

The policy movement as a policy problem

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Abstract. The policy movement is unified by a common interest in the improvement of policy decisions through scientific inquiry. The movement is differentiated, however, because this common interest is highly ambiguous and subject to interpretation from different perspectives. This paper applies a policy sciences perspective to the movement's disappointments over the last few decades, and in particular, the failure to realize earlier aspirations for rational, objective analysis on the more important and controversial policy issues. The paper offers a definition and diagnosis of the underlying problem, and suggests what can be done about it as a matter of individual and collective choice.

Introduction

A policy movement has emerged over the four decades or more since the original conception of the policy sciences was crystallized by Lasswell and his collaborators.¹ The common interest of the movement has been to improve policy decisions through scientific inquiry. This common interest provides for some degree of solidarity within the movement, but also serves externally to justify the emerging profession and to constrain professional roles and responsibilities.²

The policy movement is differentiated, however, because the common interest is highly ambiguous and subject to interpretation and elaboration from different perspectives. (In this respect, the policy movement is no different from any other social or political movement.) Thus, for example, various disciplinary perspectives gave rise to distinguishable parts of the movement now roughly identified by such terms as 'public affairs' (philosophy), 'policy analysis' (economics), 'management science' (public and business administration), 'policy studies' (political science), and 'socio-economics' (sociology). The 'policy sciences' are not easily identified with any particular discipline, although the main institutional base of the policy sciences has been the Yale Law School.³

This differentiation complicates any assessment of the policy movement as a whole: The various parts tend to differ in their judgments of the relevant standards, data, and inferences to be drawn from them, whenever their judgments are made explicit. Nevertheless, in my judgment, the results typically have fallen short of the aspirations for rational, objective analysis that were dominant in the policy movement two decades ago, when the first-generation policy schools were established. Despite many small successes on technical

issues, there have been many disappointments on larger and more controversial issues in such areas as welfare, energy, security, and the economy: Decisions still tend to have significant unintended and adverse consequences, despite the availability of many more policy analyses; and analyses themselves often merely reflect and reinforce prior political divisions within the policy arena.

These disappointments are, paradoxically, reason for optimism about the future of the policy movement. They have begun to stimulate reconsideration of the aspirations of two decades ago, resulting in *new* interests in such neglected topics as problem definition, interpretation, and value critical inquiry – and *renewed* interest in the policy sciences, which has remained a distinguishable part of the policy movement. Among other things, the original conception of the policy sciences anticipated limits to rationality and objectivity on the part of the scientists; clarified the epistemological foundations of those limits; and devised means of improving scientific inquiry within those limits. The new interests in the policy movement have begun to converge with the policy sciences.

The purpose of this paper is to consider the disappointments of the policy movement as a policy problem, from a policy sciences perspective.⁴ Three basic questions are addressed: First, what is the common problem underlying these disappointments? Second, what accounts for it? Third, what can be done about it? A related purpose is to encourage others to take the policy movement as a policy problem, from the perspectives of other parts of the policy movement. Comparisons among the questions and answer will, I hope, inform the individual and collective choices that will shape the future of the policy movement.

I. The common problem

There are many possible answers to the first question, What is the common problem? Here we consider a definition of the common problem from a policy sciences perspective, some other definitions, and their bearing on the clarification of our professional roles and responsibilities.

A. A definition

For an answer to the first question, my colleagues and I have reconsidered our own practical experiences as well as appraisals by others of analytical errors in a wide variety of policy decisions. This broad base of specific experiences indicates a recurring pattern, which amounts to a definition of the common problem underlying the disappointments of the policy movement.

Simply stated, most *preventable* errors of policy analysis stem from the analyst's perspective: As the analyst simplifies a problem to make it tractable

for analysis and action, some important part of the relevant context is misconstrued or overlooked altogether. The analytical error – what is misconstrued or overlooked – becomes apparent only in retrospect, after resources have been committed and the unintended and often adverse results start coming in. Consider the following three illustrations of the common problem, selected from a larger sample.⁵

* Gordon Lewis examined cutbacks in eligibility for Aid to Families with Dependent Children (AFDC) at the beginning of the Reagan Administration. These cutbacks were intended to reduce social welfare costs. However, the unintended outcome of the cutbacks was probably an increase in overall costs for one service covered by AFDC, day care. The cutbacks in AFDC provided incentives for recipients to divert demand for day care services to another program, Social Security, which also covered day care but at higher costs to the federal government. The analytical error, a common one according to Lewis, was that analysts and public officials had reduced the problem to one program, and therefore overlooked interactions among programs.⁶

* Paul Stern observed that analysts typically relied on the simplifying assumptions of economic theory, often further simplified to build analytically tractable models, in their studies of energy demand. This reliance on economic theories and models 'misleads analysts by focusing attention on a few of the important bases of energy demand and away from several others that are critical for understanding and for policy.'⁷ The other important bases of energy demand are broadly psychological. They include communication, trust, convenience, commitment, information, and consumer misperceptions. The analytical error was that analysts had reduced the problem to the theories and methods of a particular discipline, and therefore unwittingly accepted 'blind spots' in the search for better policy.

* Colin Gray contended that strategic scholars who arrived in Washington in 1961 were partly responsible, a decade later, for the debacle in Vietnam and in other areas of foreign policy.

The methodology of the civilian strategists has been dominated by an 'economic conflict' model. The assumption that international conflict can be analyzed in terms of rational 'strategic men' has been vital for the progress of theory-building in strategy, but it has proved fatal to the relevance of theorists who have shifted from model-building to prescription.⁸

The analytical error was that the civilian strategists had reduced a practical problem to a rationality assumption, which was more suitable for theory than for practice.

These three illustrations bear more directly on the limited rationality of decisions based on expert analyses. A fourth illustration bears more directly on the limited objectivity of such analyses.

* Baruch Fischhoff and colleagues have examined continuing controversies over the risks associated with new energy production facilities. These con-

troversies stem from different definitions of risk, but analysts typically defend their definitions as objective. The authors show, however, that no definition of risk can be entirely objective: Any definition includes some of the effects of a facility, but necessarily excludes others. Hence the definition necessarily expresses the analyst's subjective opinion.

Thus, objectivity should always be an aspiration, but can never be an achievement of science. When public and experts disagree, it is a clash between sets of differently informed opinions.⁹

The error lies in the attempt to reduce a technical definition to an analytical act, even though a technical definition is also a political act.

Notice in these illustrations that errors stem from a fragment of knowledge used to simplify a complicated problem: The problem is reduced to a program, or the theories and methods of one discipline, or a rationality assumption, or a technical definition. Other fragments of knowledge may likewise induce the analyst to overlook or misconstrue important aspects of the problem at hand.¹⁰ Notice further that technically-sophisticated methods or theories do not prevent these errors of analysis. If something important is overlooked or misconstrued, technical sophistication merely compounds the error. Finally, notice that in each illustration there is a significant difference between the analyst's subjective map of the problem, and the problem as it exists in the real world. The difference becomes manifest through action, as the actual consequences of an act diverge from the expected consequences.

Plato recognized this difference in the allegory of the cave in *The Republic*. Walter Lippmann explored it in a chapter on 'The World Outside and the Pictures in Our Heads,' first published in 1922.¹¹ Drawing on such ancient and modern classics, Harold Lasswell incorporated the difference into the maximization postulate, which, as we shall see below, is the core postulate of the policy sciences. Herbert Simon later reformulated the difference as the first consequence of the theoretical principle of bounded rationality, which is currently the most influential formulation:

The capacity of the human mind for formulating and solving complex problems is very small compared with the size of the problems whose solution is required for objectively rational behavior in the real world. . . .

[T]he first consequence of [this] principle of bounded rationality is that the intended rationality of an actor requires him to construct a simplified model of the real situation in order to deal with it.¹²

Other formulations of the difference are also current in traditional disciplines ranging from anthropology to economics and philosophy.¹³

Thus the definition of the common problem is grounded not only in a broad base of specific experiences, but in systematic reflection on such ex-

periences across many disciplines. The definition implies that the task for policy analysts is to recognize the difference between 'the world outside and the pictures in our heads' and to learn how to see more of the relevant context more reliably.

B. Other definitions

Other definitions of the problem, more or less explicit, demonstrate that the definition above is neither tautological nor obvious. These other definitions locate the problem in the policy arena, in the lack of theory, and in the lack of professional recognition, rather than in the analyst's perspective.

