denigration of the "straw man" policy process heuristic. To this particular question, the answer must be much more agnostic. Lasswell's (1971) proposed "maximization theorem" is a candidate (see Brunner, 1991, pp. 77–78); it

holds that living forms are predisposed to complete acts in ways that are perceived to leave the actor better off than if he had completed them differently. The postulate draws attention to the actor's own perception of the alternative act completions open to him in a given situation.

However, the maximization postulate is less consonant with an empirical theory than it sounds. In the first place, it seems entirely too dependent on traditional economic reasoning; there are simply too many instances in which *imperium economia*—however convenient and enticing—does not prevail, as Amitai Etzioni, Robert Bellah, and the communitarians are quick to point out. Second, the maximization postulate is too prone to understandable ambiguity, depending, as it does, on "the actor's own perception." Nor does Yehezkel Dror's (1971) advocacy of metatheory seem particularly persuasive, even after twenty-five years.

Unfortunately, the standard disciplinary formulations are even more suspect and querulous in terms of theory building. For example, let us take the case of benefit-cost analysis: U.S. president Bill Clinton accepted an entirely new budget-

busting component in 1996 when he ordered expanded disability benefits for U.S. Vietnam veterans who might have contracted prostate cancer, basing his decision on scientifically inconclusive evidence linking prostate cancer to the herbicide Agent Orange. Considering that close to 3 million men fought in Vietnam and that 10 percent of all men (regardless of their Vietnam experience) contract prostate cancer, the relevant government benefits could be substantial (Purdum, 1996). More to our point, President Clinton's actions hardly seem to validate a strictly economic approach. Or institutional analysis: In a similarly iconoclastic manner, Paul Sabatier and his colleagues (1995) indicated that the standard institutional pressures seemed to be less than compelling in examinations of the operations of the U.S. Forest Service. Or even "objective" economic data: *The Economist* ("Damned Lies," 1996, p. 18) opined that "finding the right number is much harder than you might think. . . . Many of these activities cannot be seen and cannot be numbered." These and numerous other instances emphasize the complexity of policy actions that render analysis from a limited perspective less than useful (Bobrow and Dryzek, 1987) and, concomitantly, enhance the value of the policy process model.

In these cases, one can justifiably wonder if the policy sciences would be better served if they adopted more of a systems analysis perspective (read: policy process "model") as opposed to a general systems perspective, that is, if they accepted tentatively the policy stages/process for its constrained worth and leave it behind when bigger and better things materialize. The systems model is certainly more holistic in its approach, searching for, perhaps stumbling toward, the "big picture," replete with the requisite daunting big-picture complexities and linkages. In physics, a general systems perspective would be known as a *grand unifying theory*; in other words, most physicists' Holy Grail. However, systems *analysis* (from the Greek, "to loosen or break apart") attempts to answer the question of the disaggregated parts in lieu of the much more forbidding whole. In its defense, we can admit that linkages can remain elusive (or that the reconfigured whole is different from the earlier, unreconstructed body), but at least we have some idea as to what makes up (or, just as important, what does not make up) the parts. Although this information may appear as little more than isolated and unrelated, we know from Thomas Kuhn (1962) that these parts are the founding elements of "scientific revolutions." Given the idiosyncratic episodes addressed by most policy research, we might well be better served to devote ourselves to the quest for a series of mid-range theories, as Robert Merton (1968) set out to do some years ago, or even toward a better, generalized understanding, rather than a policy grand unifying theory.

In the case of the policy process, we still may be unable to grasp how the entire system works *in verifiable* (or, if one prefers Karl Popper's terminology, *falsifiable*) theory, but with the assistance of, for example, Eugene Bardach (1977), we have a much better idea of how agendas are formulated and policy alternatives presented. And as Charles Lindblom (1990; Lindblom and Cohen, 1971) has repeatedly advised us over the years, although Holy Grails are all well and good and fully warrant our lusting after them, still "usable knowledge," "lay probing," and

"muddling through" do provide a valuable illumination of their own. Or in the famous phrase of John Maynard Keynes, "It is better to be roughly right than precisely wrong," especially when we know that the precisely wrong will surely find itself manifested with great certitude into public policies.

CONCLUSION

In short, before we discard a useful friend—in this instance, the policy process or policy stages framework—we need to make sure, first, that it really does warrant a place in the dustbin of abandoned paradigms; second, that we have a better, more robust framework on which to rely; and third, that even in our quest for the theoretical, we have little use for the operational. None of these criteria (or the alternative models) argues decisively or even very strongly for abandoning the policy stages framework.

The policy process paradigm has never given us everything we might have wanted from it, so we need to ask two additional questions: In lieu of alternative policy formulations, have we loaded an impossibly heroic stature upon the policy stages framework? And more centrally, exactly what are we asking it to provide? A theory of political change or occurrences? Perhaps, but what about other—and now neglected—stages of public policy? And, failing that, as we certainly must, then certainly operational insights or, as Lasswell observed, "better intelligence leading to better government" is an acceptable alternative to empty theorizing. In Lasswell's own words (quoted in Brunner, 1991, p. 81):

It is the growth of insight, not simply of the capacity of the observer to predict the future operation of an automatic compulsion, or of a non-personal factor, that represents the major contribution of the scientific study of interpersonal relations to policy.

It was, of course, F. Scott Fitzgerald—the consummate policy analyst for the Roaring Twenties—writing about the fatally deluded Jay Gatsby, who offered what could be an appropriate paean to the troubled and maligned policy stages framework, heuristic, or model:

Gatsby believed in the green light, the orgiastic future that year by year recedes before us. It eluded us then, but that's no matter—tomorrow we will run faster, stretch out our arms farther. . . . And one fine morning—

NOTES

1. In the early 1980s, when Garry Brewer and Peter deLeon were finalizing their *Foundations of Policy Analysis* (1983), they asked Lasswell if he might prepare a foreword. He chose not to, explaining that the book and its format were fine just as they were.

2. Perhaps of equal importance, these stages assisted in the design of a number of academic curricula, engendering a flurry of policy design, estimation, and evaluation courses.
3. Obviously this is meant to be a representative rather than an exhaustive listing; apologies to those missing are hereby given.
4. On 25 November 1986, President Ronald Reagan explained to the American public that although "our policy goals [in dealing with the revolutionary government of Iran] were to be well founded . . . information brought to my attention yesterday convinced me that, in one aspect, the *implementation* of the policy was seriously flawed" (emphasis added), thus announcing the denouement of the Iran-Contra scandal.
5. A more complete review of Sabatier and Jenkins-Smith's essay is deLeon (1994); also see Lawlor (1995). Sabatier (1991) drew on more than just the policy process/stages framework, as he also included Elinor Ostrom (1990) and Richard Hofferbert (1974, 1990) in his criticisms.
6. The source here is numerous conversations with Professor Sabatier on this subject. One can fairly cite Professor Sabatier's earlier mastery of the implementation literature as evidence that his disavowal of the policy process is recent at the very best.

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