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# A paradigm for practice

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**Abstract.** Lack of consensus on a paradigm for practice inhibits the cumulative development of practical knowledge and skills. It also encourages devolution of these and other paradigm functions by default to the established paradigm in the policy movement, reductionism, which includes positive methodology. But reductionism is insufficient for practice. It tends to displace practical aims and expectations, and discount and delegitimize practical and other ways of knowing. This gives rise to problems in practice, illustrated here by problems in psychotherapy, global climate change, and various 'high-modern' schemes to improve the human condition. To help alleviate such problems eventually, this article outlines a contextual paradigm for practice based on examples of good practice, the policy sciences, and related sources. The immediate purpose is to stimulate policy scientists, other practitioners, and other contextualists to make explicit and compare their paradigms.

## Introduction

In law, medicine, and other professions, 'practice' typically means action to help solve the problems or realize the opportunities of diverse clients, patients, and others through the application of appropriate knowledge and skills in particular contexts. Participants in public policy decision processes 'practice' in this sense. Like other practitioners, they can be considered professional only to the extent they take responsibility for applying their distinctive knowledge and skills on behalf of common interests, not special interests (Lasswell and McDougal, 1992:xxii). The formal education of policy professionals, including their scientific advisers, was an important motivation in the establishment of the first public policy schools in the U.S. around 1970. The justifying premises were that no important policy problem falls entirely within the boundaries of any one discipline, and that the various disciplines in any case were preoccupied with their own problems of theory and method. Therefore new policy programs were needed to integrate and apply knowledge toward the solution of public policy problems. Establishment of these programs in the policy movement coincided with a peak of optimism about progress in the social sciences. Increasingly during the 1960s, they came to be considered sciences mature enough to provide objective foundations for improving public policy, raising policy above politics as envisioned in Progressive-era aspirations for scientific management (Lippmann, 1922; Brunner, 1991; Brunner et al., 2005).

Despite progress in the decades since then, paradigms for practice remain largely implicit and uncoordinated in the policy movement. So do paradigms for practice in other professions, from this outsider's perspective. This state of affairs is problematic in several respects. First, it inhibits improvements in practice. A paradigm consensus is the basis for guiding the cumulative development of appropriate knowledge and skills, communicating them effectively, and justifying and challenging particular claims of

knowledge. Second, in the absence of a paradigm for practice, these functions often are devolved by default to the established paradigm in the policy movement, reductionism, which includes positive methodology. It aims to reduce diverse observations to general relationships that predict new observations with precision and accuracy. But reductionism is not sufficient to guide practice; among other things, it diverts attention from factors important for action in specific contexts. Moreover, the default tends to displace practical aims and expectations, and discount and delegitimize practical and other ways of knowing. This is 'the objectivity crisis' according to the late chairman of the science committee in the U.S. House of Representatives, George E. Brown Jr. (1992bc:779–780). He observed that 'we have, unconsciously and ironically, imbued science with more value than other types of understanding which are overtly and explicitly value-based.' Third, the absence of a paradigm for practice leaves open a niche for a rising 'faith-based' community. A spokesman for that community claims that 'when we act, we create our own reality,' and dismisses 'the reality-based community' as people who 'believe that solutions emerge from judicious study of discernible reality' (quoted in Suskind, 2004). From a policy sciences perspective, neither the judicious empirical inquiry of pure science nor the faith-based action of pure politics alone is likely to advance the common interest in particular contexts or the fuller realization of human dignity for all.[1]

This article sketches a paradigm for practice that eventually might help alleviate such problems. The present purpose is to stimulate policy scientists and other practitioners to make explicit and compare the paradigms underlying their own work, as necessary steps toward crystallizing a paradigm consensus. Early in the 1990s, the author was prompted to do this by a conversation with S. Alan Stern, then a colleague in a space policy research project who was engaged in concurrent research at the boundary between planetary and astrophysics. Asked how he knew when he had discovered a planet orbiting a distant star, Stern replied without hesitation, 'it's the convergence of multiple independent streams of information' on the particular context. It was immediately apparent that this methodological principle characterizes the work of policy scientists and other practitioners much better than reductionism, the established alternative emphasizing the abstraction of general relationships from many contexts. Stern's reply suggests that a paradigm for practice might be informed by the experience of scientists, historians, and other scholars who address questions about any particular context, including a distant solar system. These scholars in turn might find a paradigm for practice useful in their contextual work. A paradigm for practice intended primarily for policy scientists and other practitioners cannot accommodate all the needs of people who inquire into particular contexts for scientific, practical, or other purposes, but it might help them clarify commonalities in contextual inquiry of any kind.

Development of a contextual paradigm for practice should be informed by actual practice from the outset.[2] Thus the first section of this article examines exemplars of practitioners and other contextualists at work in different fields to identify commonalities in their basic aims, expectations, and methodologies. The second section draws on these commonalities and other sources including the policy sciences to crystallize an alternative contextual paradigm and to compare it with reductionism. Differences in the basic aims, expectations, and methodology of the two paradigms can

be complementary in some circumstances, but minimizing the differences perpetuates paradigm problems in practice. These problems are illustrated and their persistence diagnosed in the third section. The conclusion summarizes the task ahead as the liberation of practice not from science but from the exclusive claims of reductionism.

## 1. Practitioners at work

This section examines three classics on practice in military intelligence, psychotherapy, and investigative journalism to clarify commonalities that provide empirical grounds for crystallizing a suitable paradigm. The commonalities do not conform to the established paradigm. Examination of other contextual work can clarify convergent and additional commonalities.

### *Three classics*

After World War II, Alexander George reconstructed an 'indirect method' that intelligence analysts developed and used with some success in support of the war effort. In an outstanding example, 'British content analysts were able to infer that Nazi propaganda talk about the forthcoming use of a secret, unconventional air-bombardment weapon was no bluff. This inference was made in November 1943, eight months before the inception of the V-1 "buzz-bomb" attacks' (George, 1956:341; see also George, 1959:140–150). The obvious objective of the propaganda, which began as early as June 1943, was to boost German domestic morale badly shaken by heavy Allied air raids. The inference was also based on generalized precedents: '*German propaganda never deliberately misled the German people in matters involving an increase of German power*' (George, 1956:341; his emphasis). But another generalized precedent complicated the picture: the new weapon did not appear within the maximum period the chief propagandist normally allowed for preparatory propaganda, about three months. (Joseph Goebbels avoided longer delays that risked resentment over false promises.) Nevertheless, 'the analyst concluded – correctly – that scheduled employment of the new weapon must have been unexpectedly delayed. He even identified the approximate dates on which something must have happened to cause the delays' (George, 1956:341). For ten days beginning August 19 and seven days beginning September 11, references to the new aerial reprisal weapon suddenly dropped out of Nazi propaganda. These dates coincided with a British air raid August 17 on a suspected secret weapon development site at Peenemünde, and Allied air raids September 7–8 on suspected launching platforms in northern France. The analyst 'found that the "gaps" did not coincide with "other events" and, significantly, that the propaganda commitment on reprisal and new weapons was watered down when propaganda was resumed after each "gap"' (George, 1956:342). This helped confirm other sources of intelligence on the significance of Peenemünde and the launching platforms, and the effectiveness of air raids on these targets.

In a second classic, Erik Erikson reconstructed the method of 'disciplined inferences' in psychotherapy, drawing upon his success with a young man suffering in waking hours from a recurring anxiety dream so severe that he feared loss of his

sanity. In the dream, '*There was a big face sitting in a buggy of the horse-and-buggy days. The face was completely empty, and there was horrible, slimy, snaky hair all around it. I am not sure it wasn't my mother*' (Erikson, 1959:79; his emphasis). This report, including its omissions, was considered in the context of the patient's history, a physical examination, and standardized psychological test results. But Erikson relied primarily on 'the patient's capacity to produce during a series of therapeutic encounters a sequence of themes, thoughts, and affects which seek their own concordance and provide their own cross-references' (Erikson, 1959:86). He found a concordance among cross-references from four periods in this patient's life. These involved his mother in early childhood, a grandfather associated with horse-and-buggy days in the patient's childhood, his recent failure to find inner peace in a theological seminary, and his therapy with Erikson. 'Such redundancy points to a central theme, which, once found, gives added meaning to all the associated material. The theme is: "Whenever I begin to have faith in somebody's strength and love, some angry and sickly emotions pervade the relationship, and I end up mistrusting, empty, and a victim of anger and despair"' (Erikson, 1959:88). Was this interpretation correct? According to Erikson (1959:93), 'The simplest answer is that this particular patient was delighted when I told him of [it] and of my anger over his unnecessary attempts to burden me with a future he could well learn to manage.... The patient left the hour – which he had begun with a sense of dire disaster – with a broad smile and obvious encouragement.'

In a third classic, I. F. Stone used a method honed in decades of work as an investigative journalist in Washington, D.C. to solve an ancient puzzle: After a trial in 399 B.C., Athens put Socrates to death for the exercise of free speech, despite Athenians' passionate commitment to free speech. Socrates had been accused of violating the law 'by corrupting the youth,' but neither the text of the law nor the specific charges survived. Socrates' disciples, Plato and Xenophon, wrote the only surviving contemporary accounts, but both were 'anxious to put their beloved master in as good a light as possible' (Stone, 1979:26). Stone examined these 'scanty and one-sided' sources and other surviving information on the trial in the context of what is generally known about ancient Athens. A series of observations and inferences (detailed below) converged on a startling conclusion: Socrates taught antidemocratic politics to Critias and others in the dictatorship of the Thirty Tyrants, both before their brief and brutal rule in 404 B.C. and after the amnesty that followed. 'Athens felt Socrates was still inculcating disrespect for its democratic institutions, and feared an attempt to overthrow the democracy again' (Stone, 1979:68). In Stone's judgment, the condemnation of Socrates was not justified. 'The 501-man jury itself was deeply troubled and reached its verdict of guilty only by a narrow margin. But these fresh insights give us a glimpse of the political realities and extenuating circumstances which Plato...has so successfully obscured for 2,500 years' (Stone, 1979:68; see also Stone, 1989).