Practicing policy analysts sometimes locate the problem in the policy arena. Alice Rivlin, for example, has considered the record of policy analysis on important issues like macroeconomic policy. The general problem is that 'On the one hand, there is paralysis, deadlock, and stalemate; and on the other, the enormous attraction of panaceas as a cure for our most serious ills.'¹⁴ Some of the problem stems from the explosion of policy analysis, which overloads public officials with more information than they can manage. The overloads are exacerbated by the technical jargon often used in analyses and by the tendency of analyses to heighten awareness of uncertainties about complex problems. The resulting frustration

... has led many otherwise sensible people to hope, against reason, that there might be some easy, new solution – something no one had yet thought of – that will suddenly solve everything. Nevermind that the latest cure-all is counter-intuitive, that it conflicts with common sense as well as with the accumulated evidence of how our system works.¹⁵

In Rivlin's view, the explosion of policy analysis provides tools for understanding problems, but no easy solutions.

This definition provides the basis for three recommendations: That we distinguish what is uncertain from what is not, that we develop proposals (even if they are mundane) offering near-term practical results, and that we 'try to write succinctly and in English.'¹⁶ This definition also implies that our role is to provide advice to those who are duly authorized to make decisions. Beyond advice, however, we *cannot* do anything more to improve the policy arena, in which the problem is located under this definition. The recommendations and implications of this definition do not take us very far toward improvements in the advice we *can* provide.

The more academic specialists in policy sometimes locate the problem in an alleged lack of theory. This definition is at least implicit in a preoccupation with theory to the exclusion of practice – even if the preoccupation is rationalized as a step toward unspecified improvements in future practice. This definition is explicit in claims that we lack normative theory for assessing, in

specific cases, what constitutes 'improvements' in policy decisions or decision processes; or in claims that we lack empirical theory to realize such improvements. Empirical 'theory' is variously interpreted as a conceptual framework, as causal relationships among variables, or as the integration of such relationships into comprehensive causal theory.

No doubt advances in empirical and normative theory are possible and desirable insofar as they contribute to practical policy inquiry. From a policy sciences perspective, however, there is no lack of central theory, which is something quite different from causal theory.¹⁷ An adequate body of central theory – comprised of concepts as well as normative and empirical propositions – has been available for some time. Lasswell made this claim in 1956, and illustrated it with a review of the atomic bomb decision and a preview of decision problems expected to arise from science-based technology. Within this rich theoretical tradition, the most significant task is to relate central theory to observations on specific contexts as events unfold. There is inevitably a gap between observations on specific contexts and central theory, which is necessarily formulated in general and ambiguous terms.¹⁸ What we lack are institutions for bridging the gap on a continuing basis.¹⁹

If taken too seriously, demands for the elaboration of theory could become a diversion from (or even a substitute for) practical policy inquiry. The demands could rationalize the postponement of inquiry into specific practical problems, pending some grand theoretical breakthrough. They could divert resources from inquiry into such practical problems, however pressing and important those problems might be outside academia. They could, over a period of time, reduce the policy movement to another ivory tower in the academic landscape, with little relevance to practical problems outside. And even if some latter-day Isaac Newton should achieve a grand theoretical breakthrough, practical relevance would still require the painstaking specification and elaboration of theory through observations on particular, ever-changing contexts.²⁰

Still another definition of the common problem is an alleged lack of professional recognition from the public or public officials, which constrains investments in policy analysis and reduces the impact of analysis on policy decisions. The empirical grounding of this allegation is questionable at best.²¹ In any case, the implied demand for professional recognition could divert effort and other resources into promotion of the policy movement, and away from our common interest in the improvement of policy decisions.

The demand bears a passing resemblance to demands by other professions to improve their relative value positions. Such demands are typically justified in terms of broader social interests. However, as an increasingly skeptical public learns the difference between professional service and professional self-service, the justifying rhetoric becomes an inflated currency. Over the long-run, professional recognition depends on the extent to which performance corroborates claims, as assessed by representatives of those broader

interests. Our resources are better invested in improving performance, and in listening to our critics, than in self-promotion.²²

C. Alternative roles and responsibilities

Any definition of the common problem has some bearing on the clarification of our professional roles and responsibilities.

We incur a general responsibility when we accept resources from society as a whole on the promise that such resources will be used, in good faith, to improve policy decisions through scientific inquiry. Our responsibility is limited, however. We are not responsible for policy decisions. As Rivlin suggests, our role is to advise the public and public officials who are authorized to make policy decisions, and not to usurp their role or responsibility.²³ Moreover, we are not responsible for those decision-makers who are uninformed because they have rejected or ignored adequate policy inquiries adequately presented to them. Furthermore, we are not responsible for a lack of adequate policy inquiries when the necessary resources – time, funding, and access to closely-held information, for example – are denied to us.

Within such limits, however, we are responsible for making full use of every opportunity open to us to improve policy decisions through scientific inquiry.²⁴ What this responsibility means depends in part on how we define the shortcomings of our work as 'scientific inquiry.' If the definition offered here is essentially correct, most preventable errors of analysis stem from the analyst's perspective. These errors are preventable because the analyst to a large extent can control his or her perspective on a particular policy problem. And for the same reason, the analyst is responsible for his or her perspective and for any errors of analysis arising from it.

The meaning of our responsibility also depends upon the consequences of our policy inquiries, quite apart from their shortcomings as science. An 'improvement' in policy decision can be assessed with respect to such broader common interests as freedom and human dignity, or with respect to their opposites. An 'improvement' from inquiry can also be assessed with respect to individual or shared interests within the policy movement, regardless of broader common interests. For example, analysts may tacitly collude in a minor-league game that defines an 'improvement' as a redistribution of status and income among themselves, or in a slightly larger game that defines an 'improvement' as a redistribution of values in favor of the policy movement at the expense of other social groups. These games transform the professional justification into a rationalization.

In summary, the responsibility that follows from our professional justification is ambiguous. What it means depends on the shortcomings of scientific inquiry as we define them, and on the social consequences of such inquiry – who wins and who loses with respect to the many values at stake – as we

assess them. These meanings are continuously specified as we carry on our work, whether or not we are consciously aware of them. They are important enough to be made explicit as a matter of conscious choice.

II. Epistemology

Now consider the second question, What accounts for the common problem as defined above? I shall contend that the assumptions of positivism exacerbate the common problem, while the assumptions of alternatives to positivism help ameliorate the common problem.²⁵ Positivism and the alternatives are epistemologies, which purport to clarify what we can know, how we can know it, and how we can know we know it. But they also serve to explain and justify different understandings of 'scientific inquiry' and standards of research and practice associated with those understandings.

A. Positivism

I. Basic assumptions. For most policy analysts, positivism is science. Positivism postulates the existence of repetitive behavior under universal covering laws, exemplified by the laws of motion discovered by Isaac Newton in the 17th century. Thus the diverse behavior of inanimate objects – a planet, a falling apple, a pendulum – as well as the behavior of living forms may be reduced in principle to invariant relationships that represent a fixed, underlying reality.

A valid covering law includes all the antecedent conditions that cause the behavior to be explained or predicted. Whenever these conditions are instantiated in a particular context, the behavior in question occurs. The law does not pertain to all that goes on in a particular context, but only to that which is regular and repetitive across contexts; the other aspects of those contexts are irrelevant.²⁶ Covering laws are context independent in this sense.

The most influential form of positivism in the policy movement is positive economics. The ethical standpoint, scientific purpose, and research standards of positive economics were summarized in the following terms by Milton Friedman:

Positive economics is in principle independent of any particular ethical position or normative judgments. ... Its task is to provide a system of generalizations that can be used to make correct predictions about the consequences of any change in circumstances. Its performance is to be judged by the precision, scope, and conformity with experience of the predictions it yields.²⁷

An important theoretical generalization is in itself necessarily inaccurate

because it abstracts from many contexts that which is repetitive.²⁸ Only the predictions deduced from the generalization, not the generalization itself, are relevant to an empirical test.

Thus theoretical progress depends upon empirical testing of quantitative predictions deduced logically or mathematically from existing hypotheses. But progress also depends upon creating new hypotheses. However, creating new hypotheses is a subjective matter beyond the purview of positive scientific method. As Friedman put it,

The process [of creating new hypotheses] must be discussed in psychological categories, not logical categories; studied in autobiographies and biographies, not treatises on scientific method; and promoted by maxim and example, not syllogism or theorem.²⁹

The point of positive scientific method is to render the subjective origins of an hypothesis irrelevant to the objective results of an empirical test.

