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Steve Fuller


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A Review of Reviews

BEING THERE WITH THOMAS KUHN:
A PARABLE FOR POSTMODERN TIMES*

STEVE FULLER

ABSTRACT

Although *The Structure of Scientific Revolutions* is one of the most influential books of this century, its author, Thomas Kuhn, is notorious for disavowing most of the consequences wrought by his text. Insofar as these consequences have appeared "radical" or "antipositivist," this article argues that they are very misleading, and that Kuhn's complaints are therefore well placed. Indeed, Kuhn unwittingly succeeded where Daniel Bell's *The End of Ideology* tried and failed, namely, to alleviate the anxieties of alienated academics and defensive policy-makers by teaching them that they could all profit from solving their own paradigmatic puzzles. The influence of *Structure* is traced from the philosophy of science into the social sciences and science policy. Special attention is paid to the import of the General Education in Science curriculum at Harvard, in which Kuhn taught for most of the period prior to writing *Structure*. Harvard President James Conant had designed this curriculum in order to keep "pure science" in the good favor of the American public, in whose eyes it suffered after the use of the atomic bomb. While Conant was keen to stress the distinctiveness of science from other social practices, Kuhn's model seemed to provide a blueprint for reconstituting any practice as a science. This enabled potentially antiscientific academics to become scientists themselves, thereby neutralizing any radical challenges to the ends of scientific inquiry. The article concludes by reconstructing some of the inchoate possibilities for radical critique that Kuhn's success preempted, and by making some suggestions for how they may be recovered in the present academic environment.

“Perhaps the revolutionary never existed—but then it was necessary to invent him.”
—Alan Musgrave (on Kuhn's denial of the radical views imputed to him)1

Contrary to expectations, the title of this essay does not refer to the recent interesting attempts to accommodate Kuhn's account of scientific practice to the existential phenomenology of Martin Heidegger.2 Rather, it refers to the critically acclaimed motion picture starring Peter Sellers. In what follows, the

* A review of the reception and importance of Thomas Kuhn's *The Structure of Scientific Revolutions* on the thirtieth anniversary of its publication.

movie *Being There* will serve as a “paradigm” for understanding the reception of Thomas Kuhn's *The Structure of Scientific Revolutions*, one of the most consistently influential academic works of the twentieth century.\(^3\) Since 1962, when the book was first published, over one million copies have been sold worldwide, including translations into sixteen languages.\(^4\) Yet, from the first opportunity he was given (up to his most recent interview), Kuhn has disavowed all of the more exciting and radical theses imputed to him by friends and foes alike.\(^5\) Indeed, he has even endeavored to demonstrate by example, in his subsequent work, that he could not have been capable of anything so exciting and radical.\(^6\) It is doubtful that there has ever been another academic who has met the greatness thrust upon him with such apparent ingratitude.\(^7\) What is more remarkable, of course, is how, in full cognizance of Kuhn’s disclaimers, so much of the academic world has nevertheless felt compelled to position themselves with regard to his work.\(^8\) What might we learn from this very strange episode in the annals of scholarship? But, first, a quick trip to the movies... . . .

The protagonist of *Being There* is Chauncey, the ward of a wealthy Washingtonian whose death marks the beginning of the film. Chauncey is a kindly man of childlike simplicity who, in the course of settling the estate of his deceased employer, is subject to several misunderstandings that, by the end of the film, land him as a candidate for the presidency of the United States. The comic element of the plot is that Chauncey never quite realizes what it is that he is saying that makes all the bigwigs hold his opinion in increasingly high regard. Indeed, for the most part, Chauncey is uncomfortable with this newfound attention and makes periodic protests that he is not the man they think he is. Needless to say, these protests are either ignored or themselves misinterpreted. From the audience’s standpoint, what seems to be happening is that Chauncey’s interlocutors take his quite literal references to gardening and television, the

7. Horgan quotes Kuhn as saying, "I've often said I'm much fonder of my critics than my fans" (49). Karl Marx ran a close second to Kuhn in his desire to disown his admirers.
8. Many of the luminous who have so positioned themselves are collected in Gutting. However, the most artful "Kuhnnotropist" is undoubtedly the sociologist Robert Merton, who tries to demystify any aura of genius that might surround Kuhn by showing that the success of *Structure* can be easily explained by Merton's own "principle of cumulative advantage." (The explanation of Merton's own genius in arriving at this principle will no doubt be tackled by some future historian, hopefully not at the expense of one of Merton's own theories!) See Robert Merton, *The Sociology of Science: An Episodic Memoir* (Carbondale, Ill., 1977), 71–108. I myself have indulged in shameless Kuhnnotrope, as evidenced in Steve Fuller, *Social Epistemology* (Bloomington, Ind., 1988), 85–89, 97–98, 111–119, 142–169, 219–224. This essay is partly in atonement for my errant ways.
entire scope of his existence, as metaphors for various aspects of political life. Moreover, Chauncey unwittingly confers the utmost profundity on his utterances by speaking not very often, and then only in short and simple sentences. *Being There* is clearly a spoof on the superficiality of American politics, but it is more than that. Some of Chauncey’s interlocutors have lingering doubts about his untraceable past and peculiar manner, but typically they give him the benefit of the doubt and try to make the most of the situation. In fact, the collective participation needed to propel Chauncey to ever greater acclaim is increasingly evident in the film, until he becomes a genuine rallying point for the disparate individuals involved. From a similar comedy of errors that has marked Kuhn’s reception, I hope to draw a somewhat hopeful lesson. But for readers unfamiliar with the career of *The Structure of Scientific Revolutions*, a few details are in order to show that “comedy of errors” does not exaggerate the book’s reception in the field from which its influence has flowed, the philosophy of science.

The first extensive critical review of *Structure* is the one that emblazoned Kuhn in the minds of philosophers. Appearing in one of America’s premier journals, it was written by a young, Harvard-trained philosopher who had already published several articles showing that the logical analysis of language championed by the logical positivists did not do justice even to the very science they cherished most, physics. Like all language-games, Dudley Shapere argued, physics too has an irreducibly pragmatic character that can be understood only by a historically sensitized version of ordinary language philosophy, that is, a study of what members of a particular physics community meant in the context of their utterance.9 Although Shapere thought that Stephen Toulmin had made many of Kuhn’s points—and in a philosophically more perspicacious manner—perhaps a decade earlier, the express generality of *Structure*’s account of science and its innocence of any formal logical apparatus made it an apt vehicle for enumerating the full range of omissions from the positivist account of science.10 More-

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10. Shapere cites Toulmin’s two books on the philosophy of science which predate Kuhn’s: Stephen Toulmin, *The Philosophy of Science: An Introduction* (London, 1951); *Foresight and Understanding* (London, 1961). Toulmin shares with Kuhn an early background in physics and shares with Shapere philosophical training in linguistic analysis. Both of Toulmin’s books went into several editions and (unlike *Structure*) were picked up by commercial publishers. And while Toulmin seemed to attract a wide audience (for example, both Ernest Nagel and Jacques Barzun endorsed his books), I would suggest that the reason he never garnered Kuhn-like attention was that he wrote in the understated Wittgensteinian style of perceptively remarking on simple examples without issuing anything that might smack of a “theory” or a “scheme.”
over, by making Kuhn out to be proposing an “anti-positivist” viewpoint, Shapere made himself appear less an opponent than a mediator of opposing camps. Thus, Shapere could demonstrate the value of philosophical sophistication in moderating the more wild-eyed claims of historians overly impressed with the remoteness of the past.\(^{11}\)

The subsequent “historicist” turn in the philosophy of science—portrayed specifically as a “revolt against positivism”—proves Shapere to have been largely successful in his immediate intention.\(^{12}\) Unfortunately, there is little reason to think that it was an intention shared by Thomas Kuhn, who openly solicited that donyen of positivists, Rudolf Carnap, to have _Structure_ published as part of the International Encyclopedia of Unified Science. As personal correspondence reveals, Carnap warmly reciprocated, complimenting Kuhn on having provided a historical grounding—in the contrast between “normal” and “revolutionary” science—for the positivist distinction between questions that are decidable within the terms of a conceptual framework and those that require the introduction of extramural factors.\(^{13}\) And, over the years, Kuhn certainly has wanted to leave the impression that he has been more influenced by positivist, or at least more broadly “analytic,” considerations about the nature of language and knowledge than by the historicist ones that his work supposedly generated.\(^{14}\)

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11. This was a recurrent theme in the work of Norwood Russell Hanson, who was the most visible American standard-bearer for the emerging field of “History and Philosophy of Science” from the publication of _Patterns of Discovery_ (Cambridge, Eng., 1958) to his death a decade later. Unlike the other philosophers considered here (including Kuhn), Hanson frequently appeared in the pages of American newstand periodicals (especially the _Nation_) during the late 1950s and early 1960s. Even pages from _Patterns_ were excerpted in _The Saturday Review_. In addition, Hanson helped established History & Philosophy of Science programs at Cambridge, Indiana, and Yale Universities. Of the figures discussed here, Hanson seemed to be the most preoccupied with defining and crossing disciplinary boundaries. Characteristic work in this vein include “Scientists and Logicians: A Confrontation,” _Science_ 138 (1962), 1311–1314; “The Irrelevance of History of Science to Philosophy of Science,” in _What I Do Not Believe and Other Essays_, ed. S. Toulmin and H. Woolf (Dordrecht, 1971), 274–287.

12. The canonical presentation of the story Shapere wanted his audience to believe is _The Structure of Scientific Theories_, ed. Frederick Suppe [1973], 2nd ed. (Urbana, 1977), 3–232. In an afterword to the second edition, Suppe predicted that Kuhn’s influence was waning and that the future of historicist philosophy of science belonged to Shapere and Toulmin. In retrospect, the most striking omission from Suppe’s 800-page tome is any reference to the person who turned out to carry the torch for historicism, Larry Laudan, whose _Progress and Its Problems_ (Berkeley, 1977) was published the very same year as the second edition of Suppe’s book. Chapter seven of Laudan’s book threw down the gauntlet at the emerging field Sociology of Scientific Knowledge, which (see below) stands in an interesting relation to Kuhn.


14. For Kuhn’s increasing tendency to embrace positivism, see Suppe, 647. Also, contrast Kuhn’s early disavowal of any connection to the Sociology of Scientific Knowledge (Kuhn, _Essential Tension_, xxi–xxii), with Kuhn’s eagerness to insert himself in recent philosophical debates in semantics and the theory of reference (Kuhn, “Possible Worlds in History of Science,” in _Possible Worlds in Humanities, Arts and Sciences_, ed. S. Allen [Berlin, 1989], 9–32). Finally, consider Kuhn’s accommodating posture to the criticisms made of his work by such positivistically inclined philosophers as Carl Hempel and Wesley Salmon: Kuhn, “Rationality and Theory Choice,” _Journal of Philosophy_ 80 (1983), 563–570.
understand the overarching significance of Structure, especially the sorts of projects it has helped and impeded, we need to start taking seriously that Kuhn's book constituted, pace Shapere, less a revolt against positivism than a continuation of positivism by other means. However, this is not to deny to Shapere the honor of having turned Kuhn into an object of philosophical fascination by parceling out Structure into analysis-sized problems of "meaning-variance," "theory-laden observation," and "absolute presuppositions." In this way, Shapere enabled Kuhn to become routinely discussed in the same breath as the ascendant philosophers of science of his generation, especially Toulmin, Russ Hanson, and, of course, Paul Feyerabend.15

In the Anglo-American philosophical community of the early 1960s, Shapere's constructed cohort of "historicists" were still perceived—to refashion a bit of Kuhn—as mere anomaly-mongers, proffering elaborate counterexamples to positivist accounts of explanation and confirmation, anomalies which, once stripped of their historical superfluities, would be rendered tractable to logical analysis.16 The idea that the historicists might replace or succeed the positivists in a common lineage did not appear persuasive (or at least not an item discussed in the main philosophy of science journals) until Imre Lakatos published a volume of papers from a symposium held during the 1965 meeting of the International Congress of Logic, Methodology, and the Philosophy of Science, which was hosted by Lakatos's home institution, the London School of Economics.17 The symposium in question was officially devoted to the implications of Karl Popper's philosophy of science, but Lakatos was keen on using the

15. Kuhn, Toulmin, and Imre Lakatos (to be discussed below) were all born in 1922. Hanson and Feyerabend were born in 1924. Shapere aside, the clustering of these figures as an intellectual cohort is largely a product of the next generation of philosophers, such as Ian Hacking, Larry Laudan, Harold Brown, Fred Suppe, and Peter Machamer, all of whom were born around 1940. See, especially, Machamer, "Understanding Scientific Change," Studies in History and Philosophy of Science 5 (1975), 373–381; Brown, Perception, Theory, and Commitment: The New Philosophy of Science (Chicago, 1977). The latter, a textbook, received Kuhn's official endorsement.