### *Commonalities*

Despite striking differences there are significant commonalities in the aims, methods, and expectations underlying these three examples of good practice, or 'exemplars.' The first is that practitioners sought solutions to practical problems or puzzles

in particular contexts, not generalizations across contexts. Existing generalizations sometimes were applied heuristically as means to help interpret information on the context. Consider some details.

The apparent aim of the content analysts' work was to help the Allies assess and target air raids, an important contribution to the war effort according to General Eisenhower (George, 1959). The immediate problem was to make sense of observed gaps in Nazi propaganda about a new weapon and apparent inconsistencies in light of generalized precedents from Nazi propaganda. George (1956:336–337) found that 'the content analyst makes use of existing generalizations about the types of situations and circumstances in which the elite tends to employ preparatory propaganda.' For Erikson (1959:86), the primary aim was 'recognition and removal of the patient's inner and often unconscious resistances to his own wish to see clearly and to get well.' In pursuit of an interpretation that might resolve the patient's problem, he thought clinically about information on the patient – 'that is, he scans in his mind different models in which different modes of knowledge have found condensation' (Erikson, 1959:74). These include anatomical, physiological, and pathological models. I. F. Stone's work is a partial exception. In this interview with himself, Stone provided a practical justification for his obsession with the ancient puzzle of Socrates' trial: '*Why should this fascinate an old Washington muckraker like you?* Because it's a black eye for all I believe in, for democracy and free speech.... All our basic problems are there in miniature' (Stone, 1979:22–23; his emphasis). But Stone did not aim to solve contemporary problems in the practice of democracy. He sought to solve an historical problem, perhaps because ill health had forced him to give up *I. F. Stone's Weekly*. Substantive generalizations were neither obvious nor necessary for Stone's solution to the ancient puzzle.

The second commonality is that practitioners used context-sensitive methods: The significance of each detailed observation (including omissions) depended on the context, and understanding of the context as a whole depended on the convergence of inferences from detailed and comprehensive observations. Linkages among parts and the whole were constructed through inquiry, not taken as fixed or given or assumed in advance. Again, consider some relevant details.

In George's reconstruction of the indirect method of the content analysts, a 'logic of the situation' assessment from the standpoint of the enemy elite played an important role. The analyst 'keeps in mind that the elite is guided not merely by its own past practice or by a rigid operational doctrine. Rather, the elite is presumed to make an assessment of the factors upon which a contemplated action is likely to depend, and from this assessment it derives some estimate of the requirements, if any, for preparatory propaganda support. Finally, it is assumed the choice of a preparatory propaganda strategy is also influenced by considerations of the feasibility and the probable effectiveness of the available strategies and techniques' (George, 1956:337).[3] Within this 'logic of the situation' assessment, George (1956:338) likened reasoning in the indirect method to 'an effort to reconstruct the missing pieces in an action sequence. Certain parts of the mosaic of action are given or readily assumed. Other pieces are missing. In effect, therefore, the analyst rehearses in his mind the different possible versions of a missing piece, trying to decide which version is more plausible, given the values of the pieces already known to him.'

Erikson's method of disciplined inferences begins with consideration of the patient's visible or otherwise localizable symptoms in context. "Any good doctor knows ... that the patient's complaint is more extensive than his symptom, and the state of sickness more comprehensive than the localized pain or dysfunction' (Erikson, 1959:74–75). The synthesizing trend in the clinical material provided by the patient 'permits the clinician to observe with free-floating attention, to refrain from undue interference, and to expect sooner or later a confluence of the patient's search for curative clarification and his own endeavor to recognize meaning and relevance' (Erikson 1959:86). The clinician 'makes inferences based on a rapid and mostly pre-conscious cross-checking against each other of anatomical, physiological, and pathological models' (Erikson 1959:74). Thus the clinician reconstructs the significance of the information, but 'has no right to test his reconstructions until his trial formulations have combined to a comprehensive interpretation which feels right, and which promises, when appropriately verbalized, to feel right to the patient' (Erikson, 1959:89). If the intervention based on a comprehensive interpretation is correct, the patient emerges from the encounter more whole and less fragmented.

Stone's method depended on construction of the context of Socrates' trial. He reported that 'I've been happily bogged down in ancient Athens for several years, trying to explore all of Greek thought and civilization, in order to reach a better understanding of the trial' (Stone, 1979:23). But '*How can a newspaperman find something new to report about a trial that took place so long ago?* You re-examine all the source material for yourself. You go back to the texts in the original language, so that you can evaluate every nuance. You search out the internal contradictions and curious evasions. It's not so different from digging the real truth out of a Pentagon or State Department document' (Stone, 1979:26; his emphasis). The result was a series of practical inferences that converged on his overall conclusion. For example:

- He observed that Xenophon's defense omitted lines from Homer that Socrates had taught, lines approving the divine right of kings and the use of violence to suppress free speech. Why? 'They were too damaging a part of the prosecution's case' (Stone, 1979:68).
- He observed that Plato chose to be uninformative in the Euthyphro dialogue. Why? 'A lawyer might surmise that he blocked out as much as he could of the specific charges because they were too damaging and too hard to disprove' (Stone, 1979:26).
- He observed that Aeschines cited the Socrates case as a praiseworthy precedent in another trial 54 years later, and won the trial. Why? 'Aeschines could not have swayed the jury by that reference unless he was saying something about the relations between Socrates and Critias that was generally accepted as true by the Athenian opinion of that time' (Stone, 1979:34).

Stone (1979:34) noted that 'This last bit is well known to scholars but its significance has never been fully appreciated.' He constructed its significance by linking it to inferences from other observations, including deeds: '*Socrates remained in the city all through the dictatorship of the Thirty Tyrants.*' Stone (1979:34) emphasized this 'Because that single fact must have accounted more than any other for prejudice against Socrates when the democracy was restored.' Stone also filled in information

on the social context withheld by Socrates' apologists. 'The dictatorship of the Thirty Tyrants was the dictatorship of the wealthy landed aristocracy to which Plato and Critias belonged. This was the social circle from which most of Socrates' followers were drawn. Athens understood this, though the modern reader often doesn't' (Stone, 1979:37).

Notice that all three methods employed an underlying assumption to interpret words and deeds: people act from their own subjective perspectives, choosing the course of action expected to leave the identified self better off.[4] For example, Plato and Xenophon identified themselves as disciples of their master Socrates, and chose to evade or omit damaging material to protect his reputation. Similarly, Erikson begins with the contract between the psychotherapist and the patient (Erikson, 1959:75–76), an act of choice from which each expects to benefit. In fulfilling the contract 'there is a core of *disciplined subjectivity* ... which it is neither desirable nor possible altogether to replace with seemingly more objective methods' (Erikson, 1959:76; his emphasis). The subjectivities include 'the clinician's "mixed feelings," emotions and opinions.' These must be used as part of the evidence 'instead of putting them aside with a spurious claim to unassailable objectivity' (Erikson, 1959:93). Similarly, the content analysts observed a line of Nazi propaganda and then reasoned as follows: 'The opposing propagandist would not adopt such propaganda goals for implementation through these media channels at this time and in this situation unless an action of the type being inferred were indeed planned or intended' (George, 1959:337). This indirect method 'recognizes, as an earlier approach did not, that propaganda can be used to further a forthcoming initiative in a variety of ways. Therefore it does not expect to find a regularly recurring relationship between a type of elite intention and a type of propaganda strategy accompanying it' (George, 1959:335). The earlier approach was consistent with reductionism. In it, the analysts mistakenly assumed that 'the possibility of predicting an opponent's initiatives [required] discovery of a regular pattern, or relationship, in the opponent's past behavior, which would serve as a rule of inference in new instances' (George, 1956:334).

The third commonality is that none of these practitioners expected completeness or closure in his understanding of the context as a whole, or of the many observations and inferences integrated into that understanding. Instead, each understood that his conclusions depended on unique contexts open to change, including change as a result of action in the context by practitioners themselves.

George (1959:145; his emphasis) reports that 'the British analyst's tentative estimate, *made in early November, 1943*, that *at this time* German leaders expected to have the new offensive weapon ready for use sometime between mid-January and mid-April, 1944, proved to be amazingly accurate' based on post-war examination of *Goebbel's Diary.* But air raids on the V-weapons' launching sites in December 1943 were effective enough to delay the first use of the V-1 for six months until June 15. (Significantly, that came days after the Allies had established positions in Normandy.) Erikson (1959:84) recognized that 'Clinical work is always research in progress.' But he warned against substitution of research goals for therapy: 'A research-minded clinician...must always take care lest his patients become footnotes to his favorite thesis or topic.' Erikson (1959:78) also recognized that the psychotherapist influences the history of the patient as he examines it. He 'recognizes his activities as a function

of historical processes, and is forced to conclude that in some sense he is "making history" as he "takes" it' from the patient. Stone again is a partial exception. He could not intervene in events in ancient Athens of course, but he did cite the Swiss historian Jakob Burckhardt in partial justification of his inquiry: 'Burckhardt once said that, in a hundred years, someone would reread Thucydides and find something in his history that "we had all overlooked"' (Stone, 1979:26). Applying his methods to ancient sources, Stone found insights into the trial of Socrates that classicists had overlooked for 2500 years.

*Other examples*

Other examples from practitioners or those who study practice might eventually clarify additional commonalities to improve the empirical grounding of a paradigm for practice. For now, consider a few examples demonstrating partial convergence on the commonalities already gleaned from the three classics, even if convergence is sometimes disguised by differences in vocabulary.