The rise of positivism in the behavioral sciences around mid-century was motivated by the Newtonian ideal,³⁰ and supported by the introduction of quantitative and formal-deductive methods. Positivists assumed that if the behavioral equivalents of Newton's laws could be discovered, they would provide a basis for rational and objective policy. Rationality would be served because the consequences of policy alternatives could be predicted with precision and accuracy, independent of the particular context. Objectivity would be served because these predictions would be independent of the positivist's subjective viewpoint: Anyone else could employ positive scientific methods to replicate the results.

Thus, in principle, the discovery of a valid system of generalizations would reduce controversy in the policy arena to differences over value judgments. And the positivist scientist would be able to maintain a neutral position above and apart from these controversies, which are separate matters beyond the boundaries of positive science.

2. *Assessments.* Judged by its own standards, positive science does not perform as promised. After roughly four decades of behavioral research, positivists have not yet discovered universal covering laws that predict human behavior with accuracy and precision, independent of both context and the positivist's viewpoint.

Consider rational choice theory as one example.³¹ The central generalization, an hypothesis in the form of a covering law, is that any actor behaves as if he were objectively rational: He chooses the alternative that maximizes the expected value of outcomes. Logically this entails a God-like omniscience – knowledge of all alternatives, all outcomes, and a consistent utility function for valuing all outcomes. Such omniscience is beyond the capability of man or machine. But recall that what matters are predictions from the hypothesis, not the plausibility of the hypothesis itself.

To predict behavior for an empirical test, the analyst must specify auxiliary hypotheses representing the actors' view of the choice situation. For example, actors such as 'politicians' are assumed to maximize a value such as 'power,' given the known outcomes of the few alternatives available. These auxiliary hypotheses are typically standardized across actors, fixed across time, and otherwise kept simple – in accord with the ideal of reducing complex behavior to the simplest terms, and in accord with the practical requirements for mathematical deduction.

However, there can be no clean empirical test of the central hypothesis of objective rationality: The results of the empirical test depend upon the analyst's specifications of the situation. If the analyst's specifications of the auxiliary hypotheses approximate the actors' views of the situation, then predicted choices may approximate observed choices. Otherwise, they will not. Moreover, the results of the test will vary if the actors' change their views of the situation, or if the situation itself changes. And this is typically the case, as actors adapt to situations through trial and error, and the situations themselves evolve.

In short, the objective rationality hypothesis is by itself untestable; the results of any test depend upon the analyst's specification of a particular context; and the results are restricted in scope to that context.

Consider macroeconomic theory, in which theoretical generalizations are expressed in complex models to forecast aggregate behavior over various time horizons. This constitutes perhaps the largest domain of experience in forecasting by formal-deductive methods. However, on-going appraisals by the American Statistical Association and the National Bureau of Economic Research, among others, show that the results are disappointing by various standards.³² In general, there has been no detectable improvement in forecast accuracy since the early 1950s; judgmental forecasts are typically as accurate or more accurate than forecasts based on complex formal models; and true ex-ante forecasts based on formal models are less accurate than forecasts incorporating judgmental adjustments in model specifications.

Why has the continuous development of complex forecasting models failed to improve forecast accuracy? Ascher's interpretation is the most insightful:

[T]he 'improvement' of substituting new specifications for old ones merely replaces the representation of the previous context with a representation of the newer context, but without coming closer to a more generally valid representation. There is no a priori reason why there should be a set of aggregate-level propositions that are generally valid over time.³³

The explicitness and complexity of such models, as well as their dependence on law-like relationships, inhibit timely revisions as the economy evolves.³⁴ Moreover, the evolution of the economy is open to technical innovations and discretionary political decisions that are difficult to predict and are typically

ignored by economic forecasters.³⁵ In short, the models lag behind the open, evolving context and never catch up.³⁶

Finally, consider the laboratory experiment, the epitome of positive methods used in policy planning and evaluation. Under optimal conditions, analysts can control conditions for observing the effects of alternatives on subjects randomly assigned to treatment groups, isolate these effects from concurrent events outside the laboratory, and replicate the experiment many times. But even under these optimal conditions, strong generalizations that predict with precision, scope, and accuracy have eluded us. Cronbach, for example, reports that decades of experiments on aptitude by treatment interactions (ATIs) in educational psychology have produced only weak or inconsistent generalizations, and cites parallel examples from personality and cognitive psychology.

Explanations for our inability to find strong theoretical generalizations through experimental methods emphasize the prevalence of untested, higher-order interactions. As Cronbach put it,

An ATI result can be taken as a general conclusion only if it is not in turn moderated by further variables. If Aptitude \times Treatment \times Sex interact, for example, then the Aptitude \times Treatment effect does not tell the story. Once we attend to interactions, we enter a hall of mirrors that extends to infinity. However far we carry our analyses – to the third order of fifth order or any order – untested interactions of a still higher order can be envisioned.³⁷

The higher-order interactions are beyond the practical reach of a direct experiment. They nevertheless show up as unexplained variance and inconsistent results that frustrate generalization.³⁸

Such interactions reflect the complex conditioning of human behavior, which cannot be fully isolated, controlled, or replicated even in laboratory settings. The observed effects of treatments depend upon a wide range of predispositions that people import into an experiment, in addition to those identified in the experimental design. Effects also depend upon the fine structure of the treatment and other factors not included, or even perceived, by the experimenter. Effects also depend upon the temporal milieu. As Cronbach put it, 'Generalizations decay. At one time a conclusion describes the existing situation well, at a later time it accounts for rather little variance, and ultimately it is valid only as history.'³⁹

Specialists in the practice of management and decision, not just theory, have arrived at similar conclusions. Consider, for example, how Ralph Siu summarizes the context of practice in the policy arena:

[E]verything is continually changing – not only the events themselves, but also the very rules governing those events. This kind of arena is alien to the scientific tradition of fixed boundary conditions, clearly defined variables,

nonsubjective assessments, and rational consistency within a closed system. In the ball game of competitive actualities, everything is in flux, and all systems are open.⁴⁰

Fortunately, alternative understandings of 'science' provide a basis for integrating theory and practice, as we shall see below.

3. *Divergence.* Responses to the apparent failures of positivism have been diverse. Some have rejected positivism altogether in favor of alternative epistemologies. Others have retained the orthodox faith on the false expectation that it has already succeeded by its own standards or the dubious expectation that it may eventually succeed if they keep trying. Between these extremes are many responses that abandon peripheral tenets of positivism in order to preserve faith in the core tenets. (In technical terms, these are defenses through partial incorporation.) But there is no necessary agreement on what is peripheral or core.

For example, a neo-positivist who takes a position on a controversial policy issue abandons one tenet – the boundary between normative and positive commitments – but not necessarily other tenets. A neo-positivist who realizes that relationships are context dependent may abandon the Newtonian ideal but retain faith in positive methods. A neo-positivist who discovers that 'hard' positive methods do not guarantee objective, replicable knowledge – as is the case when multiple applications to the same problem produce different results – may incorporate 'soft' interpretive methods.

Perhaps the most prevalent pattern has been to abandon the quest for universal covering laws, but to retain some faith in positive methods. Consider, for example, an anonymous response to an earlier assessment of positivism according to its own standards:

Many, including myself, who use positivist-like methods (among others) are not constantly in search of such covering laws and even admit that human behavior may not perfectly, or even closely fit the repetitive behavior assumptions of pure positivist theory. And by God it still is of some use in policy analysis and political inquiry.

The issue is not the utility of positive methods but their appropriate roles and justification in the conduct of specific inquiries.

Such responses quietly revise the nominal research standards of positive science which, as we have seen, are the accuracy, precision, and scope of predictions. The revised and effective standards become the mere use of quantitative or formal-deductive methods or the statement of hypotheses in universal forms. In either case, true *ex-ante* predictions are not necessary for research to survive peer review. It is normally sufficient to demonstrate a reasonable fit between observations on a specific context and *retrodictions* from theory – which defers demonstrations of the limited scope of the empiri-

cal test to others. The pages of behavioral science journals are littered with demonstrations that previously-published generalizations are contingent rather than universal.