16. Of all the historicists, this unflattering image stuck most readily to Feyerabend, who in 1964 was most famous for challenging Ernest Nagel's model of explanation by reduction: Paul Feyerabend, "Explanation, Reduction, and Empiricism," in Minnesota Studies in the Philosophy of Science, ed. H. Feigl and G. Maxwell (Minneapolis, 1962), III, 28–97. In retrospect, this challenge is normally regarded as the first time the authority of the historical record had successfully opposed a formal logical model. Although few philosophers relinquished the goal of reductionism at that time, rather than simply ignoring Feyerabend as raising irrelevant concerns, they endeavored to accommodate Feyerabend's challenge within the model. See Kenneth Schaffner, "Approaches to Reduction," Philosophy of Science 34 (1967), 137–147. During most of the 1960s, Kuhn's name typically appeared in philosophy journals as a corroborating footnote to a discussion of Feyerabend's challenges. It is important to remember that Feyerabend did not consolidate his own philosophy of science into a readily available text until the mid-1970s: Feyerabend, Against Method (London, 1975). Before that time, Feyerabend was taken to be a radical philosopher of physics, whose views had interesting implications for the philosophy of science more generally. An apt comparison in our own day is with Arthur Fine, whom Joseph Rouse expects will come out of the closet shortly. See Rouse, "The Politics of Postmodern Philosophy of Science," Philosophy of Science 58 (1991), 607–627.

forum to present himself as the heir-apparent to Popper's chair. However, as fate would have it, Kuhn stole the show, with his contribution to a Popper Festschrift being the one in terms of which all the other speakers—including Popper, Lakatos, Toulmin, and Feyerabend—rewrote their papers. Thus, Criticism and the Growth of Knowledge, despite the Popperism packed into its title, is normally taught in seminars today as a repository of "famous responses to Kuhn." And, after its publication, Kuhn starts to get routinely cited in order to legitimate various developments across the humanities and social sciences.

Despite Shapere's success at mounting a movement against the positivist regime in the philosophy of science, he failed to anticipate that his main vehicle, Kuhn, would turn out to be a Trojan Horse. Because we still think of Feyerabend, Toulmin, Hanson, and Kuhn as of a piece, the views of the first three thinkers tend to be reduced to those of the fourth, the details of which are typically best known to us. However, this reduction obscures the criticisms that Kuhn's supposed comrades lodged against his views, which were more substantial than those that the positivists ever raised. A less pronounced, but similar, occlusion of differences has even occurred to the parallel band of anti-positivists in Britain represented by Popper and his followers. They, in particular, detected an unsavory affinity between Kuhn and the positivists. Kuhn seemed to identify the essence of the scientific enterprise with puzzle-solving, specifically filling in the gaps of a paradigm, a practice that suspiciously resembled the logical articulation and empirical confirmation of a theory. Where was the image of science as a critical enterprise, one—as Popper liked to say—continuous with Socratic questioning? In Kuhn's scheme, the answer was to be found in the relatively rare instances of "revolutionary science," whose resolution depended entirely on which of the contesting sides eventually turns out to have the most followers. Thus, the most important moment in any inquiry—namely, determining how to proceed so as to best satisfy the ends of inquiry—

18. Thus, Lakatos's most famous and sustained post-dissertation work appeared in this volume: "Falsification and the Methodology of Scientific Research Programmes," in Criticism, 91–196. A relevant point here is that Popper was awarded his chair at the LSE in the "Logic of the Social Sciences," based largely on such wartime works as The Poverty of Historicism and The Open Society and Its Enemies. Not surprisingly, Popper's main lobbyist for a permanent post at the LSE had been the liberal economist Friedrich von Hayek. The seminal work in methodology for which Popper is perhaps best known today, The Logic of Scientific Discovery, was a product of his younger days in Austria and was not translated into English until 1959. Thus, Popper's reputation as a general philosopher of science in the English-speaking world emerged as part of "the revolt against positivism," even though Carnap (again!) supported the original publication of Popper's book in the series associated with the positivist journal, Erkenntnis.


20. The best of these papers are compiled in Gutting. The publication of Criticism coincided with the second edition of Structure, in which Kuhn answered some of his critics in a postscript. As one might expect, Kuhn has himself consistently denied that his model of scientific change applies to the humanities and social sciences. In fact, Kuhn had this to say when invited to a conference in which Structure was discussed as one of the major advances of twentieth century social sciences: "I know a great deal less than I should, and in any case virtually nothing at all, about the social sciences and I will not create confusion by bluffing it" (letter to Karl Deutsch, cited in Advances in the Social Sciences, 1900–1980, ed. K. Deutsch, A. Markovits, and J. Platt [Cambridge, Mass., 1986], 278).
was removed from the realm of rational deliberation to an apparently blind process akin to natural selection, the so-called Planck Effect.\textsuperscript{21} As we shall see, the political imagery of revolution added an element of risk to this unpredictability, which subtly suggested that the questioning of ends should occur neither too often nor for too long.\textsuperscript{22}

It is worth dwelling for a moment on the significance that Kuhn's philosophical critics attached to the word "rational" and especially its counterpart, "irrational." These words charged a great many academic debates in the 1960s, many of which involved Popper as the standard-bearer for "critical rationalism."\textsuperscript{23} A common way of describing the aftermath of these debates provided much of the early legitimation for the first field to declare itself the natural successor to Kuhn's project, namely, the Sociology of Scientific Knowledge.\textsuperscript{24} According to

\textsuperscript{21} "Max Planck, surveying his own career in his \textit{Scientific Autobiography}, sadly remarked that 'a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it'" (Kuhn, \textit{Structure}, 151).

\textsuperscript{22} At the time, Kuhn's fondness for the imagery of religious conversion raised the most Popperian hackles, as it seemed to imply that scientific revolutions could not be resolved rationally \textit{precisely because} they involved life decisions of the utmost importance. Ironically the Popperians actually agreed with the positivists and Kuhn that decisions concerning the "absolute presuppositions" of one's practice are a matter of free existential choice. (I imagine that neo-Kantianism, especially as filtered by Max Weber, is the source of this sentiment. See Robert Proctor, \textit{Value-Free Science? Purity and Power in Modern Knowledge} [Cambridge, Mass., 1991], 151–154.) However, for that very reason, Popperians have held that the merits of such a choice must be evaluated in terms of its consequences (the extent of which are, unfortunately, rarely specified). Thus, rational beings act so as to be constrained only by the anticipation that they may want to reverse that decision at a later date, if the consequences of their actions do not turn out as planned. An ethic of "piecemeal engineering" (Popper's phrase) naturally follows from this imperative. Ironically, in the same year that Kuhn's book was published, a work by a student of Popper's was published, initially to more fanfare, which argued against the irrationality implicit in the refusal of Liberal Protestant theologians to contest absolute presuppositions: See W. W. Bartley III, \textit{The Retreat to Commitment} [1962], 2nd ed. (La Salle, Ill., 1964).

\textsuperscript{23} The most famous was probably the German \textit{Positivismusstreit} that pitted Popperian social and economic theorists (including Ralf Dahrendorf and Hans Albert) against the Frankfurt School, especially Theodor Adorno and his understudy, Jürgen Habermas, who rose to prominence in the following decade for his skill in mediating and synthesizing alternatives forms of rationality. See \textit{The Positivist Dispute in German Sociology}, ed. Theodor Adorno (London, 1976). Another noteworthy rationality debate, confined largely to Britain and its spheres of influence, concerned whether there were cross-culturally valid, universal standards of reasoning. It pitted against one another noted anthropologists and philosophers, such as Ernest Gellner, Peter Winch, Alasdair MacIntyre, Charles Taylor, Steven Lukes, and Ian Jarvie. See \textit{Rationality}, ed. Bryan Wilson (Oxford, 1970).

\textsuperscript{24} In its early days, the Sociology of Scientific Knowledge (SSK) was known as the "Strong Programme" to distinguish its own methodological imperatives from those of the founder of the sociology of knowledge, Karl Mannheim, who argued that the rational methodology of the natural sciences precluded them from sociological explanation. Since the early 1970s, when SSK first made its presence felt from the Science Studies Unit at Edinburgh University, a burgeoning and diverse body of empirical research has been promoted in departments of "Science & Technology Studies" in Europe, Australia, and increasingly America. The "Edinburgh School" made the first institutional attempt to integrate history, philosophy, and sociology of science—the three fields that Kuhn was seen as having brought together in \textit{Structure}. The canonical formulation is David Bloor, \textit{Knowledge and Social Imagery} (London, 1976). Barry Barnes has drawn detailed connections between Kuhn's work and subsequent developments in SSK in \textit{T. S. Kuhn and Social Science} (Oxford, 1982).
this account, rationalist philosophers had supposed that, without articulate knowledge of the norms of the scientific method, all order in science would dissolve and scientists would simply go about doing whatever they pleased. Nevertheless, so the sociologists argued, by closely attending to the laboratory practices of scientists, one can see that they get on quite well and regularly, even though they are typically unable to articulate the principles that govern their practices. What this seems to show, then, is that no obvious empirical place can be found in scientific practice for the methodological norms so cherished by philosophers. While philosophers should have met this line of argument easily, it is clear, in retrospect, that they did not. As a result, a valuable opportunity was missed to explain the relevant sense in which the rational/irrational distinction had to be drawn in order for the normative philosophical project to flourish.

When one looks at the typical cases of "irrationality" that worry Popperians and other philosophers so much, they are not associated with mass hysteria or other undisciplined forms of behavior. Rather, irrationality is attached to a certain complacency or inertia that arises from one's expectations being repeatedly confirmed or challenged only in ways that are easily accommodated or rebuffed. Rationality, then, is the turn of mind that resists habit, typically by criticism and actively confounding expectations. Kuhn's apparent satisfaction with the preponderance of science being unreflective puzzle-solving is the clearest indication to his philosophical critics that Structure presents an "irrationalist" picture of the scientific enterprise. One cannot underestimate the extent to which this Enlightenment idea of "Reason" triumphing over superstition and tradition remains a ready source of philosophical self-imagery even in our own day. However, in the heat of philosophical debate, this eighteenth-century notion has been periodically confused with a conception of reason that came to the fore in the nineteenth century, especially with the rise of positivism. Here "reason" is a governing principle, one that regulates the growth of knowledge by directing and measuring the path of inquiry. Presupposed is that, without such a governing principle, human energies would be dissipated into random motion. In that case, irrationality would indeed be akin to mass hysteria.


26. In fact, the distinguished Popperian anthropologist Ernest Gellner bases his entire philosophy of history on the idea that, because logical coherence is inversely related to social coherence, any attempt to achieve a more logically comprehensive picture of reality has required challenging the normative integrity of one's own social order. See Ernest Gellner, Plough, Sword, and Book (Chicago, 1989). It should be clear that I concur with the spirit of Gellner's thesis, if not its exact letter.


28. What has changed over the last two centuries is the metaphysical warrant for Enlightenment. There is less of an appeal to Reason as the essence of human nature (though it remains in Jürgen Habermas, today's leading Enlightenment thinker). For a non-essentialist version, see Steve Fuller, Philosophy, Rhetoric, and the End of Knowledge (Madison, Wisc., 1992).