- In management, according to Ralph G. H. Siu (1978:84, 83), 'everything is continually changing – not only the events themselves, but also the very rules governing those events.... In the ball game of competitive actualities ... all systems are open.' The big decisions are less scientific than artistic, involving 'the subjective, the ineffable, the holistic synthetic....'
- Similarly, 'In the hurly-burly of management, planning, and analysis, professionals understand that events are viewed differently by different people, that the context of their actions is usually more important than general laws of behavior, and that much of what they do is make sense of actions and context so that their behavior is appropriate and helpful' (Brown et al., 1999:603).
- CEO Donna Dubinsky (quoted in Goldstein, 2005:39) views business as 'a giant jigsaw puzzle' with many pieces. 'If all those pieces fit together you do well in business. If you focus on just some of the pieces you won't succeed. History is a lot like that.... You learn [from the study of history] to understand how critical context and complex systems are.'
- In design, Donald Schön (1983:49) urged professionals to join in the search for 'an epistemology of practice implicit in the artistic, intuitive processes which some practitioners do bring to situations of uncertainty, instability, uniqueness, and value conflict.' Evidently, the available epistemologies were not sufficient for such situations and practical purposes.

Additional clarification and corroboration of commonalities can be found in contextual inquiry in various disciplines, even if those disciplines are not concerned primarily with practical problems. Consider history and geology, for example, in addition to Stern's work at the boundary between planetary and astrophysics.

In answer to his own question, 'What Are Historians Trying to Do?' Henri Pirenne distinguished the aims of the historian. Unlike the sociologist who seeks laws that regulate social existence, 'the historian devotes himself to acquiring concrete knowledge of [social] existence...' (Pirenne, 1931:435). Pursuing his goal, 'the historian finds

himself confronted with a double task. He must first of all establish the facts which constitute the materials of his study, then make use of them.... Historical facts are perceptible only by the vestiges which they have left. In this respect, the position of the historian vis-à-vis his subject is quite comparable to that of the geologist' (Pirenne, 1931:437). Then he must judge the authenticity and credibility of each source, and compare different historical accounts 'to deduce some true semblance of the reality which otherwise disappears as each gives his own account of it' (Pirenne, 1931:440). 'All historical narrative is at once a synthesis and a hypothesis. It is a synthesis insomuch as it combines the mass of known facts in an account of the whole; it is a hypothesis insomuch as the relations that it establishes between these facts are neither evident nor verifiable by themselves' (Pirenne, 1931:441). In constructing a narrative 'The historian assumes ... that he can treat the actions of the dead as he does those of the living who surround him. And this comparison suffices to make comprehensible the subjective element in his accounts' (Pirenne, 1931:442).[5] There is no pretense of closure here: 'No account, detailed as it may be, ever exhausts its subject. The fullness of reality can never be expressed either by speech or by pen' (Pirenne, 1931:440). 'To achieve certainty about a subject as flowing, diverse, and complex as social behavior is impossible' (Pirenne, 1931:443).

In support of his claim that geology is an interpretative and historical science, Robert Frodeman (1995:963) focused on the problem of a geologist attempting to understand a particular rock outcrop.[6] 'The geologist assigns different values to various aspects of the outcrop, judging which characteristics or patterns in the rock are significant and which are not. Examining an outcrop is not simply a matter of "taking a good look." Rather, the geologist picks up on the clues of past events and processes in a way analogous to how the physician interprets the signs of illness or the detective builds a circumstantial case against a defendant.' The reasoning is circular, moving from parts to wholes and back on all levels: 'when we strive to comprehend something, the meaning of its parts is understood from its relationships to the whole, while our conception of the whole is constructed from an understanding of its parts' (Frodeman, 1995:963). Contradictions drive progress: a 'provisional interpretation is called into question when we are "pulled up short" by details ... that do not jibe with our overall conception. This forces us to revise our interpretation of the whole as well as our interpretation of the particulars' (Frodeman, 1995:963). Historical precedents and processes are relevant to understanding a singular event, like an outcrop. But geologic reasoning is defined by a 'sense of the overall coherence of a theory, rather than a simple correspondence between present and the past' (Frodeman, 1995:965). Finally, geology depends on the coherence of an overall historical narrative for support of knowledge claims: 'Historical narratives do not explain an event by subsuming it under a generalization, but rather by integrating it into an organized whole. Thus an outcrop does not "make sense" until it contributes to and is a component of an overall story' (Frodeman, 1995:966).

## 2. Paradigms compared

Commonalities in these and other sources can be crystallized into a contextual paradigm for practice, and compared with reductionism, the established paradigm

in the policy movement. Differences between the requirements of practice (as reflected in a paradigm for practice) and reductionism give rise to problems in practice that are taken up in the following section. But first a brief introduction to the concept of 'paradigm' is in order.

A paradigm is a myth specialized to explaining and justifying scientific practices and claims of scientific knowledge, just as an ideology is a myth specialized to explaining and justifying political practices and demands. The functions of explaining and justifying are served so long as the basic assumptions of the myth are accepted as true with such confidence that they hardly appear to be assumptions at all. Questions of their literal truth signal a fragmenting consensus and perhaps an impending revolution.[7] Founders of the policy sciences conceived and compared myths in three functional parts (Lasswell and Kaplan, 1950: Sections 6.3–6.4; Lasswell, Lerner, and Pool 1952, ch. 1). The *doctrine* consists of the basic aims and expectations compelling the intellect to accept certain practices and claims. The *formula* prescribes basic methods for progress in accord with the 'philosophy' set forth in the doctrine. The symbols of sentiment and identification are the *miranda*, or things to be admired and emulated. (They are distinguished from the credenda, or things to be believed, in the doctrine and formula.) For example, to justify their political demands, Americans across generations have invoked key symbols from the Declaration of Independence and the Preamble to the Constitution, which are important expressions of doctrine. They have also invoked the formula prescribed in the articles of the Constitution and authoritative interpretations of them. Washington, Lincoln, Old Glory, the national anthem, and events associated with them are among the key symbols of miranda.[8]

### *A contextual alternative*

A paradigm for practice must be contextual rather than reductionist to cope satisfactorily with the unique problems that practitioners confront in unique contexts. The policy sciences are an obvious source for crystallizing doctrine, formula, and miranda consistent with commonalities in practice. From the outset the policy sciences emphasized the unity of theory and practice while distinguishing their basic aims (Lasswell and Kaplan, 1950:xii). The policy sciences also are an 'adaptation of the general approach to public policy that was recommended by John Dewey and colleagues in the development of American pragmatism' (Lasswell, 1971:xiii–xiv).

The doctrine can be modeled on therapy, including but not limited to one-on-one situations. According to Lasswell (1951a:524; his emphasis), 'the role of scientific work in human relations is *freedom* rather than prediction.' This means bringing into conscious awareness internal compulsions and non-personal factors in the environment that once determined choices, freeing people to take them into account in making future choices. 'Hence 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' (Lasswell, 1951a:524). But insight exposes and perhaps alters or destroys behavioral relationships that have held in the past. Hence 'all scientific propositions about character or society must always be read "subject to insight"' (Lasswell, 1951a:524). The validity of a scientific proposition about behavior is contingent on

the context; so are indices of the variables in such a proposition.[9] Hence the 'quest for "universal laws" in the grand style' is futile, serving mostly 'to distract attention and energies from partial inquiries that can illuminate situationally localized problems in empirical ways.' If scientific propositions are not universal laws, they serve as heuristics; that is, they 'serve the functions of directing the search for significant data, not of predicting what the data will be found to disclose' (Lasswell and Kaplan, 1950:xxiii). The meaning of a scientific proposition depends on its consequences.[10] Where action is concerned, 'The analysis of meanings must ... focus on the particular contexts in which the action is performed, and on the purposes which the action as a whole is meant [sic] to achieve' (Kaplan, 1964a:46; his insertion). Thus meanings are constructed in context for scientific and policy purposes.

What can we know? Pragmatism has a suitable answer to this first question of epistemology. As Kaplan (1964a:43) summarized it, 'Knowing is not one thing that we do among others, but a quality of any of our doings.... To say that we know is to say that we do rightly by our purposes, and that our purposes are thereby fulfilled is not merely the sign of knowledge, but its very substance.' Knowing can be disciplined in many ways to arrive at a closer approximation to the truth. But knowing is subjective in that it is guided by one's purposes and based on one's perspective on the context, a perspective that is far from omniscient. There exists a fundamental difference between 'the world outside and the pictures in our heads,' to borrow a phrase from Walter Lippmann. Consequently, 'The way in which the world is imagined determines at any particular moment what men will do. It does not determine what they will achieve' (Lippmann, 1922:25–26).[11] Freedom through insight can bring outcomes more in line with preferences, but it cannot eliminate the fundamental difference. As Lasswell (1971:40) 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.' Moreover, the social scientist is not independent of the context. He 'may affect the phenomena he observes to a much more significant degree than is true of the physical scientist. The observer is part of society, and the reporting of his results part of the social process. Members of society may deliberately modify their responses in taking account of being observed or of the observations reported' (Lasswell and Kaplan, 1950:xxi). Modifying responses is the point of practical policy inquiry.

How can we know what we can know? A contextual answer to this second question of epistemology emphasizes the convergence of multiple independent streams of information on the same context. Underlying the linkages in George's mosaic of action, in Erikson's interpretation, and in Stone's solution to an ancient puzzle is the equivalent of the principle of contextuality in the policy sciences: 'If modern historical and social scientific inquiry has underlined any lesson, it is that the *significance of any detail depends upon its linkages to the context of which it is a part*. Hence the evaluation of the role of any institutional practice calls for a vast labor of data gathering and theoretical analysis' (Lasswell, Lerner, and Pool, 1952:11; their emphasis). As illustrated in each of the three classics, the context and linkages among its details are constructed around the initial problem during the course of inquiry, not fixed or given or assumed in advance. While focused on the problem, the theory constructed 'also generates predictions or explanations on dozens of other aspects' of the context

of the problem. These may be disconfirmed or confirmed with comprehensive and detailed observations from that context. The inquirer 'does not retain the theory unless most of these are also confirmed. In some sense, he has tested the theory with degrees of freedom coming from the multiple implications of any one theory' of a single case (Campbell, 1975:181–182). What survives testing is a construction of the linkages between parts and the whole most consistent with comprehensive and detailed information on the context. This is where multiple independent streams of information have converged – on a theory for $n = 1$. The theory exploits relationships among the manifold of events constituting the single case.