Nevertheless, the game continues: Positive theory and method serve to justify another conclusion about the specific context, and fit in the specific context serves to justify another theoretical generalization. The researcher finds it advantageous to conform because 'hard' methods and universal forms lend an aura of 'science' or 'objectivity' to his results. The reviewer finds it advantageous to enforce these standards because they are rather easy to apply: The positive 'scientific' form or methods of results are obvious, but the substantive grounding and consequences of those results typically are not. The justifications are logically fallacious but nevertheless persuasive to the extent that reviewers presume the positive understanding of 'science' and fail to appreciate the alternatives.

Positivism thus functions as a myth, providing a rhetorical justification for research standards based on 'hard' methods and universal forms, and for research that conforms to these standards.⁴¹ But the persuasive power of the rhetoric stems from the Newtonian ideal, not from the record of positive research in the behavioral sciences. And the revised standards transform 'scientific' forms and methods into the *de facto* ends of research. The substance of research is thereby reduced to a means of demonstrating 'scientific' prowess.

B. Other epistemologies

1. *Basic assumptions.* Other epistemologies typically postulate that behavior is selective according to the actor's subjective perspective, rather than determined by universal covering laws.⁴² The policy sciences tradition provides one of these epistemologies, but the origins and aims of that tradition are primarily pragmatic. The epistemology is not the criterion of 'scientific' inquiry, but rather a means of improving systematic, empirical inquiry on behalf of larger aims.⁴³

In the epistemology of the policy sciences, the selective characteristic of behavior is described by the maximization postulate. The postulate

... 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.⁴⁴

The actor's perceptions may be conscious and unconscious, deliberate and instantaneous, and valid and mistaken, in various degrees depending upon the situation.

The postulate distinguishes between the behavior of living forms and in-

animate objects. The behavior of inanimate objects is determined by forces in the external environment, according to Newtonian mechanics. So far as we know, a planet in orbit, a falling apple, or a pendulum does not have perceptions or an internal point of view that shapes its motions. In contrast, the behavior of a living form depends directly on an internal map of the self in the external environment. All factors in the external environment affect behavior indirectly through sensory impressions that impinge on the internal map.

This internal map is subjective because it is a selective and distorted representation of the self in the external environment, conditioned by genetic predispositions and sensory impressions through a lifetime of experience. These processes allow for variations in subjective maps from one actor to the next and over time for each actor. Consequently, just what the subjective map may be for a particular actor, in a particular situation, at a particular time, is always a matter of empirical inquiry.⁴⁵

The maximization postulate is the only postulate in the policy sciences. That is to say, its truth is taken as self-evident for purposes of reasoning. The self-evidence of the postulate is suggested by considering the obverse: That living forms act in ways that are perceived to leave the actor *worse* off than if he had acted differently. The obverse is absurd, if due allowance is made for the unconscious impulses and conscious perceptions, possibly mistaken, that are overly expressed in a given act. The task is to use the maximization postulate to infer from overt expressions why a given act 'made sense' to the actor in question.⁴⁶

Hypotheses in the policy sciences are based on such practical inferences about observed behavior using the maximization postulate, and are subject to clarification and correction when specified in particular cases.⁴⁷ These hypotheses refer to the subjective meanings of acts for the actors in question. The hypotheses of causal theory, in contrast, presume that observed behavior is a function of objective factors (or causes) that are unmediated by the subjectivity of the actors in question.

2. Implications. What are the implications of the maximization postulate for the basic questions of epistemology and for the role of the policy scientist?

The postulate implies that knowledge of behavior can be improved, but all such knowledge is context dependent. Every actor's subjective map falls far short of God-like omniscience in ideosyncratic ways. Consequently, every act is inevitably a matter of trial and error in some degree, and there are multiple trials. Over time, relatively successful interactions tend to be stabilized as personality traits or social institutions. Relatively unsuccessful interactions tend to stimulate creative adaptations. Thus the postulate allows for the creation of regularities in behavior, and for their modification and termination, at both individual and social levels.⁴⁸

The recent history of the policy movement is a case in point. Positivism has partially coordinated the subjective maps and professional behavior of many policy analysts. But when adherence to positivism is perceived to leave

analysts worse off from their own standpoints, they become receptive to alternative tenets and perhaps an alternative faith. And when adherence to such alternatives begins to pay off from their own standpoints, they begin to practice and institutionalize a new pattern.

The point may be summed up in the principle of contextuality: The meaning of anything 'depends upon its linkages with the context of which it is a part.'⁴⁹ No context of behavior is closed, and none of the linkages is invariant. The policy movement, for example, is a context open to new experience in temporal, spatial, and other dimensions. And the linkages among its component parts and with the global context are always changing. Thus analysts are not standard cogs in a universal mechanism that determines their behavior, even if analysts sometimes assume that others are. Rather, the analyst has a degree of choice as he or she observes and participates in an evolutionary process that no one can understand entirely.

We can know more about such processes, and our actual and preferred roles in them, through interpretation. Interpretation is a process of mapping the relevant context – guided by some purpose or problem, grounded in observations, and culminating in a more reliable basis for action. Interpretation relegates quantitative and formal methods to a respected but secondary role, typically more useful toward the conclusion of an inquiry rather than at the beginning.⁵⁰

From any initial map, the process of interpretation alternates between two phases so long as time and other resources permit: On the one hand, interpretation broadens observations and inferences beyond the current understanding. On the other hand, interpretation focuses attention on blind spots, contradictions, and other anomalies in the current understanding. Taken together, the two phases tend to make the previous understanding obsolete rather quickly. Over successive iterations, the two phases also tend to multiply the number of empirical and consistency constraints to be integrated into an improved understanding. In the process, inquiry becomes more than an elaboration of preconceptions: Genuine learning takes place.

Systematic guidance on what to look for and how to construe it can be found in the maximization postulate, in other parts of the conceptual framework, and in the various propositions that comprise the rest of central theory. (The conceptual framework of the policy sciences is discussed below.) Such tools provide systematic cues and questions for mapping any problem-relevant context. But the answers to those questions – the map itself – depend on observations of that context and the inferences drawn from them. The specific observations and inferences, in turn, are potentially unique understandings of the highly-abstract terms of central theory. The ability to integrate the specific and the abstract through interpretation is the key to making the most of whatever opportunity exists to construct a better map.⁵¹

The test of a map – how we know what we know – is not its conceptual, theoretical, or methodological pedigree, but its substantive grounding in the particular context and its substantive consequences when used as a basis for

action in that context. The substantive grounding of a map can be assessed with respect to the evidence in hand and with respect to the maps of others who are well-informed but represent different perspectives. Conversation and discussion are invaluable in this connection.⁵² But in the end, the assessment of a map depends upon action. Only by examining the substantive consequences of the map when used as a basis for action can we understand what has been overlooked or misconstrued – and correct the map accordingly for future action.

In short, 'science' in the epistemology of the policy sciences is understood as systematic, empirical inquiry.⁵³ This understanding goes beyond methods to substance. As Kaplan put it,

Policy must be scientific to be effective... But to say scientific is not to speak of the paraphernalia and techniques of the laboratory; it is to say realistic and rational – empirically grounded and self-corrective in application. Policy is scientific when it is formed by the free use of intelligence on the materials of experience.⁵⁴

This understanding also emphasizes the heuristic and instrumental role of central theory in scientific inquiry on behalf of larger purposes.⁵⁵

This brings us to the role of the policy scientist, and the purposes for which central theory is used. Under the maximization postulate, there is no distinction in principle between the behavior of the policy scientist and the behavior of other human beings. As Lasswell once put it,

To some extent we are all blind and no doubt will remain so. But there are degrees of impairment, and so far as decision outcomes are concerned, it is the responsibility of the policy scientist to assist in the reduction of impairment.⁵⁶

Thus the policy scientist must understand his or her own position as both an observer of and a participant in decision and social processes. This position differs markedly from the positivist's presumed value-neutral position above and apart from everyone else.⁵⁷

First, the policy scientist is distinguished by command of central theory and methods designed to help see more of any problem-relevant context, and to see it more reliably as a basis for action. The policy scientist participates by feeding symbolic representations of the problem-relevant context back into decision and social processes. This is a significant role if, as Lasswell contended, 'the act of perceiving new configurations is [probably] the most formative act in the process of shaping human history.'⁵⁸ Reliance on the manipulation of symbols distinguishes the policy scientist from other elites who also manipulate goods and services or the instruments of violence for various purposes.