29. On the changing conceptions of reason, see Maurice Mandelbaum, History, Man, and Reason: A Study in the Nineteenth Century (Baltimore, 1971). Kuhn is not alone in confusing the two senses of reason. Richard Rorty's Philosophy and the Mirror of Nature refutes the notion of
To illustrate the confusion that results when these two conceptions of reason are run together, consider Lakatos's famous charge that Kuhn is offering a "mob psychology" account of scientific revolutions.\textsuperscript{30} In response, calmer heads have reminded us that Lakatos overstates his case, as Kuhn never really said that scientists take leave of their senses once shaken from the spell of a paradigm. However true this may be as a point of Kuhn exegesis, the response fails to get at the nub of Lakatos's complaint, which can only be understood once we notice that his accusation was originally made in the context of an attack on the positivists (whom Lakatos calls "justificationists"). For a point that unites Kuhn and the positivists is their unwillingness to associate radical criticism, or a choice between alternative normative systems, with anything they would be willing to call "rational." For them, rationality is in the act of rule-following, not in the act of rule-making or rule-breaking. Lakatos understood well the significance of this distinction, which corresponds, respectively, to the evaluative and the prescriptive sides of the normative enterprise.\textsuperscript{31} As philosophy's command over the course of knowledge production has receded in the face of academic specialization, philosophy's normative posture has shifted from prescription to evaluation, from legislator to judge, or still more modestly, to accountant, a keeper of someone else's books.\textsuperscript{32} Victim to a febrile intellect and an early death,
Lakatos never managed to stem this tide, though an interest in reasserting prescriptivism in the philosophy of science might ultimately explain his distinctive doctrines concerning the history of science.\textsuperscript{33}

The idea that Kuhn helped \textit{diminish} the normative dimension of the philosophy of science merits some attention, since the first thing one usually says about Kuhn is that he undermined ("once and for all") the observation-theory, fact-value, empirical-normative, descriptive-prescriptive distinctions from accounts of science. Whatever else the latter term in each binary may mean, Kuhn (on behalf of a litany of historicists) is said to have shown that it "loads" or "impregnates" the former term. Doesn't such imagery suggest the \textit{ascent} of the normative? Here we need to dig a bit deeper, for long before they spawned opponents, the positivists had themselves already discovered that observations are theory-impregnated (in the sense that observational content is deducible from theoretical premises in a formal scientific language) and that facts are value-laden (at least in the elliptical sense that the theoretical language one uses for inquiry involves a free value choice).\textsuperscript{34} Moreover, the positivists and the anti-positivists agreed on a generally instrumental view of scientific language, one which portrayed theories as rules for governing the conduct of inquiry. The key difference between the positivists and their opponents lay in the attitude they took toward these common elements. Whereas the positivists believed that theory- or value-ladenness threatened the objectivity of inquiry, unless explicitly circumscribed by an account of "testability," anti-positivists tended to treat the uncertain objectivity of inquiry as a brute fact that could only be mitigated but never completely eliminated. For his part, Kuhn masks this uncertainty by taking responsibility for the choice between scientific languages out of the hands of self-conscious deliberators and placing it in the "invisible hand" of the Planck Effect. In so doing, Kuhn effectively reintroduces a sharp distinction between the "empirical" and "normative" questions of science in terms of a difference between the determinable present (normal science) and an indeterminate future (revolutionary science). This move made it that much harder for philosophers like Lakatos to motivate, let alone execute, a theory of "rationality" suitable for prescribing the course of inquiry.

In terms of the eighteenth- ("Enlightenment") and nineteenth- ("Positivist") century conceptions of reason sketched above, Kuhn's account can be seen as having worked in the following way to marginalize a strong sense of prescriptivism in the philosophy of science:

\textsuperscript{33} Certainly, Lakatos's most notorious contribution to philosophy has been his bald-faced endorsement of rational reconstructions of the history of science, however much they may deviate from the actual historical record. Whatever else one may wish to say about such reconstructions, they offer a sense of how the history would have gone, had it unfolded according to the philosopher's favorite rules. If philosophers can no longer prescribe the course of knowledge production, the next best thing may be to fantasize how much better it would have been had they been allowed to do so!

\textsuperscript{34} What are "value-free" and "theory-neutral" are the possible observations that deductively follow from the chosen theoretical language, and their testability by a method whose validity does not depend on whether the tested theory is true or false.
(1) Through the Enlightenment spectacles with which the Popperians looked at Kuhn, Kuhn is an irrationalist because he valorizes the unreflective practices of normal science over the reflective ones of revolutionary science. Popperians see Kuhn as simply a traditionalist.

(2) However, "normal science" is not quite the old idea of "tradition" in a new guise. Traditions aim to preserve the past in the present, but normal science exhibits cumulative growth. Traditional are the practices in which normal scientists engage; not traditional are the puzzle-solving domains to which those practices are applied. Here Kuhn trades on the positivist conception of reason, which predicates steady progress issuing from a firm epistemic base.

(3) In this way, reason is successfully contained, or relativized, to a paradigm, but inexpressible between paradigms. Thus, there is no place for a critical form of rationality that is independent from, let alone oppositional to, the dominant epistemic practices of the time.35

To illustrate the sort of rationality that drops out of Kuhn's picture of science, consider some apt analogies from the history of capitalist economics. Corresponding to the Enlightenment conception of reason is the entrepreneur, whose innovative spirit continually widens the sphere of production by breaking up cottage industries and guild mentalities. Corresponding to the Positivist conception of reason is the manufacturer whose factory outproduces all competitors. Whereas the enemy of entrepreneurs - their vision of the irrational - is the sub-optimally productive user of resources (say, one of Marx's rentiers), the enemy of manufacturers is the destroyer of resources (say, a Luddite attacking the machines in their factory). The "Kuhnian" economist is essentially a friend to the manufacturer but an enemy to the entrepreneur. However, without a supply of entrepreneurs periodically upsetting existing trade patterns, engaging in "creative destruction" (in Schumpeter's inspired terms), capitalism would itself soon become just another scheme for perpetuating inherited wealth, only this time wealth based on manufacture instead of agriculture. In that case, if one is not a member of a major industrial family, and emigration is not a possibility, then violent overthrow of the system would seem to be the only means available for the disenfranchised to acquire wealth. This is where a "Kuhnian" economics would leave us - a place not so far from where entrepreneurship's leading theorist, Joseph Schumpeter, thought capitalism was already heading. However, Schumpeter had issued a proviso, one which may also be applicable to the fate of science in Kuhn's account. Schumpeter believed that violent overthrow was no longer a real possibility, given the risk to the relatively high standard of living that even the disenfranchised (supposedly) enjoyed in mature capitalist economies. Rather, entrepreneurship would itself probably be routinized, as

35. Thus, while one can understand Sandra Harding's reliance on Kuhn to refute positivism, Kuhn's work itself provides little comfort to her own "standpoint epistemology," which implies a privileged critical perspective for groups marginalized from the dominant forms of power in society. See Harding, The Science Question in Feminism (Ithaca, 1986), 197-210.
innovation became one more thing regulated by a state planning board. A similar story has been told about the diminished prospects for scientific revolutions in the future, given the heavy personnel and material investments that have already been made in current forms of "Big Science." The question that needs to be asked of these gloomy forecasts is whether they are anything more than an artifact of an account of science that does not recognize a place for rational criticism outside of the existing epistemic power structure.

Although it would go against the "Being There" thesis to conclude that Kuhn was trying to foist an irrationalist image upon radical criticism, Structure nevertheless does seem to have had this effect on its readers, most noticeably on Paul Feyerabend, who openly converted to irrationalism in the 1970s, reveling in the "anarchy" that would allegedly be produced in a world without a uniform scientific method. In a sense, Feyerabend merely acquiesced to Kuhn's vision of what Popper's call for a "permanent revolution" in science would look like—a vision that was, to be sure, shared by the positivists, who always believed that Popper was courting cognitive chaos by eschewing definitions in philosophy and advising that scientists suspect all their knowledge claims as presumptions whose erroneous bases have yet to be discovered. The difference between Popper and Feyerabend is, of course, Kuhn. Whereas Popper saw himself as subjecting the ends and means of inquiry to the same critical standards as the products of inquiry, Feyerabend takes Popper to be offering a politically naive strategy for wresting the course of inquiry from the Big Science powermongers. As Feyerabend might put it, the problem is not that the powermongers won't

36. The locus classicus of this view is Joseph Schumpeter, Capitalism, Socialism, and Democracy (New York, 1942).

37. A telling point here is that, in recent years, with the exception of Feyerabend, no philosophical attention has been lavished on the idea that science should do something other than what it already is. Since Kuhn's work, philosophers of science have been in virtually complete agreement about which instances of historical and contemporary science deserve praise or blame. Nowadays the debates turn on the best way to account for the agreed upon cases. See, for example, Scientific Realism, ed. Jarrett Leplin (Berkeley, 1984). Contrary to received wisdom, Structure did not so much "refute" the concept of progress in science as to change the subject to one in which the philosophers disagreed only over the finer points of semantics. For a sophisticated account justifying the slowdown of science, influenced in part by Kuhn, see Nicholas Rescher, The Limits of Science (Berkeley, 1984).

38. One brave attempt to lay down an external rational critique of Big Science is Harry Redner, The Ends of Science (Boulder, Colo., 1987), esp. 252–253 (a severe critique of Kuhn). As Sujatha Raman has pointed out to me, an emerging model for rationally criticizing Big Science from outside the epistemic power structure is post-Cold War "downsizing" and "conversion" of the U.S. military establishment, which is currently (albeit painfully) being negotiated by career soldiers and civilian bureaucrats. Here, too, one finds the rhetoric of "great risk" that supposedly attends any attempt to alter radically the institution's scale and scope.

39. Several philosophers have noticed that Feyerabend presupposes the positivists' rather limited conception of rationality so that he appears more outrageous than he really is. See Arne Naess, "Paul Feyerabend—A Green Hero?", ed. Gonzalo Munevar, Beyond Reason: Essays on the Philosophy of Paul K. Feyerabend (Dordrecht, 1991), 403–416. It is worth recalling that, of all the Popperians, Feyerabend was the only one whose early reputation was built as a technical critic of positivism, which suggests that he may have ingested more positivist dogma than he and his readers have realized. See note 16.
listen to reason, but that they listen all too well! Notice here that reason is presumed to be always “internal” to science. The only politically appropriate responses to Big Science, then, are forced entries, withdrawals of support, and displays of irreverence—all, in one way or other, violent departures from reason.\textsuperscript{40}

But why must radical change occur radically? In other words, why is reason specifically precluded from being a vehicle by which a radical departure can be made in the course of one’s inquiries? When Popper remarked that the wonderful thing about humanity’s place in evolution was that our theories could die in our stead, he was alluding precisely to this missing sensibility, that risking an idea is not the same as risking a life. To portray scientific revolutions in apocalyptic terms—as Feyerabend does for fun and defenders of science do in fear—is to overlook the potentially invisible character of such revolutions. My choice of terms here is quite deliberate, as “The Invisibility of Revolutions” is the title of chapter eleven of \textit{Structure}. But Kuhn takes the relevant sense of “invisibility” to involve the following claims: (1) that the outcome of a revolution is determined not by clashing parties coming to agreement, but by the research choices subsequently made by their students; (2) that the revolutionary character of paradigm change is obscured by subsequent textbook writers who make the transition appear continuous. I will consider (2) below, but let me focus now on (1), the Planck Effect, which implies that argumentation over the scientific research agenda benefits the spectators more than the participants. In one sense, Kuhn’s point is undeniable, insofar as the champions of one research agenda rarely take themselves to be swayed by the arguments of their opponents. However, to leave the matter at that is to ignore the ways in which partisan positions shift, often unintentionally and imperceptibly, in the course of debate, as the stakes and implications of acceding to one argument over another appear in different contexts. A position that one would never have adopted at the start of a dispute may become easier to accept later, in large part because the very practice of arguing will have made one accustomed to the other’s position. Moreover, the person may not believe that he or she has conceded anything “essential” to her position along the way. Only in retrospect can a historian detect that a subtle shift in the burden of proof took place that enabled the acceptance of a previously intolerable point of view.\textsuperscript{41} Thus, radical change can

\textsuperscript{40} Dominique Lecourt, philosophical advocate of “proletarian science” during the French student uprising of 1968, has recently bemoaned the fact that inquiries into the ends of science have become monopolized by critics strongly influenced by the right-wing irrationalism of Spengler and Heidegger. Even when the critics have had left-wing pretensions, as in the case of the Green movement, they suppose that science can only be opposed by engaging in practices contrary to those of science. Feyerabend has certainly contributed to this sentiment in Europe, which goes against the self-critical rationalism of both Popperians and Marxists, despite their disagreement on so many other matters. However, as Lecourt rightly observes, these rationalists have taken today’s “technoscience” as having greater facticity than “nature” itself. See Lecourt, “The Scientist and the Citizen: A Critique of Technoscience,” \textit{Philosophical Forum} 23 (1992), 174–178.