Practical reasoning includes deduction and induction but depends critically on abduction, which proceeds from unexpected or otherwise problematic observations to infer a hypothesis that might explain them. According to the seminal figure in the logic of abduction, the pragmatist C. S. Peirce (quoted in Burks, 1946:303), 'Abduction is the process of forming an explanatory hypothesis. It is the only logical operation which introduces any new idea; for induction does nothing but determine a value [for hypothesis testing], and deduction merely evolves the necessary consequences of a pure hypothesis.'[12] For example, Stone's (1979) practical reasoning about the trial of Socrates proceeded from the puzzle to a series of particular observations, such as omissions in Xenophon's defense, to infer working hypotheses about Socrates and his teachings. Other consequences could be inferred from these hypotheses individually or taken together (deduction). And the convergence of inferences from multiple observations supported the overall conclusion that Athens put Socrates to death because he was once again undermining Athens' democratic institutions (induction). Similar practical reasoning underlies conclusions in the other two classics, Erikson's (1959) interpretation and intervention, and the content analysts' inferences about the existence of a new aerial reprisal weapon and delays in its development. In these cases and in others considered by Michael Polanyi (1966:19), 'the detailing of particulars, which by itself would destroy meaning, serves as a guide to their subsequent integration and thus establishes a more secure and accurate meaning of them.'

The methodology is 'context-sensitive' in a definite sense: the context and therefore the manifest meaning of each observation is allowed to vary as data. Among the context-sensitive methods familiar to policy scientists are prototyping, a technique intermediate between experimentation and intervention, in which scientists explore the consequences of modifying institutional practices on a small scale; and decision seminars, a technique to support contextual mapping and problem-oriented inquiry by groups (Lasswell, 1971). In addition, because the inquirer both affects the social context and is affected by it, context-sensitive methodology includes developmental constructs for self-orientation, methods of recording the focus of attention for self-observation, and free association for self-knowledge (see respectively, Lasswell, 1941, 1971, 1970a). The results of positive methods often are integrated into contextual inquiry as heuristic propositions or quantitative data, but they cannot illuminate everything of practical importance in the context. Thus 'while use is made of careful observation, measurement and record taking, quantification is relegated to a relatively secondary position. The richness of the context in the study of interpersonal relations is such that it can be expressed only in part in quantitative terms. Convincing results

can be obtained by studies which are but partially summarized in numbers' (Lasswell, 1951b:8). On the other hand, "it would be a mistake to imply that contextual theory is independent of "quantification," of statistical investigation' (Lasswell, 1959:643). Numbers, like words and deeds, can be enlightening or misleading, perhaps deliberately so.[13] The task always is to clarify what they mean in context.

Some quantitative methods illustrate more precisely the meaning of 'context-sensitive.' Consider the typical data matrix, in which a column represents the distribution of observations on one variable across many cases, and a row represents one case described comprehensively by observations on many variables. It is conventional to construe the significance of any single observation in the context of all other observations in the same column under the investigator's operational definition of the variable. But it is equally valid to construe the significance of the same observation in the context of all other observations in the same row – for example, one respondent's answers to all questions in a survey. This construction is context-sensitive because it allows the context of the same observation – for example, response A to question 29 in the survey – to vary across cases; and comprehensively described, each case in a row is unique (or nearly so) under the conjunction rule of probability.[14] To simplify the data, the first step is to calculate the similarity or difference between each pair of cases, taking into account all of their relevant characteristics in the data matrix. Then the cases can be classified according to the measure of similarity or difference using various clustering algorithms devised by numerical taxonomists.

The result is a polythetic classification, which is typically more useful in discovering new hypotheses than in testing existing ones.[15] In a polythetic classification, 'No single uniform property is required for the definition of a given group nor will any combination of characteristics necessarily define it. This somewhat disturbing concept is readily apparent when almost any class of objects is examined.' For example, 'Although cows can be described as animals with four legs that give milk, a cow that has three legs and does not give milk will still be recognized as a cow. Conversely, there are other animals with four legs that give milk that are not cows' (Sokal, 1974:1117). Objects in the same class share no more than a 'family resemblance,' and objects in different classes may share some characteristics. In short, these are fuzzy-boundary categories.[16] They are best represented by ideal types of the categories and by those cases (or specimens) that closely approximate the ideal types. Notice that the manifest meaning of any observed characteristic is contingent upon the case, and elaborated through classification of cases and the ideal types and specimens that represent classes. Thus meanings are discovered through context-sensitive methods, not fixed in advance. This helps explain and justify the case-wise construction of the data matrix.

How can we know what we think we know? In a paradigm for practice, the answer to this third question of epistemology is fulfillment of purposes through action. Fulfillment of purposes can be taken as confirmation that what we thought we knew about the context before taking action was satisfactory. That is the very substance of knowledge from the pragmatic standpoint quoted earlier (Kaplan, 1964a:43). Thus the British content analysts' inference from Nazi propaganda that the enemy had new aerial reprisal weapons under development was confirmed to some extent by air raids

and subsequent delays in deployment of the V-1 buzz bomb. Erikson's interpretation of the patient's symptoms and larger problem was confirmed to some extent when intervention based on it helped the patient overcame resistance to his own wish to get well. Stone's work on an ancient puzzle could not be confirmed through action, but publication of investigative reports in his *Weekly* subjected the perspectives behind the publication of those reports to confirmation or disconfirmation according to their practical consequences. Of course success in fulfillment of purposes in one context does not guarantee success in another context. Context-sensitive methods cannot guarantee fulfillment of the purpose of action, but neither can the context-*in*sensitive methods of positive methodology guarantee the fulfillment of their purpose, precise and accurate predictions of broad scope.

The miranda in a paradigm for practice might include exemplars of early aspirations for the policy sciences (Lasswell, 1951b). John Maynard Keynes and Alvin Hansen liberated other economists from the restrictions of classical economics, and helped practitioners meet the economic crisis of the 1930s through the New Deal policies of the Roosevelt Administration. Edited by Gunnar Myrdal, *An American Dilemma: The Negro Problem and Modern Democracy* published in 1944 was the outcome of problem-oriented research on 'A glaring discrepancy between doctrine and practice in the United States' (Lasswell, 1951b:10). Charles E. Merriam was acknowledged for his role in remolding the social sciences for practice between the world wars. 'Merriam stressed the importance of breaking down the barriers that separate scholars from one another, and of leveling-up methodological competence everywhere' (Lasswell, 1951b:6). The Vicos prototype is one exemplary application of the policy sciences recognized by current policy scientists, but not the only one. In 1952, a team of anthropologists under the leadership of Allan Holmberg, including Lasswell as an advisor, took over the remaining five years of a lapsed lease on Vicos, a typical hacienda in the remote highlands of Peru, and continued to advise the Vicosinos until 1962. The project succeeded in initiating for Vicosinos a self-sustaining process of development that continues to this day.[17] According to Holmberg (1971:61), 'The major lesson of Vicos, for Peru as a whole, is that its serf and suppressed peasant populations, once freed and given encouragement, technical assistance, and learning, can pull themselves up by their own bootstraps and become productive citizens of the nation.'

Others have lamented the low status of practical achievements in science and technology generally. The historian Lynn White Jr. (1971:163) concluded from his studies that it is unjustified 'to assume a hierarchy of values which puts practical achievements lower than the theoretical' in the history of science and technology. The physicist Freeman Dyson (1988:138) was more blunt: 'I detest and abhor the academic snobbery which places pure scientists on a higher cultural level than inventors' who find solutions to practical problems. One of Dyson's heroes was Michael Pupin who invented a practical line for transmission of electricity over long distances. Another unsung hero is microbiologist Maurice Hilleman. He was virtually unknown outside his field when he died in 2005, even though he had 'developed vaccines for mumps, measles, chickenpox, pneumonia, meningitis and other diseases, saving tens of millions of lives' and had been awarded a National Medal of Science in 1988 (Altman, 2005).

*Reductionism*

Reductionism is the declining but still dominant scientific paradigm used to justify and challenge knowledge claims relevant to practice. But it is far from sufficient where practical knowledge is concerned. The following account draws primarily on Milton Friedman's (1953) essay, 'The Methodology of Positive Economics,' a seminal source for the social sciences and the policy movement and the one most directly relevant to problems of practice.[18] Friedman's essay focuses briefly on the basic aims and expectations (or doctrine) of reductionism at the outset, and then elaborates positive methodology (or the formula) at some length. It crystallizes basic assumptions that contribute to contemporary problems in practice, including those examined later in section 3. It is also more appropriately qualified than many contemporary understandings of the established paradigm.

The doctrine of reductionism in the policy movement is modeled on physics. According to Friedman (1953:7), 'The ultimate goal of a positive science is the development of a "theory" or "hypothesis" that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed.' Supporting this aim is the basic expectation that diverse observations *can* be reduced to generalizations that yield valid and meaningful predictions. These generalizations have been variously understood as 'causal mechanisms' of rather modest scope (e.g., Brady and Collier, 2004; Gerring, 2004; George and Bennett, 2005) at one extreme, and at the other as fundamental laws in the grand style that are supposed to apply universally, without regard to context. The basic expectation typically has a constructivist corollary, the expectation that the universe of observations ultimately can be explained and predicted from fundamental laws.[19] As a system of generalizations, a theory must make 'correct predictions about the consequences of any change in circumstances' (Friedman, 1953:4). Friedman (1953:24) distinguishes two parts of a theory. The first is an 'abstract model simpler than the "real world" and containing only those forces that the hypothesis asserts to be important'; the second is a set of rules for applying the theory. The abstract model must be 'complete... There is no place in the model for, and no function served by, vagueness, maybe's, or approximations.' The closed, rigorous form of the model enables deduction of precise predictions to be compared with similarly precise observations in testing a theory. Such tests are subject to the doctrine of falsification: 'Factual evidence can never "prove" a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been "confirmed" by experience' (Friedman, 1953:9). The results can be objective. Although Friedman (1953:4–5) acknowledges that the economist is a part of positive economics and interpersonal relations are its subject, neither of these 'difficulties in achieving objectivity' constitutes 'a fundamental distinction between the two groups of sciences.' Thus 'positive economics is, or can be, an "objective" science in precisely the same sense of any of the physical sciences.'[20]

In short, what can we know? In reductionism, the answer is that we can 'confirm' objective generalizations that yield precise, valid, and meaningful predictions of any change in circumstances. For Friedman, this justified a privileged position for positive economics over normative economics in economic policy. He believed that 'differences about economic policy among disinterested citizens derive predominantly from

different predictions about the economic consequences of taking action' (Friedman, 1953:5). 'If this judgment is valid, it means that a consensus on "correct" economic policy depends much less on the progress of normative economics proper than the progress of a positive economics yielding conclusions that are, and deserve to be, widely accepted' (Friedman, 1953:6). Similarly, in a report on *Science – the Endless Frontier* released in 1945, Vannevar Bush (1960) justified a privileged position for basic science in coping with post-war problems of national security, employment, and public health. One current interpretation of this claim is that 'sound science' in the sense of reductionism is a prerequisite for rational policy.