Second, the purpose of the policy sciences as 'science' is to realize more of

the potential for free choice through the sharing of insight. The purpose is *not* prediction. As Lasswell put it,

[I]t 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.⁵⁹

Unperceived factors operating automatically, both within and outside the personality, constrain free choice. Insight brings factors of both kinds into the focus of conscious awareness, so that people are free to take them into account in making their own choices. Although 'knowledge utilization' is conventionally understood as a separate specialization, it is an integral part of the policy sciences tradition.⁶⁰

Third, the policy sciences recommend the realization of human dignity for the many (not just the few) as the overriding goal of policy. Under this recommendation, an 'improvement' in policy decisions is progress toward equal opportunity in shaping and sharing all value outcomes, including power, enlightenment, wealth, well-being, skill, affection, respect, and rectitude. As an exercise in specifying this general goal, Lasswell reviewed the Universal Declaration of Human Rights, which spells out many of the implications of the human dignity principle.⁶¹

In summary, the core principles of the policy sciences are the maximization postulate which (together with its implications) provides the basis for an understanding of 'science' as systematic, empirical inquiry; and the principle of human dignity as a recommendation which provides a basis for understanding 'improvements' in policy decisions. These core principles also serve to explain and justify the work of policy scientists, and standards of research and practice in the policy sciences.⁶²

3. Convergence. Various elements of the original conception of the policy sciences are now being rediscovered and developed, more or less independently, by a growing number of analysts exploring the alternatives to positivism. For example:

- There is growing interest in *problem definition*, as distinguished from problem solving. The premise is that the analyst necessarily constructs a definition of the problem, which cannot be taken as given. If the problem is misconstrued in the first place, the search for solutions is misguided and (in the absence of extraordinary good luck) ultimately futile.⁶³
- There is growing interest in *interpretation*, which goes beyond those considerations that are easily quantified or formalized. The premise is that meaning is always in question, and questions of meaning call for interpretation in context. The analyst cannot assume that such questions are settled once and for all through the operational definition of variables or the specification of formal models.⁶⁴

- There is growing interest in the *critique of values*. The premise is that neither the purposes, results, nor the social consequences of scientific inquiry are value-free. The scientist needs to make explicit, and examine, the value positions that are necessarily involved in his or her professional activities.⁶⁵

These premises are an important part of the practical wisdom of the more reflective practitioners.⁶⁶ The challenge for academics is to improve on the wisdom of practitioners through systematic, empirical inquiry, rather than to reject that wisdom as 'unscientific' according to the positivist understanding of 'science.'⁶⁷

Steps toward convergence on post-positivist premises have not yet culminated in the crystallization of consensus on post-positivist standards of research and practice within the policy movement. Within the policy sciences, however, there is a widespread preference for problem-oriented, contextual, and multi-method research over research of other kinds; and for practical applications that contribute to the fuller realization of human dignity. These standards are clarified in the conceptual framework, illustrated in various applications of the policy sciences, and justified and explained by the core principles of the policy sciences.

C. Alternative epistemologies and standards

Now consider once again the common problem of the policy movement, as defined above: Most preventable errors of analysis stem from the analyst's perspective. As the analyst simplifies a particular problem, some important part of the context is misconstrued or overlooked altogether. The deepest and most pervasive of the relevant perspectives are epistemological assumptions.

Positivist assumptions contribute to the underlying problem in several ways. They narrow the analyst's focus of attention to a small number of factors; they suggest that the relationships among these factors are fixed, standardized, and otherwise independent of context; and they divert attention from the significant role of the analyst's subjective viewpoint, including value commitments, in the conduct of inquiry. The resulting errors of analysis are obscured and perpetuated by the rhetoric of positivism.

Pre- and post-positivist assumptions, including those of the policy sciences, recognize and address the problem as defined above. They broaden the analyst's attention beyond the initial understanding of a particular problem; they direct attention to the context-dependent meanings of observations and relationships; and they make explicit the role of the analyst's viewpoint, including value commitments, in the construction of any context.

The analyst's choice would be relatively simple, if it were only a matter of improving scientific knowledge for decision and action in the real world. However, the choice is complicated by career considerations among other

things. Career advancement normally depends upon some minimum number of publications and grants. But post-positivism depreciates the value of an investment in technically sophisticated methods, and entails costly retraining in various interpretive and context-sensitive methods. Moreover, so long as the rhetoric and research standards of positivism prevail in peer review processes, post-positivist inquiry will be penalized.

Thus it is not surprising that post-positivist tendencies are concentrated among older, established analysts who are in a position to discount career considerations; and among younger analysts who were recently exposed to the epistemological alternatives in graduate school and are willing to accept some career risks. Intellectual courage – or camouflage – will continue to be necessary in some degree until the standards of positivism are displaced by new standards of research and practice, and the new standards are institutionalized.⁶⁸

III. Conceptual frameworks

Now consider the third question, What can be done? Recall that the task is to see more of the problem-relevant context, and to see it more reliably. My recommendation is to acquire facility in the use of a conceptual framework, adequately designed for the task, as the way to make the fullest use of the opportunities available.

A. *Alternative forms of knowledge*

Past experience is distilled into various forms of knowledge for application to future problems. Conventionally, for example, we distinguish among concepts, theoretical propositions, models, and various other forms. Fragments of knowledge in such forms – e.g., 'the AFDC program,' 'energy demand decreases as price increases,' and 'rational strategic man' – survive because they are considered useful for some purposes. However useful such fragments may be for problem-solving purposes, they are *misused* for the purpose of problem definition.

The reasons are apparent from the illustrations in section I above. The effectiveness of AFDC eligibility cutbacks in reducing welfare costs depended on program interactions that were overlooked. The potential for energy demand reduction depended on overlooked psychological factors as well as price. The debacle in Vietnam stemmed from a host of factors not included in the assumption of rational strategic man. To generalize from these examples, a fragment of knowledge in any form is unreliable in problem definition for several reasons: It reduces the factors considered in the problem definition to a subset of the factors that may turn out to be important in the real world. It misconstrues interactions among the factors considered and

those not considered. And the relationships among the factors considered tend to be stereotyped on the basis of previous experience.

Academic folklore (if not practice) recognizes the fallacy of using any fragment of knowledge to define a policy problem. The story of the blind men and the elephant is perhaps the best-known. But there is also the story of the man who lost his watch on the dark side of the street, then searched for it under the streetlamp on the other side – because that's where there was some light. The man defined the light as the boundary of the problem, but solving the problem required more light. Then there is the 'law of the hammer' generalized to other tools or methods: The man who loves his hammer defines every problem as a nail – whether or not pounding with the hammer is a solution to the problem. Finally, there are the victorious generals who prepare to fight the last war rather than the next. They define the two problems as identical problems because the last war was a problem successfully solved – despite the fact that the next war is inevitably a new problem.⁶⁹

The obvious strategy is to integrate many fragments of knowledge into a form specifically designed for problem definition. An eclectic strategy results in a collection of concepts, theories, or approaches, any of which runs an unnecessarily high risk that something important will be left out. A systematic strategy results in a conceptual framework. Properly designed, a conceptual framework identifies those distinctions that have consistently proven to be most important and useful across broad ranges of experience; crystallizes those distinctions into basic concepts that are conveniently labeled; and organizes them into a logically consistent framework for subsequent applications.

Both the substance and the form of a conceptual framework are designed to help the analyst construct a more reliable map of any particular context. This includes the integration of those fragments of knowledge that may be appropriate for understanding the particular context. But the framework is not a map in itself. It is a systematic set of concepts and labels that a policy scientist may use to construct specific maps to guide action in specific contexts – just as a geographer may use a systematic set of concepts and graphic devices to construct specific maps to navigate specific areas of physical terrain.

B. Criteria

Conceptual frameworks may be evaluated on substantive and formal grounds. Substantively, the key question is whether the framework does indeed incorporate the distinctions that have consistently turned out to be the most important and useful across broad ranges of experience. Formally, an adequate framework must be *feasible* to use within human cognitive constraints, *comprehensive* enough to cover the principal dimensions of the world outside, and *flexible* enough to use on any problem.

The flexibility criterion is important because a framework designed for use on a specific problem makes little sense: Quite apart from the fact that a practicing analyst cannot afford the time to devise such a framework for every problem that arises, such a framework would tend to reinforce preconceptions about the problem at hand. The point of a conceptual framework is to help the analyst move beyond those preconceptions as far as possible within the time available.