\textsuperscript{41} I develop this point in Fuller, \textit{Social Epistemology}, 99–116; and in Steve Fuller, \textit{Philosophy of Science and Its Discontents} (Boulder, Colo., 1989), 65–70.
occur quite unradically, indeed, invisibly. However, conspicuously absent from Kuhn's account is any discussion of how argumentation may facilitate this transition. The absence is "conspicuous" because the other historicists in Shapere's original cohort—Hanson, Toulmin, and Feyerabend—explicitly made argumentation central to an understanding of how scientific change occurs.\(^{42}\) Despite their differing emphases on this process, they made a point of distancing what they mean by "argument" from formal logical deduction, in order to convey the idea that the merits of alternative research agendas, or theories, are not already implicit in their ideal formulations, just waiting to be deduced; rather the theories' respective merits do not become evident until they are articulated publicly in terms of one another.\(^{43}\) Of course, there are all too many instances in the history of science when the relevant rivals have not directly confronted one another, and so their incommensurability remained until the Planck Effect kicked in. However, \textit{a priori} there is no reason to take this state of affairs as incorrigible. But was an alternative normative account available in the 1960s? The answer is that crucial elements were available, but they were not brought together into a coherent and persuasive whole. The key lies in following the career of \textit{rhetoric} during this period; here our main witness is Toulmin.

In the early 1960s, Toulmin was known not only, and perhaps not primarily, as a philosopher of science but as a moral theorist and philosophical logician. His youthful work, \textit{The Place of Reason in Ethics}, had successfully challenged the positivist dictum that value issues were emotively based attitudes—"mere ejaculations," in A. J. Ayer's words—not susceptible to rational argument.\(^{44}\) In place of the sharp dualism between scientific rationality and moral rhetoric, Toulmin proposed that both discourses were "rational" and "rhetorical" in exactly the same way. In practice, this meant showing that ethics is an inquiry into what is \textit{really} good, which signaled that reasons could be mustered and contested in the same way as in science. However, it also meant importing a conception of scientific reasoning that had a strongly rhetorical character, one

\(^{42}\) Credit for first realizing this point about Kuhn goes to Israel Scheffler, \textit{Science and Subjectivity} (Indianapolis, 1967).

\(^{43}\) Unlike the positivists, who portrayed scientific theories as generating predictions that are then tested against observations, both Toulmin and Hanson describe scientists more like judges who look for the rule (theory) under which the case at hand (observation) can be best subsumed. This reflects a shift in philosophical thinking from confirmation to explanation as the main business of science. Philosophers of science, then, presumably are like the legislators who construct (discover?) the rules by which the contesting interpretations of the case can be resolved. For example, in \textit{Patterns of Discovery} Hanson follows a strategy, familiar in constitutional law, of eliciting legislative principles from the conflict that arises from judges deciding "hard cases"—in Hanson's case, concerning the motion of elementary particles. As legal theorists are wont to argue, Hanson too claims that if the principles he has elicited apply for this hard case, then \textit{a fortiori} they apply to the more routine cases of scientific discovery as well. See Hanson, \textit{Patterns}, 2–3. By contrast, the positivists focused on the logic of these more routine cases, which are essentially generated as observations by the dominant scientific theory. Kuhn stands with the positivists on seeing theory-driven research as mostly "mopping up operations," but occasionally turning up Hanson-like phenomena, which are best seen as "anomalies."

\(^{44}\) Stephen Toulmin, \textit{The Place of Reason in Ethics} (Cambridge, Eng., 1950).
in which "reality" defines a pragmatic limit to a particular inquiry, the moment when its participants no longer feel the need to pursue a matter any further. Of course, the inquiry can later be reopened, with "reality" then being the subsequent point of closure. Indeed, science has institutional mechanisms to ensure that that happens.\textsuperscript{45} Ethics, Toulmin observed, is not so endowed, but that is only a matter of convention, one perhaps worth revising so that people are not led to think—as the positivist view might suggest—that important value issues are purely personal matters. Toulmin made more explicit his endorsement of a rhetorically based, anti-positivist conception of reason in \textit{The Uses of Argument}, a book that started to become very influential among Speech Communication scholars in the late 1960s.\textsuperscript{46} However, when it was first published in 1958, Toulmin's wholesale rejection of the concept of formal validity—the idea that there are forms of argument that are rationally acceptable regardless of their content—made the book very unpopular with its primary audience, philosophers, even among those who recognized that formal logic provided an inadequate model of human reasoning.\textsuperscript{47}

The point of this brief excursus into Toulmin's early thought is that, unlike Kuhn and the positivists, Toulmin never drew a sharp line between reasoning about empirical and normative matters. In fact, he was so keen on eliminating any such distinction that he challenged its ultimate philosophical basis, the Humean contrast between empirical content and logical form. However, Toulmin never worked out an alternative conception of scientific reasoning. True, he actually predated Kuhn's use of "paradigm" by one year, using the word to mean an "ideal of natural order" in terms of which the adequacy of particular explanations would be judged. So defined, a paradigm anchored scientific argument by establishing a presumption, say in the case of Newtonian mechanics, in favor of bodies moving rectilinearly unless impeded.\textsuperscript{48} Clearly, Toulmin here was groping toward a theory of rhetoric, with his use of "paradigm" recognizably falling under the rhetorical category of \textit{topos}.\textsuperscript{49} However, crucially lacking from Toulmin's account was an understanding of how radical

\textsuperscript{45} Toulmin would later call practices of this sort "compact." See \textit{Human Understanding}, 378ff. Toulmin's discussion of compactness has influenced the interest that Sociologists of Scientific Knowledge have had with how scientific disputes are "closed" and then "reopened." See Harry Collins and Trevor Pinch, \textit{Frames of Meaning} (London, 1982); Harry Collins, \textit{Changing Order} (London, 1985).

\textsuperscript{46} Stephen Toulmin, \textit{The Uses of Argument} (Cambridge, Eng., 1958). The recent spate of interest in Toulmin among German philosophers is due to the incorporation of Toulmin's ideas about validity in the theory of communicative action put forth by Jürgen Habermas, who, in turn, learned of Toulmin's work from the Speech Communication people. The canonical status of Toulmin's work in this field is symbolized by a place in \textit{Handbook of Argumentation Theory}, ed. F. van Eemeren, R. Grootendorst, and T. Krüger (Amsterdam, 1987), 162-207.

\textsuperscript{47} See, especially, Peter Manicas, "On Toulmin's Contribution to Logic and Argumentation," \textit{Journal of the American Forensic Association} 3 (1966), 83-94.

\textsuperscript{48} Toulmin, \textit{Foresight}, 56.

\textsuperscript{49} The \textit{topoi} surrounding the breakdown of a paradigm have been explored more recently in Kenneth Zagacki and William Keith, "Rhetoric, Topoi, and Scientific Revolution," \textit{Philosophy and Rhetoric} 25 (1992), 59-78.
change could arise in the course of argument: How can people operating with incomensurable paradigms come to see one another as mutually relevant audiences worthy of sustained attention and perhaps even mutual incorporation? Toulmin failed to answer this difficult question because he underestimated the need for arguers (including himself, as aspiring revolutionary) to build ethos with their audiences. There are no captive audiences in science. If the Planck Effect is as prevalent as Kuhn maintained, then obviously scientists can never take for granted that colleagues holding contrary views will listen to them. Traditionally, the function of ethos in rhetoric has been to build community from audiences with little, if any, prior common cause. The need for the different classes of a society to join forces in times of war would thus be a typical test of a leader's ethos. While this aspect of rhetoric would gain renewed prominence in the 1970s, with the reissuance of the works of Kenneth Burke, it was quite the opposite of what philosophers especially, but even some rhetoricians, had only a decade earlier taken the scope of rhetoric to be.

In reducing the voice of reason to that of a scientific insider, Structure managed to satisfy an aspiration left unfulfilled by the original logical positivists. The key was that Kuhn built change into the very logic of science. Here Kuhn had to revert to a crypto-Aristotelian distinction between "natural" and "violent" motion, as captured by the normal/revolutionary science contrast. This point

50. The question of leadership is especially acute in any philosophical attempt to revive a prescriptivist approach to science. One traditional rhetorical source of ethos is kairos, that is, a pressing occasion that calls for leadership. But what can be made so pressing that scientists listen to a philosopher—even if all the philosopher says is that they should listen to each other? Unfortunately, the prospect of philosophical legislation continues to carry the old Hobbesian baggage of a sovereign legislator who not only takes the initiative to propose laws but also has the will to enforce them.

51. Among noted rhetoricians, for example, I. A. Richards saw the academic study of rhetoric as a fellow-traveler of logical positivism, in its ability to identify and correct misunderstandings. See Richards, The Philosophy of Rhetoric (Oxford, 1936).

On matters of rhetoric in science, it might seem appropriate to return to Feyerabend's, whose study of Galileo's rhetoric is the case in point for the anarchistic epistemology advanced in Against Method. However, while Feyerabend grants rhetoric a more prominent place in exemplary episodes of scientific reasoning than the positivists were inclined to recognize, he nevertheless continues to portray rhetoric in the same negative light as the positivists saw it. After all, on Feyerabend's account, Galileo used rhetoric to cover up basic gaps in his arguments, which over the centuries philosophers have supplemented with idealized premises; however, these gaps did not escape the gaze of Galileo's Jesuit Inquisitors. The outrageousness of Feyerabend's account pertains to the short and long term of Galileo's fate. In the short term, the Inquisitors, normally the "bad guys" in positivist historiography, are presented as having offered cogent methodological criticisms of Galileo (much like the positivists themselves would). Yet, in the long term, Galileo turns out to have gotten closer to our physics than the Inquisitors with their methodological niceties. The Feyerabendian moral, then, is that the positivists mistake rhetoric for method, but method doesn't win, either: reality does—and that's something that transcends both crafty rhetoric and rigorous method. If this conclusion seems to mix truth and long-term survival, it is because Feyerabend shares with Kuhn a certain unexplained fondness for Darwinian accounts of knowledge growth.

52. Kuhn's likely source for an Aristotelian perspective on scientific change was Alfred North Whitehead, Science and the Modern World (New York, 1925), a popularization of process metaphysics that enabled Whitehead to portray twentieth-century revolutions in physics as a neo-Aristotelian revival.
informs the necessarily “Orwellian” (Kuhn’s word) role that he assigns to the rewriting of history, one which erases any trace of a revolution from science’s institutional memory.\(^{53}\) The political implications of this picture are clear: Scientists govern themselves adequately under normal circumstances, directed by science’s self-defined ends, but when a paradigm breaks down under the weight of its own anomalies, “external,” “non-scientific” intervention is required. On those occasions, and only then, larger political considerations play a formative role. Because Kuhn was convinced that science flourishes only in its sheltered normal phases, he wanted to dampen any long-term effects that these considerations might have, which explains his keenness to prevent scientists who work in the next paradigm from learning about their traumatic political origins. Contrary to the drift of much Kuhn commentary, Kuhn does not object to the rational reconstruction of history—the story of how science should have happened—that philosophers like Lakatos have promoted. After all, this is the stuff of which Orwellian history is made. What is objectionable, however, is for scientists to be told (as philosophers are wont to do) that such reconstructions deviate from the actual historical record, as that could inject a measure of critical self-consciousness that could jeopardize the puzzle-solving process. Thus, Kuhn would clearly give an affirmative answer to the provocative question posed by Stephen Brush: Should the history of science be rated “X” for scientists?