How can we know what we can know? The answer to this second question of epistemology is the methodology of positive science, the basic formula for implementing reductionism doctrine. It is restricted to testing deductions from existing hypotheses, perhaps because 'there is little to say on a formal level' about the construction of new hypotheses (Friedman, 1953:42). Given a hypothesis consistent with the evidence at hand, its 'testing involves deducing from it new facts capable of being observed but not previously known and checking these deduced facts against additional empirical evidence. For this test to be relevant, the deduced facts must be about the class of phenomenon the hypothesis is designed to explain; and they must be well enough defined so that observation can show them to be wrong' (Friedman, 1953:13). In addition, there must be more observations (data points) than terms in the hypothesis. These degrees of freedom are necessary to falsify the hypothesis. Testing can be based *only* on predictions deduced from a hypothesis, not on its assumptions. For Friedman (1953:15), the assumptions of an important hypothesis, one that explains much with little, are always false. Thus 'the relevant question...is not whether [the assumptions] are descriptively "realistic," for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions' (Friedman, 1953:15). A famous example is 'the economic hypothesis that under a wide range of circumstances individual firms behave *as if* they were seeking rationally to maximize their expected returns...and had full knowledge of the data needed to succeed in this attempt' (Friedman, 1953:21; his emphasis).

The methodology is context-*in*sensitive in that the meanings of each observation are fixed and standardized according to the investigator's a priori perspective. Reconsider the typical data matrix in a survey, for example. All respondents who gave response A to question 29 must mean the same thing by it; otherwise, the investigator's operational definition of the variable is invalid. The operational definition temporarily suppresses other responses by the same respondents. The meaning of A and all other responses to question 29 can be refined through cross-classification with another variable, but all respondents in the same category of the cross-classification are still supposed to mean the same thing by the conjunction of their two responses. The conjunction can be further refined through the cross-classification of additional variables, but very quickly the number of categories in the classification exceeds the number of responses, leaving degrees of freedom insufficient to test hypotheses.[21] Similarly, the specification of a formal relationship among variables defines a category of cases; if the relationship does not hold in all the cases to which the model is applied, the relationship is misspecified. Again it is necessary to limit the number of variable and

interaction terms to maintain degrees of freedom sufficient to test the hypothesis. Similarly, in an experimental design, the investigator must control the meanings of observations and relationships to isolate the hypothesized effects of the treatment through comparison of treatment and control group outcomes; otherwise, the design is logically flawed, allowing fallacious conclusions.

In short, the investigator abstracts from the rich meanings of an observation across different contexts of occurrence, and construct their meaning through operational definitions, model specifications, and experimental designs that are standardized between tests. Each definition, specification, and design defines mutually exclusive and exhaustive categories that reduce observations to equivalence of meaning within categories and impose hard boundaries between categories. Friedman (1953:7) summarized criteria for such hard-boundary categories in the following questions: 'Are the categories clearly and precisely defined? Are they exhaustive? Do we know where to file each individual item, or is there considerable ambiguity? ... Does the filing system avoid elaborate cross-references?' He also understood that categories in the first part of a theory, the abstract model, must be considered mutually exclusive and exhaustive for the rigorous deduction of precise quantitative predictions. Otherwise, the vagueness, maybies, or approximations of fuzzy-boundary categories would frustrate rigorous deductions from the model. But it is an empirical question whether the mutually exclusive and exhaustive categories in the model approximate the structure of the system represented by the model, especially if the system is open and evolving. The question is sharply posed, for example, in the assessment by an ecologist that ecology is wholistic in principle but reductionist in practice (B. Patten, personal communication).

Regardless of the particular method used, how can we know what we think we know? Friedman's (1953:4) answer to this third question of epistemology is unequivocal: Like any other positive science, the 'performance [of positive economics] is to be judged by the precision, scope, and conformity with experience of the predictions it yields.' Thus performance improves with greater precision, more general or inclusive scope, and more accurate predictions. Thomas Kuhn (1970:185) corroborates this in a discussion of values for choosing between incompatible practices in a scientific community. 'Probably the most deeply held values concern predictions: they should be accurate; quantitative predictions are preferable to qualitative ones; whatever the margin of permissible error, it should be consistently satisfied in a given field; and so on.'

Isaac Newton's *Principia* (1687) is perhaps the key event in the miranda of reductionism. According to Kuhn (1970:30), 'By the early eighteenth century those scientists who found a paradigm in the *Principia* took the generality of its conclusions for granted, and they had every reason to do so. No other work known to the history of science has simultaneously permitted so large an increase in both the scope and precision of research.' In the social sciences, the miranda are still dominated by nineteenth-century understandings of Newton's three laws of motion, even though they began to be contextualized early in the twentieth century by discoveries in quantum and relativistic physics. Friedman (1953) illustrated his methodology of positive economics with an example from classical mechanics, the distance traveled by a falling object as a function of time, and minimized the differences between positive

social and physical sciences as noted earlier. Herbert Simon (1985:301) acknowledged Newtonian miranda in a lecture for the American Political Science Association. 'We sometimes, perhaps, experience a mild malaise in that our research does not seem to be taking us in the direction of a few sweeping generalizations that encompass the whole of political behavior. A hope of finding our "three laws of motion" was probably a major part of the appeal of rational choice theory in its purer forms.' But Simon quickly discouraged this hope: 'a more careful look at the natural sciences would show us that they, too, get only a little mileage from their general laws. Those laws have to be fleshed out by a myriad of facts, all of which must be harvested by laborious empirical research.'

*Comparisons*

Despite potential complementarities, these two paradigms differ significantly in basic aims, expectations, and methodology. To bring some of these differences into focus, consider hypothetical pure specimens (or ideal types) of the practitioner and the reductionist.

- For the practitioner aiming to solve a problem through action in a particular context, no general hypothesis or theory can be sufficient. Conversely, for the reductionist seeking generalizations to predict across a wide range of contexts, no single case study or context can be sufficient.
- The practitioner needs to construct a detailed map of the particular context comprehensive enough to guide action in the context – a theory for $n = 1$. Conversely, the reductionist must abstract relatively few observations from many contexts to test a general hypothesis or theory.
- The practitioner clarifies meanings by integrating observations and inferences into a better map of the context, which then guides further inquiry. The reductionist standardizes meanings a priori in operational definitions, formal relationships, and experimental designs to test a theory.
- The practitioner takes into account the subjective perspectives of actors in the context mapped, and allows for change in their behavior through insight. Conversely, the reductionist assumes such perspectives are irrelevant; given goals, actors behave *as if* they were objectively rational.
- The practitioner recognizes that his or her value-laden and incomplete perspective affects mapping and self-orientation in the context, and is affected by the context. Conversely, the reductionist assumes scientific conclusions are objective, independent of context in principle.

Notice that these differences do not necessarily coincide with conventional expectations (e.g., Brady and Collier, 2004; Gerring, 2004). For example, the important difference here is between context-sensitive and context-insensitive methods, *not* between qualitative and quantitative methods; and the clarification of causal mechanisms is *not* the only aim or justification for systematic, empirical methods of any kind.

Such differences support the claim that reductionism including positive methodology is insufficient for practice, even if the aims of reductionism were practical.

Regardless of the number of formal generalizations or rules, reductionism as a paradigm leaves too much of practical importance in the particular context in the dark. This includes the perspectives of real people, both researchers and the subjects of research, whose tacit knowledge cannot be codified completely in formal rules.[22] In reductionism it is assumed that what is important is included in a hypothesis or theory, that what is left out is independent of what is included, and that meanings must be fixed and standardized to test a hypothesis or theory. The results must be re-contextualized to be useful for practical purposes, but re-contextualization is not a 'scientific' task as understood in reductionism. In short, reductionism is no adequate substitute for a paradigm for practice. Kuhn's (1970:109) observations on the incommensurability of competing paradigms in the structure of scientific revolutions are relevant here. 'To the extent, as significant as it is incomplete, that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms.' 'Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction' (Kuhn, 1970:150). Advocates of qualitative research have developed a similar point in resisting the hegemony of quantitative research (e. g., Sale et al., 2002).

But consider two other constructions of the differences between reductionism and a contextual alternative that might qualify the first construction up to a point. In a second construction, such differences are complementary in a given research area, especially differences in methods if not methodology (Gerring, 2004). Thus context-sensitive methods supply what positive methods overlook, and vice versa. Together they arrive at the truth, or a closer approximation to the truth. For example, the social psychologist D. T. Campbell (1978:184, 204) concluded that 'quantitative knowing depends on qualitative knowing' but goes beyond it by disclosing 'subtleties that the qualitative [alone] would have missed.' This can be interpreted as a defense of the methodology of reductionism by partial incorporation of qualitative methods, which are not a monopoly of qualitative researchers who might pose a threat.[23] In a third construction, basic science and applied science are complementary in society's division of labor.[24] Scientists build up a reservoir of basic scientific knowledge, while government and industry tap that reservoir for socially-constructive purposes. This was the case Vannevar Bush (1960) made near the end of World War II for government support of basic scientific research unfettered by consideration of applications or practice. Bush's case can be interpreted as a defense of reductionism through partial incorporation of the basic aims and expectations of practice. In other words, basic research is defended and promoted by citing its practical applications.