Feasibility requires that any dimension of the context be conceived in terms of a small number of categories. Research in cognitive psychology has shown that the span of short-term memory at any level of abstraction is approximately seven plus or minus two 'chunks' of information, where a 'chunk' is approximately a single concept.⁷⁰ Lasswell put it less technically: '[T]he human mind does not operate readily with a long list of terms at the same level of generality.'⁷¹ There are two ways around the constraint of short-term memory. One is to create higher-level chunks. For example, a seven-digit telephone number can be conceived as an exchange – one higher-level chunk comprised of the first three digits – and the remaining four digits. The other is to spend the time required to transfer chunks between short-term and long-term memory, or between short-term memory and external memory aides such as notes on paper.⁷²

Comprehensiveness can be realized through an architecture that specifies each dimension as a separate list of concepts, and provides for elaborating any concept through other lists. Thus in principle, any list may help elaborate a higher-order concept, and any concept may be elaborated by a lower-order list – but the hierarchy is not fixed.⁷³ If the separate lists keep the forest in overview, the elaborations focus attention on the many details of interesting trees. A similar architecture is implemented in menu-drive computer software, in which a choice on one menu calls up choices on additional menus.

Flexibility can be realized through this architecture and through general, abstract concepts. The appropriate concepts are ideal types in Max Weber's sense,⁷⁴ or prototypes representing fuzzy-boundary categories in the vocabulary of cognitive psychology.⁷⁵ Because such concepts allow for qualitative differences among specific instances, they are as useful in characterizing marginal instances as in characterizing those few instances that are prototypes or exemplify the ideal type. The appropriate concepts are *not* operational definitions which, for purposes of measurement, reduce all instances to qualitative equivalence within categories that are mutually exclusive and exhaustive.⁷⁶

C. Policy sciences framework

Lasswell and his collaborators made the first concerted effort to systemize a conceptual framework for the policy sciences as 'a by-product of the Research Project on Wartime Communication which was organized within the framework of the Library of Congress shortly before World War II...'⁷⁷ The im-

petus came in part from the Project's practical responsibilities to recruit and train personnel and to advise on strategy, tactics, and organization in connection with the communication aspects of the war effort.

As a guide to research and analysis, it was necessary to review the then current state of knowledge... Such a review was handicapped by the relatively unsystematic and fragmented nature of the literature in all pertinent fields. ... The research review was designed to give prominence to propositions amenable to further investigation.⁷⁸

The result was first published in 1950 as *Power and Society: A Framework for Political Inquiry*, although the manuscript was finished by the end of 1945.

Since then the framework has evolved in the light of experience.⁷⁹ For example, in another statement of the framework some twenty years later, the exposition was streamlined into a more concise and abstract form.⁸⁰ The previous emphasis on demonstrating continuity with a long tradition of political and social inquiry was dropped. The emphasis was shifted somewhat from propositions to basic concepts. And the problem-orientation – integrating normative, empirical, and practical considerations – was developed in more detail. On the whole, the result is much more convenient to use.

The later statement of the framework meets the formal criteria outlined in the subsection above. Substantively, some evidence for the importance and utility of the basic concepts can be found in the frequency with which parts of the framework are adapted or independently rediscovered by others.⁸¹ Most of the innovation is in new terms for minor variations on old concepts, rather than in new concepts. Moreover, what is adapted or rediscovered tends to be a single list of basic concepts. This leaves the result rather flat, relative to the flexible hierarchy of multiple lists in the policy sciences framework. The hierarchy facilitates the elaboration of richer, more refined distinctions, according to the particular requirements of the problem at hand. More importantly, some forty years of experience have proven the conceptual framework of the policy sciences to be satisfactory for inquiry into problems of theory and practice.

Consequently, questions can be raised about the utility of elaborating, as opposed to using, this or any other adequate framework. Developments in logic earlier in this century made it 'apparent that all comprehensive systems are formally equivalent (hence interchangeable) at corresponding levels of abstraction (and regardless of possible differences in the number of key terms employed at each level)'.⁸² As noted above, the priority task is to relate central theory to observations on specific contexts as events unfold. The meanings of basic, abstract concepts and the propositions of central theory depend on such observations and on what we make of them.

An exposition of the policy sciences framework and its applications is beyond the scope of this paper. But a summary of the principal dimensions of the framework is included as an appendix to this paper. And this inquiry into

the policy movement as a policy problem is itself an application of the conceptual framework and other aspects of central theory in the policy sciences. For example, in order to highlight their origins, the principal substantive themes of this paper can be recast and summarized in terms of principal dimensions of the framework as follows:

In terms of *social process*, the policy movement has been presented as an emerging elite that attempts to explain and justify itself in terms of the improvement of policy decisions through scientific inquiry.⁸³ Neither a fully-conscious identity nor a comprehensive organizational base has yet been consolidated. The distinctive base values of the movement are skill and enlightenment; the distinctive strategy is the manipulation of symbols. It is not yet clear what the net value outcomes will be for various groups within and outside the movement, or whether the long-term effects of the policy movement will be in harmony with broader social interests such as freedom and human dignity.

In terms of *decision process*, the policy movement has been presented as differentiated according to disciplinary origins, interpretations of the movement's common interest, and other factors. Each part of the movement tends to organize itself loosely through a journal and/or a professional organization. Each part tends to invent, promote, prescribe, enforce, appraise and revise or terminate somewhat different standards of research and practice in an open-ended cycle, with various degrees of specificity and internal consensus.⁸⁴ The policy movement crystallized a broader consensus on positivist research standards about two decades ago, but that consensus was incomplete and has been dissipating at an accelerating rate. The movement probably will crystallize the next consensus on post-positivist standards of research and practice involving problem definition, interpretation, and the critique of values.

From a *problem-oriented* perspective, the previous pages have accepted the improvement of policy decisions through scientific inquiry as a worthy goal, and clarified alternatives open to individual participants in the movement for the fuller realization of that goal. There is sufficient diversity among parts of the movement for the individual to have a realistic choice among:

- Alternative understandings of professional roles and responsibilities;
- Alternative standards of research and practice together with their justifications; and
- Alternative forms of knowledge in the problem definition phase of inquiry into particular problems.

There is also sufficient ambiguity to encourage the invention of new alternatives. Choices among the alternatives are complicated by various other goals that are necessarily implicated, including career advancement.

The various parts of the policy movement can accelerate progress toward realization of their common interest by acknowledging their differences and by participating in constructive discussions to clarify, if not reconcile, dif-

ferent standards of research and practice. An important part of acknowledging their differences is keeping the labels straight: 'Policy sciences,' 'policy studies,' and 'policy analysis,' for example, refer to rather different traditions within the movement. Differences are obscured when any of these labels is appropriated by another tradition, or is used to refer to the movement as a whole.

Constructive discussions depend upon the acceptance of certain norms, which help elicit the capacity for reason by moderating the level of tension among those who disagree.⁸⁵ McCloskey's summary of norms is serviceable in this connection:

Don't lie; pay attention; don't sneer; cooperate; don't shout; let other people talk; be open-minded; explain yourself when asked; don't resort to violence or conspiracy in aid of your ideas. We cannot imagine good conversation or good intellectual life deficient in these.⁸⁶

Those parts of the movement that choose to turn inward, withdrawing from participation in broader discussions, may realize a degree of short-term intellectual security at the expense of long-term intellectual vitality.

Contributions to such discussions are properly evaluated in terms of their substantive reliability in the light of the movement's past, and their substantive implications for the clarification and fuller realization of the movement's common interest – and *not* in terms of their pedigrees within the tradition of any part. In other words, the proof must be in the pudding, not the recipes. Otherwise, the recipes become ends in themselves with no common grounds for clarifying individual and collective choices among them.

Differences among the various traditions need not preclude a new consensus on standards of research and practice in the movement as a whole. As Kaplan put it, 'agreement on ethics is not necessary for a moral consensus, just as differences in epistemology do not prevent acceptance of the same body of scientific truths.'⁸⁷

Summary and conclusion

In summary, I have taken this opportunity to review the recent disappointments of the policy movement in the United States as a policy problem, and to do so from a policy sciences perspective.

- What is the common problem? As I define it, most preventable errors of analysis stem from the analyst's perspective: Typically, some important aspect of a problem-relevant context is overlooked or misconstrued. Any definition of the common problem has a bearing on the analyst's choice of professional roles and responsibilities.
- What accounts for the common problem? As I diagnose it, the most basic and pervasive perspectives are epistemological. Positivism tends to per-

petuate errors of analysis, while the other epistemologies provide means of minimizing those errors. This leaves the analyst with a choice among epistemologies and related standards of research and practice.