The transitory and suppressible character of the political in Kuhn’s account has had a decisive impact on the development of the Sociology of Scientific Knowledge. In particular, three familiar sociological perspectives simply have no place in Kuhn’s account of science: (1) the view that science systematically reflects, or legitimates, specific class interests more generally represented in society, such that major changes in science are best explained by appealing to larger societal developments; (2) the view that science lacks any clear integrity as a social practice, but is rather a loose collection of pre-existing practices that are held together by a specifically “scientific” way of talking; (3) the view that science can be exhaustively reduced to a set of variables, drawn from the larger repertoire of social science, on the basis of which policy for science can be made as part of comprehensive social policy.

Until very recently and then only tentatively, these three perspectives have been excluded from the Sociology of Scientific Knowledge, even as SSK has come to dominate the sociological study of science.\(^{54}\) For example, SSK typically


\(^{54}\) Three recent textbooks that attempt to integrate SSK concerns with larger issues of critical science policy, often within a broader sociological or political economy perspective are: Stephen Yearley, \textit{Science, Technology, and Social Change} (London, 1988); Andrew Webster, \textit{Science, Technology, and Society} (London, 1991); John Street, \textit{The Politics of Technology} (London, 1992). Note that all the authors are British and draw their examples from the U.K. and Europe, which have independent traditions of science criticism, deriving mostly from Marxism and the Ecology movement. In all three books, SSK is presented in separate chapters from those devoted to science criticism and policy, and often the attempt at integration is perfunctory. For an astute critique of Yearley, whose own research most clearly straddles the divide, see Govindan Parayil, “Review of Yearley,” \textit{Social Epistemology} 6 (1992), 57–64.
provides what might be called an "internal sociology" of normal science, in which the SSK practitioner tries to understand the set of practices that constitute what the scientists themselves identify as their lifeworld, a laboratory being the typical site for SSK inquiry. However, SSK practitioners suspend any technical knowledge that they might have of the practices under study. The subversive epistemological consequence, already noted, is that the SSK practitioner often notices things, especially incongruities between word and deed, that escape the practicing scientist's attention. But the subversion only goes so far, as SSK's own critical potential is truncated by a symmetrical tendency to suspend any technical knowledge of the social sciences that SSK practitioners might bring to bear on their case. While such methodological asceticism is not without precedent in the sociological literature, it has had the effect of precluding political factors that do not explicitly enter the scientists' own deliberations.55 For example, SSK practitioners employ discourse-analytic techniques to reveal the various voices in a language game played by a community of scientists, but they do not use such techniques to engage in an ideology critique of science that appeals to factors that sustain the game but which transcend the scientists' control or awareness. This stunted sense of critique has been a constant source of frustration for Marxist and feminist science critics, whose relations with SSK have been chilly, to say the least.56

One ready source of friction between SSK practitioners and these more radical critics of science is the Kuhnian legacy of treating the study of science as a relatively self-contained project, innocent of any obvious interest in providing a comprehensive theory of society. If anything, Kuhn's tendency to see science from a distinctly pedagogical angle has caused him and his SSK followers to stress acculturation at the expense of every other social process.57 In fact, given Kuhn's efforts at minimizing the presence of disagreement, or division of any sort, within a paradigm, he runs perilously close to what sociologist Dennis Wrong would call an "oversocialized" conception of the scientist.58 Telling in


57. Although Kuhn's pedagogical orientation is due primarily to his experience in the Harvard General Education in Science curriculum, as informed through the lectures of Jerome Bruner, he was also influenced by Wittgenstein and Popper, both of whom were trained as secondary school teachers, an aspect of their lives that carried over to their philosophical practice. For a penetrating analysis that should perhaps be read alongside the one I provide below, see W. W. Bartley III, "Theory of Language and Philosophy of Science as Instruments of Educational Reform: Wittgenstein and Popper as Austrian Schoolteachers," in Boston Studies in the Philosophy of Science XIV, ed. R. Cohen and M. Wartofsky (Dordrecht, 1974), 307–337.

58. An oversocialized conception of people sees individuals entirely in terms of their group characteristics, as in the Kuhnian tendency to see all scientists who work in a given paradigm as sharing the same mindset. In effect, this conception collapses the micro-macro distinction in sociological theory by making it seem that an entire community can be studied simply by regarding one of its members. Not surprisingly, oversocializationists tend to regard acculturation—the acqui-
this regard are the tasks that Kuhn sets for sociological inquiry in the postscript to *Structure*. He concludes the agenda as follows:

What does the group collectively see as its goals; what deviations, individual or collective, will it tolerate; and how does it control the impermissible aberration? A fuller understanding of science will depend on answers to other sorts of questions as well, but there is no area in which work is so badly needed. Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it.59

The urgency with which Kuhn recommends the study of science as a unique and autonomous enterprise prompted Feyerabend to wonder whether Kuhn was not implicitly smuggling a prescription under his description: Should Kuhn be taken to mean that science *ought* to be maintained in its current state?60 Kuhn responded, Aristotle-like, that by describing the implicit normative structure of science, we would acquire a better sense of how best to govern science.61 While some have taken Kuhn to have thereby thumbed his nose at the is–ought distinction, I take him to have simply confirmed Feyerabend's suspicion—that Kuhn wants to understand science in order to determine what it will take to maintain its *status quo*.62

A key to fathoming the curious role that the social dimension of science plays in *Structure* is David Hollinger's recent argument that Kuhn managed to "discover" that science flourishes in self-governing communities just at the time that democratic societies insist on greater public accountability from scientists.63 As in so many other cases, a sense of "community" emerges among disparate individuals as soon as they face a common foe.64 To put the point in boldest relief (probably bolder than Hollinger would permit), the construction of science as social served much the same role as earlier constructions of the scientist as individual—only now reflecting the potentially more widespread societal opposition that scientists faced. In both cases, the uniqueness of science is highlighted as demanding special treatment. Kuhn stands out for having published a book that left no clear political trace, one that cloaked a desire for science to be left to the scientists in the guise of an empirically informed theory of the nature of scientific inquiry. Thus, Kuhn succeeded where even autonomous science's most
eloquent champion, Michael Polanyi, had failed. Despite the persuasive case he made for the existence of a "scientific community" (a term of his coinage) and the admiring audiences he secured for penetrating the "tacit dimension" of scientific practice, Polanyi typecast himself as upholding the culture of science in the face of barbarous bureaucrats and creeping socialists. Participation in the quotidian battles that science fought in the public arena did not permit Polanyi the sort of detached "scientific" rhetoric that characterizes most of the writing in Structure, a point in no small measure responsible for the book's longevity.

Still, it is not hard to see that Kuhn owed more to Polanyi than the couple of footnotes to Personal Knowledge would suggest. Consider the following:

If authority alone, and particularly if non-professional authority, were the arbiter of paradigm debates, the outcome of those debates might still be revolution, but it would not be scientific revolution. The very existence of science depends upon vesting the power to choose between paradigms in the members of a special kind of community. Just how special that community must be if science is to survive and grow may be indicated by the very tenuousness of humanity's hold on the scientific enterprise.

As Kuhn proceeds to discuss the special character of the scientific community, it becomes clear that he trades on the Polanyiesque trope of converting cognitive virtues to moral ones, qualities of understanding to qualities of trust. Thus, in place of scientists holding beliefs on the basis of defeasible evidence, Kuhn finds scientists committed to a vision of reality on the basis of intuitive judgment. The sort of radical criticism that is the philosopher's stock-in-trade appears, in the Kuhn-Polanyi lexicon, as a failure to respect the difference between one's own social station and that of another inquirer. Thus, if Lakatos's charge of "mob psychology" offends, it is because it challenges the soundness of trusting expert judgment during times of disagreement over the relevant standards of judgment. Of course, Polanyi did not believe that all experts deserved to have their discretion so respected. In particular, he believed that social scientists


67. A leading SSK practitioner, Harry Collins, sides with Polanyi over Lakatos, in a way which reveals the conservative implications that can be drawn from the day-to-day openness of scientific inquiry: "Even among the experts themselves, who have been trained to many levels above what can be expected of the public's understanding, radically different opinions are to be found... It is dangerously misleading to pretend that the citizen can judge between the competing views of technical experts when even the experts cannot agree" (Collins, "Certainty and the Public Understanding of Science: Science on Television," Social Studies of Science 17 [1987], 691). Ironically, then, while sociologists can step into the breach when philosophers cannot decide among themselves which methodology best explains a certain historical episode of scientific theory choice, the sociologists' lack of scientific expertise prevents them from intervening in contemporary disputes among scientists trying to resolve their own theory choices! The underdetermination of theory choice by the evidence thus licenses, for the SSK practitioner, not the introduction of specifically sociological variables, but rather the discretionary judgments of the local experts.
could not be left to their own devices because of their tendency to extend some philosophical caricature of scientific practice—a "methodology"—to aspects of everyday life where it played no constructive role and, indeed, where it could be quite destructive if used to challenge established scientific judgment. 68

The elitist undercurrent in Polanyi's account of science is unmistakable, especially the unflattering analogy he frequently drew between, on the one hand, medieval self-governing guilds and indentured servants and, on the other, basic and applied researchers. (He seemed to think that all social scientists could be fitted into the latter category.) However, that undercurrent can also be channeled into a conception of democracy that has become increasingly prominent in the twentieth century, namely, pluralism. Pluralism emerged as an alternative to the pseudo-democracy of self-styled socialist regimes, whose idea of "equality under the law" was equal subordination to a central authority. The pluralist aims to diffuse power from a central authority to voluntary associations, which, under ideal conditions, are sufficiently divided in their labors that the members of any such association must rely regularly on the expertise of the members of other such associations. 69 Explicit rules of law play less of a governing role in this system than relations of trust born of mutual dependency. Pluralists see themselves as presenting a version of the "open society" suitable to democracies much larger and more heterogeneous than those modeled on the Athenian polis. Instead of questioning authority that strays beyond its bounds, as Popper might

68. During the 1950s and 1960s, Polanyi was not alone in thinking that the authority of the natural sciences was threatened by the social sciences. Indeed, Vannevar Bush wanted the U.S. government to fund a "National Science Foundation" that specifically excluded the social sciences, not out of a philosophical sense that the social sciences were "unscientific," but as a result of private foundations becoming reluctant to fund natural science research after its involvement in the atomic bomb project. Moreover, Bush had to fight off the impression created in the minds of several senators, notably Harley Kilgore, that the social sciences were needed as watchdogs to ensure that natural scientists served the public interest. See Nathan Reingold, Science American Style (New Brunswick, N.J., 1991), 284–333. Aside from the atomic bomb, the public image of the natural sciences was tarnished by the attempts of leaders of the scientific community to suppress without testing Immanuel Velikovsky's best-selling Worlds in Collision, which challenged the received wisdom of physics and chemistry by psychoanalyzing the creation myths of several cultures. When it turned out that some of Velikovsky's astronomical predictions were correct, several social scientists took the opportunity to chastise natural scientists for failing to live up to their own avowed standards. This episode simply led Polanyi to retreat his own position about the necessarily inexplicit character of true scientific expertise. See Polanyi, "The Growth of Science."