One can acknowledge some validity in the second and third constructions and at the same time recognize that they help sustain the problematic situation outlined in the introduction to this article: A lack of consensus on a paradigm for practice tends to inhibit the cumulative improvement of knowledge for practice, helps displace practical purposes and discount and delegitimize practical knowledge by default to reductionism, and leaves an opening for faith-based action that disparages laborious empirical inquiry. Consequently, the next section is based on the first construction of paradigm differences. Consideration of other constructions of the paradigm differences is deferred to another occasion.

## 3. Problems in practice

This section reviews manifestations of paradigm problems in psychotherapy, climate change, and more generally, applications of high-modernist ideology. In each case basic assumptions of reductionism distort or displace considerations that are important for practice. Such problems in practice persist despite careful qualifications of the reductionism paradigm and sobering appraisals of outcomes by its own criteria.

### *Manifestations*

Decades ago Erikson (1959:78) recognized paradigm problems in the practice of psychotherapy. He wrote that 'often the only clinical material which impresses some as being at all "scientific" is the evidence of the auxiliary methods of psychotherapy – neurological examination, chemical analysis, sociological study, psychological experiment, etc. – all of which derive their laws of evidence from the non-clinical field, and each of which, strictly speaking, puts the patient into non-therapeutic conditions of observation.' Clearly, he did not expect auxiliary scientific methods to be sufficient for psychotherapy even though they did contribute. Nor did he expect the integrative core of disciplined subjectivity in the psychoanalytic method pioneered by Freud to meet the established scientific standards of his time. As Zaleznik (1996:685) characterized it many decades later, 'The formula that propelled the psychoanalytic method from its inception ("what is unconscious shall be made conscious") remains vitally significant.' Lasswell observed (1930:23) that Freud's method 'grew from the necessities of the exasperated physician,' and emphasized that it included much more than a simple formula: Freud's method 'led him *systematically to treat every manifestation of the individual as part of a related whole.*'[25] Erikson (1959) is an excellent example of practice in this wholistic tradition, even if some psychoanalysts reduce practice to this or that simple formula.

Problems for practice have not disappeared. Psychotherapists today are under pressure from academic scientists and insurers to base their practice on 'scientifically' validated treatments that are 'standardized, easily described in manuals, and can quickly help people with phobias, panic attacks and other problems' (Carey, 2004). Evidently, they would shift the focus of psychotherapy from systematic consideration of the whole person in context to those selected aspects of the person deemed relevant under standardized and codified treatments that have been scientifically validated. The corpus of scientifically validated treatments would in effect displace systematic consideration of the patient as a whole. One psychiatrist, psychoanalyst, and teacher of therapeutic techniques invoked a paradigm for practice in defense: 'The move to worship at the alter of these scientific treatments has been destructive to patients in practice, because the methods tell you very little about how to treat the real and complex people who actually come in for therapy' (Carey, 2004). Moreover, a study of outcomes for 12,000 patients suggested that 'a therapist's competence may be the most critical variable, whatever the brand of therapy.' The author of the study concluded that 'It's not what treatment you give that matters but how competently you give it' (Carey, 2004). Evidently the therapist's personal competence and tacit knowledge have not been reduced to standardized, codified rules. Psychotherapy will continue to struggle

under misapplications of reductionism until the underlying paradigm differences are recognized and respected.[26]

Paradigm problems exist on larger scales as well. The U.S. Global Change Research Program (USGCRP) was established in 1987 by executive order to observe, understand, and predict global environmental change. As suggested by this limited purview, the original justification of the program appealed to reductionism, even though the program was motivated by practical problems of climate change that also would require the clarification of policy goals and alternatives and their adaptation to different contexts. For example, 'Recognizing that effective and rational responses to environmental issues can be built only on sound scientific information,' wrote the Committee on Earth Sciences (1989:8; their emphasis) advising the president, 'the overarching goal of the U.S. Global Change Research Program is: *To gain an adequate predictive understanding of the interactive physical, geological, chemical, biological and social processes that regulate the total Earth system and, hence establish the scientific basis for national and international policy formulation and decisions...*' Similarly, the Office of Management and Budget (1991:101–102) justified the program as a means 'to reduce the significant uncertainty associated with predicting the behavior of the coupled ocean-atmosphere-land system which determines climate phenomena and to form the basis for rational, comprehensive and cost effective responses to climate change.' However, in 1990, this multi-agency program received a policy mandate from Congress 'to produce information readily usable by policymakers attempting to formulate effective strategies for preventing, mitigating, and adapting to the effects of global climate change' (Public Law 101–606, sec. 104).

But consider two anonymous reviewers of a 1995 manuscript that had criticized the justifying rhetoric and took seriously the policy mandate of USGCRP.[27] One reviewer claimed that the manuscript attacked a straw man: 'I do not believe that predictive power [as] a prerequisite to policy action is anywhere nearly as widely shared as the author implies. The [first] Bush administration pushed this line with respect to climate change for the same reason that the Reagan administration pushed the identical line with respect to acid rain...it was a convenient way to stall and avoid taking action. In both cases a large portion of the science community was perfectly happy to accept this convenient fiction, and the associated money, and continue to do the research.' The other reviewer, a self-described manager of an interdisciplinary modeling team that contributed to USGCRP, concurred with the first on the politics of the program, and acknowledged the substitution of basic science for the policy mandate: 'The problem is that (the letter of P. L. 101–606 notwithstanding) the USGCRP is really a basic science program. The appropriate criticism is not that it is a bad policy program, but that historically, it has not been a policy program at all. The fact that it has had to make exaggerated claims to policy relevance to develop funding is as much a failure of politics as it is of science.' Thus it should not be assumed that the scientific community uniformly accepted the justifying rhetoric or policy mandate of USGCRP. For these reviewers it was convenient to deny them in the privacy and anonymity of the peer review process.[28] The justifying rhetoric nevertheless has been effective for scientists and for politicians: Funding for the USGCRP from 1989 through 2005 in then-year dollars totals approximately $25 billion, while legislative mandates forcing action to mitigate greenhouse gas emissions have been effectively blocked.

The USGCRP's policy mandate was an initiative of the House Science Committee under the leadership of its chairman, George E. Brown Jr., who recognized that established science was insufficient and sometimes counterproductive for practical purposes: 'Scientists tell us their data are objective, unburdened by ethical or moral implications. That leaves us politicians free to apply the data in any way we see fit. And we do. Scientific uncertainty has become an operational synonym for inaction on global environmental issues, and the debate over global change has thus become an impediment to action on a wide range of issues critical to our survival' (Brown, 1992a:21). He rejected better predictions as the point of practical policy research: 'The immediate challenge for science and technology must not be viewed as the need to reduce scientific uncertainty about climate warming. This is a hollow ambition. It is too easy to support and too unlikely to bear fruit' (Brown, 1992a:21). Instead, he appealed to a paradigm for practice, insisting that 'scientists and policy makers must work together to make certain that research goals stay focused on policy goals, that "scientific excellence" defines a path for achieving these goals, rather than an excuse for political inaction, that success of research be measured by progress toward a better quality of life for human kind, rather than by number of publications or citations or research grants' (Brown, 1992b:781). Similarly, Herbert Simon (1996:148), a Nobel laureate, observed that 'Predicting the exact course of global warming is a thankless task. Much more feasible and useful is generating alternative policies which can be introduced at appropriate times for slowing the warming, mitigating its unfavorable effects and taking advantage of favorable effects.'

Many publications and reports have been produced under auspices of the USGCRP, parallel climate science programs in other countries, and the Intergovernmental Panel on Climate Change (IPPC), which assesses scientific information for the Conference of the Parties to the U.N. Framework Convention on Climate Change. Yet the overall effort has made little difference in curbing the build up of greenhouse gas emissions that threaten the quality of life worldwide (Malakoff, 1997; Brunner and Klein, 1999; Brunner, 2001; Victor, 2004). The build up continues almost unabated in the U.S. and most other industrialized countries, with the exception of former Soviet-bloc countries that endured sharp declines in economic production after the Cold War.[29] But many citizens still believe leaders of the USGCRP who, beginning in 1989, have cited the '*social, environmental, and economic challenges facing this nation and the world*' as the practical premise for the claim that '*The national goal of developing a predictive understanding of global change is, in its truest sense, science in the service of mankind*' (Subcommittee on Global Change Research, 1997:76; their emphasis).

Others have developed more general diagnoses of paradigm problems in practice. James Scott sought to understand 'why so many well-intended schemes to improve the human condition have gone awry.' One element is a 'high modernist-ideology. It is best conceived as a strong, one might say muscle-bound, version of the self-confidence about scientific and technical progress, the expansion of production, the growing satisfaction of human needs, the mastery of nature (including human nature), and, above all, the rational design of social order commensurate with the scientific understanding of natural laws' (Scott, 1998:4). These formal schemes failed because they necessarily overlooked informal factors in the contexts to which they were applied: 'the simplified rules animating plans for, say, a city, a village, or a collective

farm were inadequate as a set of instructions for creating a functioning social order. The formal scheme was parasitic on informal processes that, alone, it could not create or maintain' (Scott, 1998:6). As a start toward avoiding the replication of such schemes, Scott (1998:6, 7) makes 'the case for the indispensable role of practical knowledge, informal processes, and improvisation in the face of unpredictability.' He also makes 'a strong case about the limits, in principle, of what we are likely to know about complex, functioning order' – an argument he notes that could be turned 'against a certain kind of reductive social science' (Scott, 1998:7). This diagnosis of paradigm problems in practice followed similar diagnoses by the Czech playwright and statesman Vaclav Havel (1992) and others (e.g., Brown, 1992b; Holling, 1995), and has been followed by still others (e.g., Brunner, Steelman et al., 2005).