- What can be done about the common problem? My recommendation is to acquire facility in the use of a conceptual framework, one adequately designed for the definition of any particular problem. An adequate framework also helps the analyst select and integrate those fragments of knowledge that are useful for solving the particular problem.

I have also encouraged others to take the policy movement as a policy problem, from whatever perspectives they find convenient and useful. This self-referential approach – taking ourselves as the objects of and clients for policy inquiry – can be unsettling. Perhaps this is why it is seldom attempted. But the self-referential approach can also be stimulating and rewarding insofar as it generates insights that can help us improve the decisions of others. In the end, I hope, discussion of the various questions and answers will leave each of us in a better position to make better choices.

Notes

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1. Lasswell (1943) is evidently the first statement of the policy sciences. Lasswell and Kaplan (1950), not Lasswell (1951b), is the first major statement for publication. The latter is relatively derivative and popularized. For more on the original conception of the policy sciences, see the subsection on the Policy Sciences Framework in section III below. For a brief historical overview, see Ascher (1987a).
2. For example, some version of the common interest is typically invoked to justify graduate policy programs in university catalogs. Rivlin (1984: 18) observes that 'A university... cannot be regarded as serious these days unless it has a graduate program in public affairs or public policy concerned with improving decision-making at all levels of government, especially the national.'
3. The movement is further differentiated by such analytical specializations as 'risk analysis,' and by policy area specialization such as 'health policy.' Vernon (1985) describes the latter as separate 'glens' of policy analysis.
4. The operative word here is 'a' (not 'the') policy sciences perspective. The definite article is best reserved for the core works of Lasswell and his collaborators, rather than interpretations of them. My numerous notes on and references to the core works of the policy sciences are intended as suggestions for the interested reader, and not as appeals to authority. Suggestions are warranted because Lasswell, in particular, is more often cited than read and understood.
5. The larger sample includes Ridgeway (1956), George (1963), Alexander (1965), Schon (1979), Betts (1978), Etheredge (1985), Norgaard and Dixon (1986), Clark and Westrum (1987), Roland (1987), and Moore (1988), among many others.

6. Lewis (1983).
7. Stern (1986: 201).
8. Gray (1971: 111).
9. Fischhoff, Watson, and Hope (1984: 125).
10. In the larger sample of illustrations, other complicated problems were reduced to quantitative performance measures, a repetitive behavior model, the semi-lattice (a form in set theory), metaphors, organizational reforms, 'hardball' political models, and various paradigms.
11. Lippmann (1965).
12. Simon (1957: 198–199). The statement of the principle is emphasized in the original. On the evolution and implications of the principle, see Simon (1983; 1985).
13. See, for example, Rappaport (1979) in anthropology, Boulding (1961) in economics, and Rorty (1979) in philosophy.
14. Rivlin (1984: 19).
15. Rivlin (1984: 21).
16. Rivlin (1984: 22).
17. Empirical propositions of central theory in the policy sciences presume that behavior is selective according to the viewpoint of the actor in question. Empirical propositions of causal theory presume that behavior is determined by and predictable from impersonal universal laws. The distinction is discussed in section II below. Lasswell (1966: 33–35) notes that 'Since the laws of social relations are about meanings, they are subject to change *with* notice (with insight).' Emphasis in the original. Lasswell (1971b: 55) also notes: 'The discovery that by finding how people refer to the past, present, and future the observer can improve his predictions of their conduct has brought about a further attenuation of the 'billiard-ball' metaphor of yesterday's conceptions of scientific explanation.'
18. Compare Simon (1985: 303), whose overview of human nature in politics 'dissipates the illusion, if anyone holds it, that an application of [theoretical] principles of rationality can discharge us ... from the need to carry on painstaking empirical research at both macro and micro levels.' Leontief (1982) makes a similar point.
19. See Lasswell (1956b), especially pp. 961–965. On the need for integrating theory and observations, see the discussions of index instability, situational reference, and hypotheses-schema in the Introduction to Lasswell and Kaplan (1950) and Kaplan (1963), especially pp. 90–101. On institutions for integrating theory and observations, see the proposal for decision seminars in Lasswell (1971b: 142–157).
20. Compare Lasswell and Kaplan (1950: xxiii), who contend that quests for 'universal laws' in the grand style are not only 'fruitless' but 'serve in the present state of political science chiefly to distract attention and energies from partial inquiries that can illuminate situationally localized problems in empirical ways.'
21. Rivlin (1984: 18–19), for example, observes that the rise of the policy movement has 'dramatically changed the nature of public policy debate ... No debate on any serious issue ... takes place without somebody citing a public policy study.'
22. Our critics are more interested in what the profession is doing for (or to) society than in what the profession is doing for itself. See, for example, Tribe (1972), Kupferberg (1979), Alter (1983), and Lukas (1989). Tribe fails to distinguish among parts of the policy movement: He critiques analysis based on microeconomics, but mislabels such analysis as 'policy sciences.' Lukas, in contrast, distinguishes at least three parts of the movement.
23. Compare Kaplan (1963: 96): 'The expert in a democracy must be only a consultant, not the decision maker.'
24. See Lasswell (1956b: 964).
25. Epistemological assumptions are the deepest and most pervasive perspectives that account for the common problem, and are therefore rather easily addressed in graduate training. Epistemological assumptions obviously are not the only factors involved in the many particular illustrations of the common problem.

26. Hempel and Oppenheim (1969: 60) are quite explicit on this point.
27. Friedman (1953: 4).
28. Friedman (1953: 14–15).
29. Friedman (1953: 43).
30. On the Newtonian ideal, see Thurow (1977: 86), McCloskey (1985: 5), Simon (1985: 301), Stern (1986: 217), and Norgaard (1987).
31. The following discussion of rational choice theory is based largely on Simon (1985: 5–7). See also March (1982), Simon (1983) and Ascher (1987b).
32. For convenient summaries of the record see Ascher (1978, 1981) and McNown (1986). Alfred L. Malabre Jr. and Lindley H. Clark Jr. report growing skepticism about economists' forecasting ability in 'Dismal Record: Changes in Economy Cause Much Confusion Among Economists,' *Wall Street Journal* (March 27, 1989), p. 1f.
33. Ascher (1981: 258).
34. Brewer (1983) also considers the costs and consequences of complex models.
35. For the details see Ascher (1982). Compare Rivlin (1984: 20): 'The poor showing of the forecasters is not due to any lack of effort or ingenuity. The real problem is that the economic system is extremely complicated, that our own economy is battered by forces outside itself which are inherently unpredictable, such as the weather or foreign wars.'
36. Gordon (1969: 157) generalized the point beyond complex models in a chapter on the evolution of events and ideas in the postwar American economy: 'Economic ideas rarely lead economic events but usually follow them.'
37. Cronbach (1975: 119).
38. Compare Campbell (1987: 418–419): '[O]ur experience in generalizing social science findings shows that higher-order interactions abound, precluding unqualified generalization of our principles, not only from laboratory to laboratory, but especially from laboratory to field settings.'
39. Cronbach (1975: 122–123).
40. Siu (1978: 84).
41. See McCloskey (1985) for an extended treatment of the rhetoric of positive economics.
42. See von Wright (1971) and the discussion below.
43. The first major statement of the epistemology is the Introduction to Lasswell and Kaplan (1950).
44. Lasswell (1971: 16). Emphasis added.
45. Compare Simon (1985: 301): The actor's own substantive 'goals and characterizations do not rest on immutable first principles, but are functions of time and place that can only be ascertained by empirical inquiry.'
46. The task of understanding an act is not to be confused with the task of evaluating the act from the observer's standpoint. For example, under the maximization postulate, even an act of suicide or voluntary martyrdom is presumed to have 'made sense' to the actor in question, whether or not the observer subsequently concludes that the act was 'irrational' according to the observer's standards of rationality.
47. Lasswell, Lerner, and Pool (1952), especially the section on Ideology and Counterideology, is an early and explicit example of the use of the maximization postulate in the construction of hypotheses, or more precisely, hypotheses-schema. On the latter distinction, see Lasswell and Kaplan (1950: xxi). The postulate itself is not an hypothesis subject to empirical tests.
48. Compare Lasswell (1966: 76): Variations in predispositions, even among people in the 'same' position, 'provide new contexts for perceiving details in new ways: the outcome lends diversity to the social process.'
49. Lasswell, Lerner, and Pool (1952: 11).
50. See Lasswell (1951b: 8): 'The richness of the context in the study of interpersonal relations is such that it can be expressed only in part in quantitative terms.' See also Lasswell (1966: 41): 'It is sometimes *falsely* assumed that statistical modes of description and correlation

are supposed to eliminate other ways of characterizing current or historic happenings, such as the interpretive essay.' Emphasis added.