69. There are a plurality of pluralisms, divided mainly according to the degree of conflict between the interest groups, or voluntary associations, in the society. A good survey is David Held, Models of Democracy (Stanford, 1987), 186–220. The model emphasized here is relatively peaceful, drawing on a coordinated division of labor, based on knowledge being distributed across the members of the society. This is the sort of polity that SSK seems to countenance. A genealogy of the position can be traced from Adi Ophir and Steven Shapin, "The Place of Knowledge: A Methodological Survey," Science in Context 4 (1991), 3–21, through Peter Berger and Thomas Luckmann, The Social Construction of Reality (Garden City, N.Y., 1967), to Alfred Schutz, [1932] "The Well-Informed Citizen: An Essay in the Social Distribution of Knowledge," in Collected Papers (The Hague, 1964), II, 120–134. Schutz, in turn, wanted to provide a phenomenological basis for Austrian economics as part of the microfoundations for unifying the social sciences. The pluralist polity would thus be a minimal state. See Christopher Prendergast, "Alfred Schutz and the Austrian School of Economics," American Journal of Sociology 92 (1986), 1–26.
advise, pluralists advocate tolerating authority that stays within its bounds. This is a subtle, but significant, shift in the sense of "openness" required of the open society in a world of, so to speak, Big Democracy. In the case of science, it marks a shift away from criticizing products to understanding practices. Kuhn participated in educating Americans to this new sense of "democratic control" of science during Structure's fifteen-year incubation period (1947–1962), during much of which he taught in the General Education in Science curriculum designed by Harvard President James Bryant Conant, who wrote the foreword to Kuhn's first book and to whom Structure is dedicated.70

Conant's vision of post-World War II America was one in which scientific research would be making greater demands on the public coffers as the pace of progress quickened. As the pace picked up, there would be greater disagreement among scientific experts about the path that science should take. Consequently, the public (and Conant typically had in mind the typical Harvard-trained corporate executive or government official) needs to become "expert in judging experts."71 Here, in his characteristically no-nonsense manner, is how Conant put the matter:

Whether we like it or not, we are all immersed in an age in which the products of scientific inquiries confront us at every turn. We may hate them, shudder at the thought of them, embrace them when they bring relief from pain or snatch from death a person whom we love, but the one thing no one can do is banish them. Therefore every American citizen in the second half of this century would be well advised to try to understand both science and the scientists as best he can.72

Conant's key presumption that survives in Kuhn and, as we shall see shortly, in SSK is that the bridge between science and society is best constructed by the public growing more accustomed to science—rather than by scientists growing more accustomed to the public.73

Conant's vision was operationalized by the chemist Leonard Nash, with

73. I do not mean to deny that Conant was concerned that scientists be properly "socialized." However, he responded by offering courses that exposed scientists to the old liberal arts culture which dominated the elite colleges prior to World War II. See Richard Freeland, Academia's Golden Age: Universities in Massachusetts, 1945–1970 (Oxford, 1992), 79.

It would be unfair to imply that all of the outreach work inspired by SSK has tended to reinforce an uncritically positive image of science. For example, a series of booklets published by Deakin University Press are designed to enable Australian citizens to interrogate the role of science and technology in their lives, often by drawing on the critical questions asked by philosophers and sociologists of science in their inquiries. Especially noteworthy is W. Randall Albury, The Politics of Objectivity (Victoria, 1983).
whom Kuhn co-taught "Natural Sciences 4," and to whom Kuhn dedicated his first book:

The goal of our course [is] the development of a healthy and informed attitude toward, an appreciation and understanding of, science... In discussing the earlier and the more recent activities of science it is carefully pointed out to our students that the urgency of the desire for the solution of a particular scientific problem constitutes no guarantee that it will be or even can be solved... That great strides will be made in science cannot be doubted, but that they will lead immediately to a desired end... can be legitimately questioned. This unbounded optimism can be tempered by the suggestion that unlimited scientific progress in a specified direction cannot be bought or otherwise induced. Favorable conditions for an advance can be created, and the advance may be accelerated; but the advance itself cannot be created.74

It should come as no surprise that when Nash goes on to diagnose "two delusions" that cause students to ask of science more than it can reasonably give, he arrives at suspiciously philosophical disorders: "the cult of the fact" and "the cult of the method." The remedy that Nash proposed as "the case approach" involves presenting students with carefully edited versions of classic experimentalist writings that will enable them at least to follow, and perhaps even to re-enact, the experiment in question.75 Using a line of argument that Kuhn would later adopt in explaining his Gestalt-based notion of paradigm, Nash defended the suitability of the case approach on the grounds that to understand science is to understand how patterns can be extracted from ambiguous data, a cognitive process that has basically remained unchanged since the rise of experimentation.76 Thus, Nash had no qualms about selecting experiments only from before the mid-nineteenth century. Indeed, such experiments had the additional advantage that their accompanying texts presupposed relatively little esoteric background knowledge. Even so, the amount of pedagogical guidance involved in this process — "preparation of the materials," in Nash's own clinical terms — must have been considerable, as Nash anticipates and meets charges of "spoon-feeding" the students.77

A comparison of the original texts with Nash's "prepared" versions reveals that left on the cutting-room floor was any trace of the social production of knowledge, especially as expressed in appeals to disciplinary audiences and other potential readers. Of course, such excisions become more difficult to

75. Although nowadays an appeal to "cases" in studying science suggests an approach that draws, at least by analogy, on ethnographic field work, in fact this is quite different in spirit from the Harvard approach, which was modeled on effective teaching techniques developed in the law and business schools, whereby stylized episodes would be presented to students as the basis for deriving relevant, generalizable principles. For an illuminating discussion of the political context from which this practice emerged, see William Buxton and Stephen Turner, "Edification and Expertise: Sociology as a Profession," in Sociology and Its Publics, ed. T. Halliday and M. Janowitz (Chicago, 1992). I draw some implications for normative philosophy of science from this lineage in Fuller, End of Knowledge, chapter 6.
76. Nash, 116; Kuhn, Essential Tension, 293–319.
77. Nash, 115.
perform on later scientific writings, in which the “logic” of the text is determined almost entirely by the writing conventions of a given discipline, conventions which themselves cannot be understood without delving into the background interests involved in the formation and maintenance of the discipline's boundaries. 78 However, the course that Nash and Kuhn taught was designed to introduce such larger social factors only after the students had grasped the “technical detail” and “tradition of work” that are highlighted in the edited texts. Thus, a strong internal-external history of science distinction is manufactured in the student’s classroom experience, in a way that would not be apparent if the students were simply taught the texts in their original form. As Kuhn might put it, the incommensurability that exists between today's scientists and students is bridged by having students observe historical cases of science as simulations of how science is performed today.

Pivotal in extending the “hands-on” understanding of science officially promoted in Conant’s curriculum has been SSK’s pioneering ethnographic approach to science. Traditionally, the cognitive standpoint informing ethnography has been critical, not of the people under study, but of those who would suppress their voices. Thus, implied by SSK’s critique of rationality is that philosophers of science have routinely misrepresented scientists—indeed, in a way that has contributed to the manic-depressive cycle with which Big Science has been received by the public: impossible expectations followed by disappointment and even betrayal. 79 This is a familiar concern from Conant and Nash, but can the connection between their history-based curriculum and SSK’s ethnographic encounters be made any closer? After all, Kuhn himself has professed hostility to the ethnographic turn in SSK. 80 Yet, once again, the key is Conant himself, who explicitly justified the study of past experimental science as “the equivalent of a magic tour of laboratories” in which visitors would be able to observe the scientists in action, interrupting them with questions about their curious practices. 81

Of course, the currency in which “public understanding” is traded has changed over the past three decades, now that we live in what Guy Debord calls “the society of the spectacle.” For, what Conant originally proposed as an impossibly ideal vehicle for the public understanding of science has recently been advanced by one of SSK’s leading practitioners as an idea whose time has come. 82 As in Conant, we find here an interest in getting citizens to appreciate the “ordinariness” of normal scientific work — and hence not to pin too many hopes or fears on

78. For a cross-disciplinary treatment of the style manual as contested terrain, see Charles Bazerman, Shaping Written Knowledge (Madison, Wisc., 1988).
79. The most comprehensive treatment of this manic-depressive cycle in the popular psyche is John Burnham, How Superstition Won and Science Lost: Popularizing Science and Health in the United States (New Brunswick, N.J., 1987).
80. Horgan, citing Kuhn, 49.
81. Conant, Common Sense, 5.
82. Steven Shapin has recently made this argument in “A Magician’s Cloak Cast Off for Clarity,” The Times Higher Education Supplement (London, 14 February 1992), 15.
it. Perhaps even more now than in the 1950s, part of the appeal to ordinariness involves leaving scientists to do their work just as “you,” the average citizen, would want others to leave you to do yours:

In democratic societies it is always a sound instinct to trust the people with the truth—even if some work has also to be done to overcome institutionalized idealizations. And in this case the truth is that scientists are neither more nor less than our best current experts in their domains. Their workplaces have got all the clutter of your favorite mechanic’s garage, and he is the man you trust with your car.  

The analogy threatens to break down, upon considering that you trust the mechanic not because you see him do things to your car that you yourself would never have dreamed of doing. In fact, that alone might invite suspicion that he is incompetent or overcharging you. Rather, you trust the mechanic because your car runs better after he has worked on it. Unfortunately, the connection between workmanship and outcome is not nearly so close in the case of Big Science, a point repeatedly made by Conant, who, for that reason, stresses the need to identify science with the craft of doing science, which is portrayed as something quite distinct from simply being “well-informed” about the results of science, a state of mind that Conant denigrates as instilling a “merely” critical, outcomes-oriented attitude.  

The appeals to hands-on experience and on-site visits to scientific workplaces are part of what the political scientist Yaron Ezrahi has called the “visual culture” that characterizes modern liberal democracies. According to Ezrahi, the “seeing is believing” brand of empiricism is perhaps the Scientific Revolution’s chief continuing legacy to our understanding of democratic governance. However, the legacy is an ambiguous one that has not generally encouraged a focused critical attitude toward the objects seen. Opening the laboratories to the public certainly gives the impression that the scientists have nothing to hide. As a result, that gesture of openness may be just enough to discourage the public from further probing behind the scenes. Moreover, in cases where members of the public look closely at a scientist’s work, they are likely to be struck by the distinctive skills on display, while failing to wonder about the overall ends that those skills serve. One might say that the epistemological fallacy associated with spectator culture—a fallacy that has only become more trenchant with our ability to see more of the world through the “small screen” of television—is a version of affirming the consequent: namely, “seeing is believing” generates the conclusion “out of sight, out of mind.” In the case of science, then, whatever escapes the spectators’ visual horizon—the networks of power and channels of information that exist behind and beyond the lab environs—is simply not factored into their sense of a proper accounting for science. Indeed, given that

83. Shapin, 15.
84. Conant, Common Sense, 315–321.
86. Readers intrigued by the figurative imagination should consider the following association: Nash, originally trained to prepare sample chemicals, now moves from materials to media, wherein he prepares case studies, the pedagogical equivalent of staged events.
a larger share of the scientist's time and energy is being devoted to "entrepre-
neurial" activities outside the lab, lab work in today's world would seem to be
little more than a showcase activity — perhaps, like so many tribal rituals, done
primarily for the benefit of the spectators.\footnote{87}

Conant misjudged the long-term impact of the General Education curriculum.\footnote{88}
True, it was not adopted by many colleges. However, in Kuhn's hands, a failed
undergraduate course of study turned out to be remarkably successful in, so to
speak, the area of faculty development. At least, widespread conversion to
the pedagogical perspective of Natural Sciences 4 would explain why certain
tensions in Conant's original vision have remained unarticulated by those who
come to their understanding of science through Structure. These tensions may
be summarized in three sets of questions:

(1) If pure science affects us only through its applications, then why should
General Education curricula focus on the creative process of pure science, rather
than on the real "strategy and tactics" by which the pure and the applied are
networked together? (Conant reserved the military metaphor for the heuristics
scientists used to channel their creativity.\footnote{89}) Why continue focusing on indi-
vidual achievement, where an understanding of institution building would seem
to be in order?

(2) If the public understanding of science has become crucial because of the
investments and impacts of Big Science, then why study historical cases of
"Little Science"? If it is indeed true that "the scientific mind" has not changed
over the centuries, then why not simply conclude that the scientific mind is not
the crucial factor in fathoming the workings of Big Science?

(3) If the value of scientific work can only be judged by fellow scientists
(especially if, as Conant insisted, it was to be judged of the "man," not even of
the project\footnote{90}), then how exactly is the General Education curriculum supposed
to contribute to decision-making about science in a democracy?