*Persistence*

Such paradigm problems in practice persist despite careful qualifications that challenge reductionism as used in the policy movement and public policy arenas. Friedman (1953:10) understood that 'No experiment can be completely controlled, and every experience is partly controlled, in the sense that some disturbing influences are relatively constant in the course of it.' He understood that 'If there is one hypothesis that is consistent with the available evidence, there are always an infinite number that are' (Friedman, 1953:9). Objective criteria for choosing among them cannot be specified completely: 'The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria "simplicity" and "fruitfulness," themselves notions that defy completely objective specification' (Friedman, 1953:10). More importantly, even if the model in a theory must be abstract and complete, the rules for testing or applying it 'cannot possibly be abstract and complete. They must be concrete and in consequence incomplete.... In seeking to make our rules as "objective" as possible, our aim should be to formulate the rules explicitly in so far as possible and continually widen the range of phenomena for which it is possible to do so. But no matter how successful we may be in this attempt, there inevitably will remain room for judgment in applying the rules' (Friedman, 1953:25). And finally, 'There is never certainty in science, and the weight of evidence for or against a hypothesis can never be assessed completely "objectively"' (Friedman, 1953:30).

When the rules for applying the model (the second part of a theory) are distinguished from the model itself (the first part) and taken into account, reductionism is no more 'objective' than a paradigm for practice. Indeed, 'much of what passes for objectivity in science is little more than custom and routine, and it is this that often creates the impression that personal judgment has been safely reined in' (Brown et al., 1999:625). For some reductionists, personal judgment based on anything other than supposedly objective data and criteria is a source of anxiety when and if acknowledged. Among practitioners, personal judgment is recognized as a fact of life.

Paradigm problems in practice also persist despite sobering appraisals that have consistently called reductionism into question in the policy movement. For example,

in an appraisal of decades of controlled experiments in educational psychology, Lee J. Cronbach concluded that they had yielded only weak generalizations with many exceptions because of undetected and undetectable interactions among the variables. 'If Aptitude × Treatment × Sex interact, for example, then the Aptitude x 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 analysis...untested interactions of a still higher order can be envisioned' (Cronbach, 1975:119). The variables and interactions left out of the experimental design are not independent of those included. As a result, 'The "laws of learning" from experiments are undependable when taken into the classroom. Many of the problems derive "from failure to take account of important variables in natural educational settings"' (Cronbach, 1975:121, in part quoting McKeachie). Among the interacting and important variables is time, which undermines the stability of generalizations. Consequently, '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.... [W]e cannot store up generalizations and constructs for ultimate assembly into a network' (Cronbach, 1975:122–123).

In an appraisal of the accuracy of a wide range of policy-relevant forecasts, William Ascher concluded that 'all evidence points to the essential importance of the validity of core assumptions antecedent to the choice and application of methodology. Behind any forecast, regardless of the sophistication of methodology, are irreducible assumptions representing the forecaster's basic outlook on the context within which the specific trend develops.... These core assumptions are the major determinants of forecast accuracy' (Ascher, 1981:259; see also Ascher, 1978). In explaining the lack of improvement in the forecast accuracy of complex numerical models, Ascher (1981:258) noted that 'the improvement of substituting new [model] specifications for old ones merely replaced 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 more generally valid over time.' Often the core assumptions are political and subject to change through discrete and discretionary decisions. Similarly, in an appraisal of the policy movement, Alice M. Rivlin (1984: p.:20) concluded that "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 and foreign wars. I doubt we will ever improve the accuracy of forecasting very much, especially the forecasting of economic turning points. Instead, we will have to learn to live with the uncertainty.'

Designers of numerical forecasting models implicitly recognize the importance of omissions in their models when they attempt to consolidate all relevant knowledge and data into a surrogate of their target systems for simulation experiments or predictions.[30] But experience indicates that such models are useful primarily for exploring the consequences of available knowledge, not as surrogates for the target system, because knowledge of the target system is inevitably incomplete (Tennekes, 1990; Bankes, 1993). Incomplete or inaccurate knowledge of initial conditions alone is enough to limit the predictability of deterministic non-linear systems (Lorenz,

1963). For models of complex adaptive systems based on human agents (e.g. Holland, 1992), it would be impractical if not impossible to collect complete and accurate data from the target system to input initial conditions representing the 'internal models' or perspectives of many simulated agents, even if relationships and parameters in the model were correct.[31] Moreover, on logical grounds, 'Verification and validation of numerical models of natural systems is impossible. This is because natural systems are never closed and because models are always non-unique' (Oreskes et al., 1994:641). A difference exists between the world outside and our numerical models, in addition to the pictures in our heads.

*So what?*

There are multiple responses to the problems of reductionism. One response is declining faith in the paradigm. Consider an anonymous response to an earlier assessment of positive methods 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 positivism. And by God it still is of some use in policy analysis and political inquiry' (quoted in Brunner, 1991:76). This is 'positivism light.'[32] It may be interpreted as a defense of positive methodology through rejection of the doctrine, leaving positive methods without an obvious justification for their use. Another response is to reject reductionism and positive methodology altogether. In some circles "'Positivism"...now serves as a term of abuse and perhaps conceals that its critics have no coherent alternative' (Silverman, 1993:198–199). Still another response as noted earlier is to dismiss reductionism as a straw man in the privacy and anonymity of the peer review process while invoking the rhetoric of reductionism in public to justify the USGCRP.

Declining faith in reductionism compromises its rhetorical power in scientific forums and policy arenas. But reductionism continues to function as a means of defending basic science and its privileged position in policy arenas, and does so effectively in connection with the USGCRP. This reductionism in practice is what matters, much more than qualifications like Friedman's (1953) or sobering appraisals like those of Cronbach (1975), Ascher (1978, 1981), and Rivlin (1984). Reductionism in practice is reminiscent of a larger pattern: 'The content of the political doctrine is determined by its political function rather than the matters of fact it purports to describe' (Lasswell and Kaplan, 1950:119).

Those interested in ameliorating paradigm problems might consider why reductionism persists in the policy movement and in policy arenas. Further inquiry might confirm that part of the answer is historical. Reductionism led to science-based technologies that made a significant difference in World War II, including radar and the atomic bomb. President Roosevelt recognized such contributions when he asked Vannevar Bush in November 1944 to advise him on applying the lessons learned in the war to post-war policy problems. Bush (1960) crystallized basic assumptions about reductionism in public policy that were later institutionalized in the National Science Foundation and elsewhere, even though Bush's specific proposals for something like the NSF were set aside (Greenberg, 2001). Another part of the answer is psychological,

involving both conscious and unconscious interests. Most scientists find it more expedient to work through the established paradigm and its various institutionalizations than to challenge them. But this entails selective inattention to disappointing outcomes like those reported by Cronbach, Ascher, and Rivlin, and qualifications like Friedman's. Selective inattention is 'the general predisposition to ignore disturbing news or comment, or to dismiss and neutralize unwelcome messages by the mechanisms of forgetting, denial, substitution... and so on' (Lasswell, 1971:62; see also Sullivan 1973). The resulting blind spots attenuate the anxiety that might otherwise arise, for example, from a conscience that prizes the advancement of knowledge for the benefit of all (Goleman, 1985; Lasswell, 1970b). For some politicians, reductionism is a convenient instrument of politics.

And finally, another reason why reductionism persists is the lack of consensus on a contextual alternative to justify claims of practical knowledge and strategies of practical policy inquiry. An alternative is typically important if not necessary for revolutions in paradigms and other myths (Lasswell, Lerner, and Pool, 1952:4; Kuhn, 1970:77). An alternative paradigm for practice could be the opening wedge in breaking down the psychological, institutional, historical and other factors accounting for the persistence of reductionism and paradigm problems in practice. This brings us full circle back to the rationale for a paradigm for practice in the introduction.

## Conclusion

The task for policy practitioners and their advisors is to consolidate an alternative paradigm for guiding the cumulative development of practical knowledge and skills, communicating them effectively, and assessing particular claims of practical knowledge. The paradigm for practice proposed here is only one point of departure. It will have served its purpose if it stimulates policy scientists and other practitioners to explicate and compare the paradigms for practice underlying their work. Meanwhile, the priority task in further research is the elaboration of conceptual and theoretical tools to improve the production and assessment of practical knowledge. As envisioned in a sequel to this article, these tools will be based on additional examples of good practice in policy inquiry, working criteria for appraisal of the intelligence and appraisal functions in decision process (Lasswell, 1971:85–88, 93–97), and related tools for contextual, problem-oriented, and multi-method inquiry. These tools will specify the central principle in the formula of the paradigm proposed here – the convergence of multiple independent streams of information on the same context – in ways guided by paradigm doctrine.

The established paradigm in the policy movement of the social sciences is not sufficient for cumulative improvements in practice. Reductionism systematically abstracts too much of practical importance out of any particular context and misconstrues what remains. Nevertheless, the results of reductionism can and should be integrated into practice where appropriate for mapping the particular context and orienting the identified self for action in that context. For these practical purposes, quantitative measures can summarize some relevant information on the particular context, and generalizations can serve as supplementary heuristics for understanding that context. But quantitative measures and generalizations used appropriately for practical

purposes mean something quite different than they do in reductionism. Recognizing this requires a degree of self-emancipation from reductionism by those practitioners or advisers socialized in the mainstream of the policy movement. The curricula of public policy and other professional schools might be modified to assist in the task.

This is not abandonment of science but liberation of practice from the exclusive claims of reductionism. The anthropologist Claude Levi-Strauss (1966:16) celebrated the 'science of the concrete' as empirically grounded, and therefore 'no less scientific and its results no less genuine [than the exact natural sciences]. They were achieved ten thousand years earlier and still remain at the basis of our own civilization.' Lasswell and Kaplan (1950:xxii) suggested that the materials for a political science were to be found in 'political prudence,' the working knowledge of politics in various persons and groups. Similarly, the materials for a paradigm for practice exist in the working knowledge of policy practitioners and their advisors. Kaplan (1963:92) went further toward liberating scientific policy inquiries from the exclusive claims of reductionism: 'Policy must be scientific to be effective' he wrote. 'But to say scientific is not to speak [only] 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.'