51. Gelernter (1989) claims some progress in interpretation by computers.
52. On the importance of good conversation, as opposed to methodological tests, see McCloskey (1985), especially Ch. 2.
53. According to Lasswell (1971b), 'When knowledge is systematic, it goes beyond the aphoristic remarks that are strewn through the 'wisdom' literature of the past. ... To insist on the *empirical* criterion is to specify that general assertions are subject to the discipline of careful observation.' Emphasis in the original. See also Brunner (1982).
54. Kaplan (1963: 92).
55. Compare Kaplan (1963: 97) on scientific theory generally: 'Even the most basic principles of science are not eternal and unqualified truths, but instead the most powerful heuristic instruments known so far.'
56. Lasswell (1971b: 40).
57. Torgerson (1986) develops the differences between these positions.
58. Lasswell (1966: 76). Recall Keynes' (1965: 383) famous remark that 'the ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist.' See also Reich (1988).
59. Lasswell (1951a: 524), in a concluding section titled 'Freedom and the Sciences of Man.' See also Lasswell (1966: 77): 'Insight is a potential base for all value choices; this is the fundamental significance of science for freedom.' Compare Lasswell and Kaplan (1950: xiii): 'Our own values are those of the citizen of a society that aspires toward freedom.'
60. For examples of the results of highly-sophisticated inquiries presented in terms suitable for citizens generally, see Lasswell (1941; 1945; 1950).
61. See Lasswell (1971b: 40-44), which also considers the alternative: 'The task of specifying a human 'indignity' model can be executed by generalizing, for instance, the Nazi program for non-Aryans.' Fischer (1980: 22) is simply mistaken when he characterizes Lasswell as an advocate of the value-free position.
62. Thus the role of the policy scientist in politics is justified not by spurious claims of objectivity or value neutrality, but by service to broader common interests such as freedom and human dignity. Benveniste (1984) sheds some light on this issue.
63. See, for example, Schön (1979; 1983), Dery (1984), deNeufville and Barton (1987), and Dunn (1988).
64. See Rabinow and Sullivan (1979), Vickers (1987) on 'the appreciative system,' and various works on qualitative methodology. March (1982: 39) suggests that 'life is not only choice but interpretation, that they are heavily intertwined, and that the management of life and organizations is probably as much the latter as the former...'
65. See Torgerson (1985) for an overview and citations that link the policy sciences with critical theory and other intellectual traditions. Compare Lasswell (1966: 42-63), which is titled 'A Critique of Values.'
66. See, for example, George (1963), Sui (1978), Stone (1979), and Vickers (1987).
67. See the discussion of 'political prudence' in Lasswell and Kaplan (1950: xxii). The integrative possibilities are nicely illustrated in Reich (1988), which both clarifies and integrates the three premises outlined above. His practical policy insights for a broad audience frequently appear in *The Atlantic Monthly*.
68. On controversy over research standards, see Leontief (1982), Schneider, Stevens and Tornatzky (1982), Brunner (1982), and Lukas (1989).
69. Compare Lasswell (1971b: 84): 'As the maximization postulate implies, we tend to repeat the strategies whose pay-offs - in all value categories - are perceived as indulgent, not deprivational. Basic psychological mechanisms tend to stabilize, even to rigidify, our perceptions of the self in relation to the environment.'

70. See Simon (1981: 81–89).
71. Lasswell (1971a: 18).
72. Perhaps the basic point is that people process streams of chunks in series, within the constraints of short-term memory, rather than in parallel. For a convincing demonstration of this point, see Alexander's (1965, II: 60) discussion of the four sets that may be constructed from four common objects. He concludes that 'you cannot conceive all four sets at once in a single mental act.'
73. For example, Lasswell (1971b: Ch. 3) takes goals – one of five basic concepts in the problem-orientation – and elaborates it in terms of a list of values and another list of basic components in social process.
74. According to Weber (1949: 100), an ideal type is 'a mental construct for the scrutiny and systematic characterization of individual concrete patterns which are significant in their uniqueness...' See also Kaplan (1963: 101).
75. The literature is summarized and cited in Brunner (1986: 207–212).
76. According to Friedman (1953: 11) positive theory requires an 'analytical filing system' of mutually exclusive and exhaustive categories.
77. Lasswell and Kaplan (1950: v).
78. Lasswell and Kaplan (1950: v).
79. This was anticipated by Lasswell and Kaplan (1950: vi): '[T]he task of developing a framework for inquiry contemporaneous with the state of research in the field is an unending one.'
80. Lasswell (1971b), especially Chapters 2, 3, and 4, is still the best short statement of the framework and its use.
81. Brunner (1987) reviews a number of examples.
82. Lasswell (1956b: 965).
83. Technically, the most basic common interests within and outside the movement are myths, insofar as they are used to justify and explain the possession and use of any value, including power, skill, and enlightenment. A myth is not necessarily false. See Lasswell, Lerner, and Pool (1952) and Lasswell and Kaplan (1950: Ch. VI) on the functions and dynamics of myth. On scientists in social process, see Lasswell (1966), especially the section on the Unnamed Revolution, and Lasswell (1970b).
84. A rich body of literature on the decision process has developed since Lasswell (1956a) summarized the older work, and may be used to investigate the decision process within each part of the movement.
85. The point is that discussion is *not* constructive under all circumstances. See Lasswell (1977) on 'the politics of prevention.'
86. McCloskey (1985: 24).
87. Kaplan (1963: 90).

Appendix

Principal dimensions of the policy sciences framework

Adapted from Lasswell (1971b), Chs. 2 and 3

<i>Postulate</i>	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.
<i>Values</i>	<i>Outcomes and institutions</i>
Power	Victory or defeat in fights or elections. Government, law, political parties.
Enlightenment	Scientific discovery, news. Languages, mass media, scientific establishments.
Wealth	Income, ownership transfer. Farms, factories, banks.
Well-being	Medical care, protection. Hospitals, recreational facilities.
Skill	Instruction, demonstration of proficiency. Vocational, professional, art schools.
Affection	Expression of intimacy, friendship, loyalty. Families, friendship circles.
Respect	Honor, discriminatory exclusion. Social classes and castes.
Rectitude	Acceptance in religions or ethical association. Ethical and religious associations.
<i>Social process</i>	<i>Components</i>
Participants	Individuals, groups, value shapers (official, non-official), value sharers (official, nonofficial).
Perspectives	Value demands, expectations, identities, myths (doctrines, formulas, mirandas).
Situations	Unorganized (territorial, pluralistic), organized (territorial, pluralistic), value inclusive or exclusive, crisis or intercrisis.
Base values	Positive assets (perspectives, capabilities), negative assets (perspectives, capabilities) by value category.
Strategies	Coercive, persuasive, communicative (diplomacy, propaganda), collaborative (military, economic).
Outcomes	Value (indulgences, deprivations), decisions, choices (by phases of decision process).
Effects	Value (accumulation, enjoyment, distribution), institutions (structure, function, innovation, diffusion, restriction).
<i>Decision process</i>	<i>Outcomes</i>
Intelligence	Gathering, processing, dissemination of information.
Promotion	Adding intensity to the dissemination of value demands.
Prescription	Stabilizing expectations on norms to be severely sanctioned if challenged in various contingencies.
Invocation	Initial characterization of a concrete situation in terms of conformity of non-conformity to prescription.
Application	Final characterization of a concrete situation in terms of conformity or nonconformity to prescription.
Termination	Canceling a prescription and dealing with claims of those who acted in good faith under it.

Appraisal	Characterizing the aggregate flow of decision according to policy objectives, and identifying both formal and effective responsibility for successes or failures.
<i>Problem orientation</i>	<i>Questions and tasks</i>
Goals	What future states are to be realized as far as possible in social process? Goals clarification.
Trends	To what extent have past and recent events approximated the preferred terminal states? Trend description.
Conditions	What factors have conditioned the direction and magnitude of the trends described? Analysis of conditions.
Projections	If current policies are continued or modified, what would be the probable future of goal realizations or discrepancies? Projection of developments.
Alternatives	What intermediate objectives and strategies will optimize the realization of preferred goals? Invention, evaluation, and selection of alternatives.

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