The tensions reflected in these questions represent the tricky ongoing negotia-
tions between science and power in modern democracies.\footnote{91} The philosophical
movements that most strongly identified with experimental science prior to
World War I, positivism and pragmatism, grounded science's superior epistemic
status on its alleged ties to socially beneficial technologies. I say "alleged" be-
cause, while science did much to explain and to legitimate the great technological
advances of the nineteenth and early twentieth centuries, it contributed rela-

\footnote{87. In this discussion, I make the controversial assumption that there is a viewpoint from which one could comprehend a significant part of the whole scientific process, a perspective that is, at least in principle, open to anyone. Ezrachi, for one, denies it, as do other postmodernists. I explicitly defend retaining the idea of a public sphere for governing science in Fuller, End of Knowledge, chapter 8.}
\footnote{88. Conant, Several Lives, 373.}
\footnote{89. James Conant, On Understanding Science (New York, 1946), 102ff.}
\footnote{90. Conant, Common Sense, 323.}
\footnote{91. See Proctor, Value-Free Science?, for the relevant history in Germany and America.}
tively little to their actual invention.\textsuperscript{92} Science becomes instrumental in the construction of "progressive" technologies around the time that it also participates in the construction of war machines. After World War I, most notably in the case of logical positivism, rationalist philosophers start to distance the theoretical trajectory of science from its real or potential applications, which were, in turn, typically presented as deflecting science from its natural trajectory.\textsuperscript{93} Yet, this turn comes just as science is increasingly driven by military and industrial concerns, indeed, with major conceptual breakthroughs often arriving as the spinoffs of what would ordinarily be regarded as "applied" research.\textsuperscript{94} At the very least, then, Conant needed to ground a strong sense of what was "internal" and "external" to science in order to prevent public exposure of what ethicists call "the problem of dirty hands," namely, the ambivalence that arises from a practice (in this case, natural science) that unavoidably issues in both good and bad consequences.\textsuperscript{95}

Clearly, Conant had his work cut out for himself. It was a much harder job than, say, Polanyi had in Britain, where the aesthetic line of "science for its own sake" still held the keys to the coffers. In America, funding for pure science ultimately had to be justified in terms of its long-term public benefits. Yet, if pure science were forced to compete in the capitalist marketplace, it would easily lose to industrial based research, which could also use higher salaries to attract keener intellects. All of this Conant openly acknowledged. In that case, what would the public do, once it becomes "expert in judging experts"? Conant argued that these lay meta-experts would provide a pool of funds large enough to enable all professionally approved practitioners in a field to compete among themselves, according to established rules of scientific evaluation.\textsuperscript{96} This process, now known as "peer review," was not much different from what Polanyi advised, but it served to circumvent the problem of dirty hands. For even if Conant could not prevent the prospect of more science producing equal amounts of evil and good, he could help manufacture an asymmetry in the amount of accountability demanded of research designated "pure" (relatively low) vis-à-vis that designated "applied" (relatively high).\textsuperscript{97} Thus, the ultimate goal of Conant's General Education curriculum was a lay public capable of discriminating between instances of these two types of research—a public conditioned to see the dirt in one set of hands but not in the other.

\textsuperscript{92} Michael Mulkay, "Knowledge and Utility: Implications for the Sociology of Knowledge," \textit{Social Studies of Science} 9 (1979), 69–74.

\textsuperscript{93} Of course, irrationalist antiscientific philosophers continued to see a close connection between theory-driven research and technological consequences, as physicists were at pains to learn in the Weimar Republic. See Paul Forman, "Weimar Culture, Causality, and Quantum Theory: 1918–1927," in \textit{Historical Studies in the Physical Sciences}, ed. R. McCormmach (Philadelphia, 1971).

\textsuperscript{94} See \textit{Big Science}, ed. Peter Galison and Bruce Hevly (Stanford, 1992).

\textsuperscript{95} For a philosophical discussion of the issues involved in "dirty hands," see Bernard Williams, \textit{Moral Luck} (Cambridge, Eng., 1981), chapter 4.

\textsuperscript{96} Conant, \textit{Common Sense}, 32ff.

\textsuperscript{97} I mean quite literally that the basic-applied research distinction originated in accountancy, specifically that used by the U.S. National Science Foundation. See Daniel Greenberg, \textit{The Politics of Pure Science} (New York, 1967), 31–36.
If Conant's writings persuaded U.S. government officials and much of the lay public to support the funding of pure science without requiring too much accountability in return, Kuhn's *Structure* occluded the tensions in Conant's vision for the potentially more critical audience that was brewing within the academy, namely, professional sociologists, who strove throughout most of the century—with decidedly mixed results—to acquire the mantle of science.\(^{98}\) By the second term of the Eisenhower administration, sociologists such as C. Wright Mills and Alvin Gouldner had started a backlash, distancing the goals of the social sciences from those of the natural sciences, with the latter being increasingly portrayed as amoral, if not immoral, technologies of control available to the highest bidder, complicitous in what Mills called the emerging "scientific-military-industrial complex."\(^{99}\) An assumption that united Mills and Gouldner was that, since the world is causally too complex to afford any unmitigated goods, the only intellectually responsible attitude to the status quo, including science itself, was reasoned skepticism. Here was full recognition of science's dirty hands.

The distinction that Mills and Gouldner drew between the natural and social sciences did not aim (as it so often does today) to replay the cluster of turn-of-the-century Natur-Geisteswissenschaft debates (generalizing versus particularizing methodologies, matter-based versus mind-based ontologies). Rather, they elaborated two types of reasoning that Karl Mannheim had identified at the outbreak of World War II: a free-standing "functional" rationality that operated without consideration of the ends it served (natural sciences) and a socially embedded "substantive" rationality that extended inquiry into the ends that reason served (social sciences).\(^{100}\) Substantive rationality was no less scientific than its functional counterpart, if by "scientific" one meant systematic empirical inquiry; but it was more comprehensively critical, "reflexive" (in Gouldner's sense), by virtue of placing the status of the inquirer within the scope of inquiry. Thus, in addition to wanting to know why X is the case, the substantively rational scientist would also ask: Who wants to know? However, *Structure* deflected much of sociology's critical gaze on the natural sciences by showing (once again, unwittingly) how it, too, could legitimize itself as a "real" science, and thereby (presumably) no longer be excluded from the ruling power structure.\(^{101}\) An apt slogan for this turn of events is the subtitle of another movie

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starring Peter Sellers, the one in which he played Dr. Strangelove: *How I Learned to Stop Worrying and Love the Bomb*.

Although Kuhn, no less than Polanyi, wanted to restrict both the interrogation and appropriation of scientific authority, this concern remains muted in the pages of *Structure*. Indeed, social scientists were attracted to Kuhn's book precisely because it seemed to provide a blueprint for how a community of inquirers can constitute themselves as a science, **regardless** of their subject matter. The fact that *Structure* draws its historical examples almost entirely from physics and chemistry was conveniently overlooked.\(^\text{102}\) In short, Kuhn's interest in generalizing Polanyi's account of scientific authority meant that he had to detach that account from the local squabbles in which Polanyi first developed it. However, this move, in turn, created a free-floating legitimating narrative—a "myth," properly speaking—that could be used by any discipline in need of boosting its status: quite the opposite of what either Polanyi or Kuhn would have wanted. The disembodied character of *Structure's* narrative was not lost on fellow historicists Hanson and Toulmin, both of whom suspected Kuhn of having interdefined his key terms, especially "paradigm" and "revolution," to such an extent that there was no way for a discipline to have a paradigm without a revolution, and vice versa.\(^\text{103}\) The twin specters of tautology and unfalsifiability that Hanson and Toulmin raised against Kuhn were, of course, bugaboos to the philosophical mind, but safe havens for fields in need of legitimation.

Here it is worth recalling an astute account of the social function of narrative recently suggested by literary critic J. Hillis Miller.\(^\text{104}\) Miller argues that narratives can sublimate the potentially disruptive character of social change in two ways. On the one hand, a narrative can script such change through the reenactment of a sequence of events that in the past had enabled others to reach a social status similar to the one currently desired. The clearly labeled plot structure of Kuhn's account of scientific change contributes to this function well, as it allows others to see that one's discipline is behaving as a science would at a certain stage in its development. On the other hand, a narrative can effect a subtler social channeling, one that operates as a safety valve for dissipating subversion. In this case, the wording of the narrative incorporates, typically by extended metaphor, elements that threaten the social order which sustains the narrative's legitimacy. An evocative expression (not Miller's) that captures this function is *verbal co-optation*. The desired effect is that the threatening elements are contained by severing any connection that they might have to a world outside

\(^{102}\) The restriction of Kuhn's examples to physics and chemistry reflects the experimental emphasis that Conant placed in designing Natural Sciences 4. Most of these examples are given more extended treatment in *Understanding Science*, which was later expanded (with Kuhn's acknowledged help) in *Common Sense*.


the narrative. In the Kuhnian narrative, with its talk of "crises" issuing in "revolutions," the political economy of science has been so verbally co-opted that Kuhn's sociological followers, be they Mertonian or SSK, have been able to cast several models of scientific activity in the discourse of political economy without either making links with the larger political and economic scene that sustains Big Science or even drawing much on the empirical and explanatory resources of political science and economics.\textsuperscript{105}

As evidence for this pseudopolitical economy of science that has been sustained by Kuhn's work, I will focus on the simple locution, "Big Science," coined by the historian Derek de Solla Price, as that forms the basis for much of the other work in this vein.\textsuperscript{106} Price is generally recognized as the founder of "scientometrics," the discipline that designs quantitative measures for tracking the growth of knowledge, typically with an eye to science policy concerns. Although Price's original work was independent of Kuhn's, scientometricians have subsequently focused on identifying the "life cycle" of scientific specialties, deriving the stages in this cycle from Kuhnian categories of old paradigm-anomalies–crisis–revolution–new paradigm.\textsuperscript{107} Notwithstanding the merit of much of this work, it rests on simplifying assumptions about the international political economy of science that cannot help but contribute to the image of science as a self-contained, self-sufficient enterprise. For example, the major "product" of science is presumed to be the journal article, whose value is measured in terms of the other articles that cite it as instrumental in their own production. What makes today's science so "big," then, is the number of articles produced, and especially the exponential rate at which they are being produced. Price justifies his circumscription of scientometrics by appealing to a contrast between the politically motivated accounting procedures that governments use to estimate "research and development" expenditures and the policy of "free trade" and "open markets" demonstrated by the publication practices of scien-

\textsuperscript{105} Followers of the postmodern scene will recognize a pronounced Baudrillardian element in the success that social scientists have had in acquiring legitimacy by means of the Kuhnian narrative. Once enough research programs have persuaded enough audiences that they constitute sciences, even if it is impossible that they could all be right (because a true science has only one paradigm at any given time), and even if an increasing number of historians doubt that the Kuhnian narrative is itself right, those new "sciences" have already acquired lives of their own—hyperrealities—which create similar expectations in other fields desirous of becoming sciences. See Jean Baudrillard, \textit{Simulations} (New York, 1983).

\textsuperscript{106} Derek de Solla Price, \textit{Big Science, Little Science . . . and Beyond} [1963] (New York, 1986). Among those influenced by Price, some (such as Warren Hagstrom, who credits his knowledge of the history of science to Kuhn's tutelage at Berkeley) paint an orderly Mertonian picture whereby science's "reward structure" is governed by internally generated status-markers, such as citation counts. Others, such as Bruno Latour, revive Conant's original military metaphor of "strategy and tactics" to paint a more competitive picture of science. See Warren Hagstrom, \textit{The Scientific Community} (New York, 1965); Bruno Latour, \textit{Science in Action} (Cambridge, Mass., 1987).

\textsuperscript{107} Marc De Mey, \textit{The Cognitive Paradigm} (Dordrecht, 1982), 111–170, esp. 148–170. One scientometric indicator of where a field is in the Kuhnian life cycle is "Price's Index," which measures the obsolescence rate of journal articles in terms of diminishing citation patterns. A rapid obsolescence rate means a rapidly advancing research front, a paradigm at top puzzle-solving performance levels.
tific journals. Price seems to be impressed with the fact that out of a myriad of national interests can emerge a global picture of science that is reducible to fairly simple and intuitive logistic curves, thereby testifying to the existence of science as a spontaneously generated and self-sustaining organism.