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## Notes

1. Compare Clark and Majone (1985:17; their emphasis), who 'stressed the need for *appropriate* criticism because, in the vacuum created by its absence, scientific inquiry in policy contexts will continue to be criticized in terms of standards and criteria arrogated from pure science on the one hand and pure politics on the other.'
2. It should also be informed by previous work with related purposes, including most obviously the policy sciences, represented here by the works of Harold D. Lasswell and his collaborators, especially the Introduction to Lasswell and Kaplan (1950), and by Ascher and Hirschfelder-Ascher (2005:7–8). Additional works in the broader policy movement include Campbell (1975) on degrees of freedom in case studies, Schön (1983) on the reflective practitioner, Clark and Majone (1985) on criteria for the appraisal of scientific inquiries with policy implications, Silverman (1993) on interpreting qualitative data, Holling (1995) on a new science of the integration of parts, Frodeman (1995) on geology as an historical and interpretative science, and Simon (1996) on the sciences of the artificial, including design. Predating the policy movement are relevant and insightful works on history by Pirenne (1931) and Becker (1932).

3. George (1956:335) was careful to point out that 'the indirect method is applicable only to those intended actions of an opponent that involve some measure of deliberation and policy-planning on his part.' This excludes spontaneous reactions. The method is also inapplicable when enemy elites use channels of communication closed to the propaganda analyst, and when they decide against preparatory propaganda for action.
4. 4. In the policy sciences this assumption is made explicit in the maximization postulate, 'which 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 alternative act completions open to him in a given situation' (Lasswell 1971:16).
5. Anticipating Lasswell's (1971:16) maximization postulate, Pirenne (1931:442) generalized his point: 'All historical construction – which amounts to saying all historical narrative – rests upon a postulate: that of the eternal identity of human nature. One cannot comprehend men's actions at all unless one assumes in the beginning that their physical and moral beings have been at all periods what they are today.'
6. In a presidential address to the Geological Society of America informed in part by Frodeman (1995), Victor Baker (1999:633) claims that 'Almost alone among modern sciences, geology has preserved a method of inquiry that emphasizes synthetic reasoning for the interpretation of Earth's signified causal processes.'
7. Thomas Kuhn (1970:92–94) noted functional similarities between scientific and political revolutions.
8. Lippmann (1922:235) illustrates invocation of the miranda: 'The question of a proper fare on a municipal subway is symbolized as an issue between the People and the Interests, and then the People is inserted in the symbol American, so that finally in the heat of a campaign, an eight cent fare becomes un-American. Lincoln suffered that it might not come to pass....' Doctrinal symbols also figure in constructing a consensus on the issues of the day: 'When political parties or newspapers declare for Americanism, Progressivism, Law and Justice, and Humanity, they hope to amalgamate the emotion of conflicting factions which would surely divide, if, instead of these symbols, they were invited to discuss a specific program' (Lippmann, 1922:206). On key symbols see Lasswell (1954).
9. On index instability see Lasswell and Kaplan (1950:xx–xxi) and Cronbach (1975). On the contingency of propositions compare Lasswell (1930:260): 'If events appear to be predictable, this is so because our knowledge of contingencies is limited, and our sequences of similar configurations may still be treated as special instances of "no sequence." The stable is a special case of the unstable, to put the ultimate paradox.'
10. Compare (Burks 1946:306) on pragmatism: 'an hypothesis has meaning if and only if it has empirical or practical consequences, and ... the sum total of those consequences is its total meaning.'
11. Compare Kaplan (1964a:47): 'We may be free to choose the purpose, but we are not free to determine that the purpose shall be fulfilled as we ourselves choose.'
12. See also Fann (1970). Anticipating Campbell (1975:181–182), Peirce (quoted in Burks 1946:306) also insisted that the conclusion of abduction, an explanatory hypothesis, 'should be such that definite consequences can be plentifully deduced from it of a kind which can be checked by observation.'
13. In 1929, Sir Josiah Stafford observed that 'The government is very keen on amassing statistics.... But you must never forget that every one of those figures comes in the first instance from the [village watchman], who just puts down what he damn pleases' (quoted in Cochran et al. 1980:6). See also Cochran (1978) and Huff (1954).
14. The probability of the conjunction $(A, B, \ldots, X)$ is less than or equal to the probability of $(A) \times$ the probability of $(B), \ldots, x$ the probability of $(X)$. This is easily visualized with Venn diagrams.
15. Compare Thomas Kuhn (1970:200): 'The practice of normal science depends on the ability, acquired from exemplars, to group objects and situations into similarity sets which are primitive in the sense that the grouping is done without an answer to the question, "Similar with respect to what?"' A polythetic classification can test the implicit hypothesis in regression analysis that one statistical relationship describes every case in the data matrix.
16. Concepts in natural language also are fuzzy-boundary in form (Rosch 1978). For example, 'chair' is more representative of the concept 'furniture' than 'rug' and 'telephone,' which are relatively peripheral. See also Kuhn (1970:44–45).

17. For appraisals of the Cornell-Peru Project in Vicos, see Paul Doughty (2002). Doughty participated in the project, part of a distinguished career that earned him the 2005 Malinowski Award of the Society for Applied Anthropology.
18. However, both 'reductionism' and 'positivism' are references to fuzzy-boundary categories of basic perspectives that are not necessarily equivalent or even compatible. For example, a peer review directed attention to the following statement from a 1931 paper by Moritz Schlick (1979; his emphasis), founder of the Vienna Circle of logical positivism: 'The laws of nature ... cannot be verified for *all* cases; they are prescriptions, rather, rules of procedure that direct the scientist to orient himself in reality, to discover true propositions, to expect certain events.' This heuristic function of scientific propositions is part of the doctrine in the paradigm for practice sketched earlier.
19. Philip Anderson (1972:393), a solid state physicist and Nobel Prize laureate, accepts the reductionist assumption, which he calls a 'hypothesis,' but rejects the constructivist corollary: 'The ability to reduce everything to simple fundamental laws does not imply the ability to start from those laws and reconstruct the universe.' This is a seminal article in complex adaptive systems research in which emergence is an important theme (Horgan, 1996; Holland, 1992). Polanyi (1966:34) anticipated Anderson's point in a lecture titled 'Emergence': 'the laws governing the particulars in themselves would never account for the organizing principles of a higher entity which they form.'
20. On objectivity, some critics go beyond Friedman to a set of assumptions about reality. 'The ontological position of the quantitative paradigm is that there is only one truth, an objective reality that exists independent of human perception. Epistemologically, the investigator and investigated are independent entities. Therefore, the investigator is capable of studying a phenomenon without influencing or being influenced by it...' (Sale et al., 2002:44). In his Postscript Kuhn (1970:206) concludes that 'the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems illusory ... in principle.'
21. For example, the Census Bureau once used at least 25 variables, with 2 to 13 categories each, to describe households in poverty in the U.S. A cross-classification of all these variables would have $2.24 \times 10^{14}$ categories, a number large enough that each category would have an expected frequency of 0 or 1. Each case is unique (or nearly so) under a comprehensive description (Brunner, 1986). Empirically, a $5000 annual income meant different things in different kinds of poor households (Brunner, 1983).
22. Polanyi (1966:20) demonstrates that 'the process of formalizing all knowledge to the exclusion of any tacit knowledge is self-defeating.' See also Kuhn (1970:192).
23. On the mechanism of defense through partial incorporation among other myth dynamics, see Lasswell, Lerner, and Pool (1952:4–6). The dynamics of symbol expansion and contraction (Lasswell and Kaplan, 1950:104–107) also are relevant to the politics of paradigm differences. As a variation on the second construction, positive methodology might be defended by partial incorporation of context-sensitive methods as complementary *between* formal tests of theories.
24. However, applied science (technology) is not equivalent to practice, but only part of practice when it informs a decision for action in a particular context. Greenberg (2001:4) notes that 'The similarities and differences between science and technology, and their relationship, have spawned long running definitional disputes, with the current fashion favoring a blurring or rejection of a distinction between basic and applied research.'
25. Compare Lasswell (1977:277) in afterthoughts on Lasswell (1930): 'To describe a personality system comprehensively at any cross-section in time is to discover the pattern of value demands (and specifications) of the person, and to describe the mechanisms chiefly relied upon by the person in seeking to maintain internal order within a changing configuration of external environments. The value demands are selective in regard to outcome; the mechanisms are selective in terms of the style by which outcomes are sought.'
26. Meanwhile, it is worth recalling that pragmatism long ago provided critical insights for justifying psychodyamic theory and practices (Ascher and Hirschfelder-Ascher, 2005:7).
27. The manuscript in question was later revised slightly and published as Brunner (1996), not as a scientific contribution but as an editorial accompanied by two editorial responses. The reviews are on file with the author.
28. Compare Greenberg (2001:26): 'In science politics, as in all politics, the realities of the past are often reshaped to suit the needs of the present.' Compare also the broadest hypothesis from the comparative study of ideologies: '*Any general symbol may be elaborated in support of any specific proposition;*

*any general or specific symbol may be applied or nonapplied in action*' (Lasswell, Lerner, and Pool 1952:10; their emphasis).

29. Data on greenhouse gas emissions for the U.S. and other industrialized countries are available in the Greenhouse Gases Database updated by the U.N. Framework Commission on Climate at http://ghg.unfccc.int/.
30. In plenary session remarks at the workshop on 'Climate Science in Support of Decisionmaking,' sponsored by the U.S. Climate Change Science Program (CCSP), Alexandria, VA, November 14–16, 2005, James Mahoney, director of CCSP, justified numerical models as a top priority of CCSP by citing them as the principal means of integrating climate change knowledge and data. The CCSP includes the USGCRP and the Climate Change Research Initiative.
31. In connection with the garbage can model of organizational choice, a precursor to models of this kind, it has been acknowledged that 'No real system can be fully characterized in this way,' even though 'the simulated organization exhibits behaviors which can be observed some of the time in almost all organizations' (Cohen, March, and Olsen, 1972:16).
32. This is also an example of the law of the instrument: 'Give a small boy a hammer, and he will find that everything he encounters needs pounding' (Kaplan 1964b:60). The question is not whether positive methods are useful, but for what purposes, in what circumstances, and compared to what alternatives.

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