Price’s thinking here is by no means idiosyncratic. It would be hard to overestimate the grip that Kuhn’s Aristotelian picture of science-as-organism has had on science policy thinking and research over the last thirty years. As in the philosophy of science, the idea has likewise served to diminish the normative dimension of the policy-maker from prescriber to evaluator. The analogous contrast here is between planner and forecaster. In the case of scientometrics, it caters to a “clinical” picture of science policy, whereby the policy-maker attempts to diagnose and treat an objective condition of a science on the basis of key symptoms, or “indicators.” The science in question is taken to have a natural trajectory, which the policy-maker can then slow or hasten, or, more drastically, divert in some other direction, perhaps halting progress in the science altogether. Each more substantial intervention is seen as incurring a greater risk to the delicate balance of factors needed for the science to flourish. It has become common in Western countries for science policy-makers to see themselves primarily in the business of adjusting the tempo of scientific change, and preparing the public for absorbing the intended and unintended technological consequences of that change. The burden of planning is taken to lie in reconstructing, not the scientific enterprise, but the larger society, presumably because a society’s ordinary institutions are safely manipulable to meet the public interest, whereas the “natural” ends of science can be easily perverted if forced to serve interests other than its own.

A case can be made that Structure was used to legitimate this view of science policy even before it had substantially transformed more strictly academic con-

109. Thus, in concluding about “the future of science,” Hagstrom says: “The central problem facing society with regard to all of the professions is the same: how can they be controlled without having their effectiveness destroyed?” Hagstrom, 294. An interesting project for the cultural theory of risk pioneered by Mary Douglas and Aaron Wildavsky would be to examine which groups would be placed “at risk” by a stronger sense of science planning than the forecasting approach countenances. My guess is that at risk are the social scientists, whose legitimacy rests on their having correctly followed a presumptively universal pattern of scientific development! For a survey of the social causes and effects of forecasting over the last thirty years, see Max Dublin, Futurehype (New York, 1989). Dublin argues that, contrary to its own hype, forecasting has politically conservative, not liberating, consequences, because it is based on extrapolating from current trends. The high degree of likelihood attached to the future resembling the past artificially raises the level of risk in potential deviations.
110. Even in West Germany of the 1970s, where Structure was read largely through the critical Marxism of Habermas, the call for “state planning” of science amounted to waiting for a paradigm to “mature” before redirecting (or “finalizing”) research from “basic” to “applied.” In practice, this policy touched only the sciences that have reached an advanced stage of puzzle-solving, whereby the prospect of technological spinoffs from solving additional puzzles becomes less economical than simply having the scientists address public needs directly. This is a far cry from the specters of planned science that set Polanyi on the warpath during the early years of the Cold War. See Finalization in Science, ed. Wolf Schaefer (Dordrecht, 1984).
ceptions of science. One of the most influential policy theorists of the post-
World War II period, Don K. Price, discusses Kuhn's views in some detail in
a 1965 book, the critical acclaim of which soon afterward propelled Price into
the presidency of the American Association for the Advancement of Science.\footnote{111}
Reflecting on his experience as an officer in the Ford Foundation's program of
technical assistance for developing countries, Price argued that both science
and politics had their own proper ends, and that effective science policy was
forged by keeping communication channels open, while respecting the inherent
differences between these two spheres of activity. Price appeals to Kuhn in order
to draw a connection between scientists' instinctive rejection of nonparadigm-
based accounting procedures, such as a federal budget, and their ability to get
at the nature of some subject matter. Defending those instincts, Price proceeds
to argue that if, on the contrary, scientists were driven primarily by political
imperatives, they would be engaging solely in applied research, leaving more
discipline-based pursuits to grow fallow.\footnote{112} Unfortunately, this argument ob-
scured the delicate fact that modern governments have been just as eager (if not
more so) to support research that scientists deem "pure" as that which they deem
"applied." In the name of pure science, they could get high quality information
("intelligence") that might someday serve to legitimate otherwise dubious political
acts, in return for the scientists' assurances that they will not feign compen-
tence in determining the appropriate uses for their work. Thus, the problem of
science's dirty hands is solved by the principles of democratic pluralism, as
midwived by Thomas Kuhn!

A measure of Structure's unwitting effectiveness is that no book of its kind has
since been written, even though such books were quite common from the late
eighteenth to the mid-twentieth centuries. The genre I have in mind may be
called \textit{didactic macrohistory of science}. A book of this sort purports to draw
general lessons from the unfolding of our collective cognitive development.
Even when the sequence of events displays a stepwise "logic," the stress is
consistently placed on the need to prepare for a future that will be significantly
different from the present, but which, with some ingenuity, can be gleaned
by studying the past. The presumption is that "being later" always carries an
epistemic advantage. The argument in these books often proceeds by arranging
historical precedents in a suitably illustrative manner, and contesting the ar-
rangements that others have made. Thus, such historian-philosopher-scientists
as Auguste Comte, William Whewell, Ernst Mach, Charles Sanders Peirce,
Pierre Duhem, Emile Meyerson, and Gaston Bachelard would assess whether
a certain scientific practice would generalize across all the sciences and even into
the regimentation of everyday life, they would pass judgment on the value of
such a thing happening, and they would offer advice on what can be done to

\footnote{112} Here Price is following the original reasoning that Vannevar Bush used to establish the
National Science Foundation as an outpost for "pure science." See Hollinger, 901.
encourage the more propitious prospects they have divined. Often a book of this sort would be the way by which a philosopher communicated with a wider audience, showing how, in Hegelian fashion, history dramatizes one's favorite abstract arguments. In the first half of this century, the cases of Russell, Whitehead, and Dewey come readily to mind.

However, unlike Hegel's dialectic, the life cycle of a Kuhnian paradigm merely repeats without making progress. Moreover, not only does Kuhn confine progress to a given paradigm, he also throws up formidable epistemic barriers to anyone wishing to peer beyond the current one. The risks entailed by revolution and the uncertainties of the Planck Effect have been emphasized here. Retrospectively, the didactic historian would have to contend with what Lakatos dubbed "Kuhn Loss," the phenomenon whereby problem areas deemed important in an earlier paradigm simply disappear from a later one, only perhaps to reappear still later on. I do not take issue with these astute historiographical insights, but rather with Kuhn's failure to offer more guidance on what to make of them. In effect, Kuhn exhibits a marked insensitivity to his own historicity, that is, the elementary hermeneutical point that his understanding of the past is predicated on his living when and where he does.\textsuperscript{113} That Kuhn wrote \textit{Structure} in the United States in 1962, and not in Germany in 1862 or in France in 1762, does not seem to tell him any more (or less) about where science might be heading. The message is the one originally promulgated in the General Education curriculum: the scientific process remains essentially the same whenever and wherever it occurs. While Kuhn is hardly the first historian to overlook his own historicity, the contiguity of his perspective has been most remarkable, contributing to the present-day absence of histories of science comparable in scope to, say, Paul Kennedy's \textit{The Rise and Fall of the Great Powers}—a book I choose deliberately for reasons that will become clear below.

Historians of science have made a point of decrying the influence that \textit{Structure} has had on how non-historians think of their field.\textsuperscript{114} Nevertheless, two styles of doing the history of science are traceable to Kuhn's work. Both derive from his experience in the Conant curriculum. One style harkens back to Kuhn's fledgling efforts at Natural Sciences 4. It takes off from Kuhn's own oft-repeated story of how, given his own recent physics training, he originally found the texts he had to teach by Aristotle and even Newton unintelligible.\textsuperscript{115} This characterizes the "relativistic" style that SSK picked up, which conceptualizes the past as a foreign country separated by time as if by space, requiring acculturation into native customs quite unlike our own. But once Kuhn became a proficient instructor, the second, more Piagetian style predominated, which focused the student's attention only on salient aspects of scientific episodes that were neces-

\textsuperscript{113} Here I must take exception with the excess hermeneutical insight that Richard Bernstein ascribes to Kuhn. See Bernstein, \textit{Beyond Objectivism and Relativism} (Philadelphia, 1983), esp. 20–34.

\textsuperscript{114} See, especially, Reingold, 389–409.

\textsuperscript{115} See, for example, Kuhn, \textit{Essential Tension}, xi-xiii.
sary for recognizing and resolving some paradigmatic tension.\textsuperscript{116} Here we find the Orwellian historical perspective that enabled the students in Kuhn’s course to acquire the kind of “understanding” of science that Conant sought. Although few professional historians have adopted this style, it is well represented by philosophers and psychologists who profess a “cognitivist” approach to the history of science.\textsuperscript{117} Both of these styles strike most workaday historians as betraying undue philosophical influence: extreme relativism on the one hand, and extreme universalism on the other. However, the one philosophical influence that is missing is the one that continues to inform didactic macrohistories of politics, such as Kennedy’s. It is a perspective that confers epistemic privilege on “being later” without presuming that what one now sees is better than before. Such a perspective involves neither obscurantism nor Whiggism—to invoke the swear words that historians are prone to use for the Kuhn-generated extremes. Rather, it prods the audience to convert a usable past into a viable future.\textsuperscript{118}

In hindsight, \textit{The Structure of Scientific Revolutions} unwittingly achieved for its audiences much of what Daniel Bell’s \textit{The End of Ideology} tried but failed to do for his, namely, to alleviate the anxieties of alienated academics and defensive policy-makers by teaching them that they could all profit from solving their own paradigmatic puzzles. But I have also argued that Kuhn’s lesson has exacted its own substantial Kuhn’s Loss, as the triumph of his paradigm of scientific change has marginalized the forums for a radically critical rationalism, one that questions the ends of science as well as the means. However, if the reception of Kuhn’s book were just another case of Hegel’s Cunning of Reason passing through the world, it would not merit this essay’s subtitle, “a parable for postmodern times.” At the start, I commented on Kuhn’s peculiar reaction to his success. Equally, there is Kuhn’s audience’s peculiar reaction to \textit{Structure}. A common thread that runs through the formal and informal comments that people make about the book is that it is quite thin in their own field of expertise,

\textsuperscript{116} Kuhn picked up his Piaget from Jerome Bruner, lecturing at the Harvard School of Education. The link that Kuhn implicitly draws between a Piagetian child advancing to the next stage of cognitive development and a student reenacting a gestalt switch from the history of science comes out most clearly in Kuhn, “A Function for Thought Experiments,” in \textit{Essential Tension}, 240–265.

\textsuperscript{117} Like Kuhn himself, only twenty years later, Marc De Mey gained a grounding in cognitive psychology while attending Jerome Bruner’s lectures at Harvard. De Mey’s \textit{The Cognitive Paradigm} is the most comprehensive attempt at a “cognitivist” understanding of science that is in Kuhn’s spirit. The best single historical monograph in this vein is Howard Margolis, \textit{Patterns, Thinking, and Cognition} (Chicago, 1987). Today, “cognitive history of science” covers a wide variety of approaches, most of which are rather un-Kuhnian in that they draw more on artificial intelligence than on developmental psychology. A good sense of the range is provided in \textit{Cognitive Models of Science}, ed. Ronald Giere (Minneapolis, 1992).

\textsuperscript{118} The only didactic macrohistory of science of recent vintage to have garnered some attention is Philip Mirowski, \textit{More Heat than Light} (Cambridge, Eng., 1989). This excellent work (which deserves to become a historiographical paradigm) not only shows the steady decline of neoclassical economic theory as it develops a metaphor drawn from defunct physics, but it also points the way toward redeploying resources from the institutionalist tradition to constitute a new social theory of economic value.
but truly enlightening in some other field, one in which they have had an interest for a long time, but could not locate a suitable point of scholarly intersection. We might say, then, roughly speaking, that *Structure* has a philosopher's sense of sociology, a historian's sense of philosophy, and a sociologist's sense of history. A text with these characteristics is assured a good reception just as long as the practitioners of the different disciplines continue talking only to their own colleagues and not to those of the field which Kuhn supposedly represents so well for them. At that point, the day when (if!) inquiry de-specializes, the attraction of Kuhn will fade. Moreover, precisely because *Structure* is *not* a perfect text, and does not cloak its imperfections in jargon, it invites further participation by the reader to correct its flaws and complete its argument. This perhaps explains why everyone finds it important to position themselves in terms of Kuhn without feeling compelled to follow in his footsteps.

*Virginia Polytechnic University*