Higher Superstition

The Academic Left and Its Quarrels with Science

PAUL R. GROSS
University of Virginia

NORMAN LEVITT
Rutgers University

THE JOHNS HOPKINS UNIVERSITY PRESS
Baltimore and London
CHAPTER THREE

The Cultural Construction of Cultural Constructivism

The point is that neither logic nor mathematics escapes the contamination of the social.

Stanley Aronowitz, Science as Power

So long as authority inspires awe, confusion and absurdity enhance conservative tendencies in society. Firstly, because clear logical thinking leads to a culmination of knowledge (of which the progress of the natural sciences provides the best example) and the advance of knowledge sooner or later undermines the traditional order. Confused thinking, on the other hand, leads nowhere in particular and can be indulged indefinitely without producing any impact upon the world.

Stanislaw Andreski, Social Sciences as Sorcery

Natural scientists—at least those with a sense of fair play—are usually diffident in confronting the disciplines that study science from a social and historical point of view. They do not feel that their particular expertise in some area of science automatically endows them with insight into the human phenomenology of scientific practice, or that their familiarity with the recent results and the liveliest questions of their specialty qualifies them to pronounce on its evolution as that relates to the course of human development. Apart from the most arrogant, they concede that the psychological quirks and modes of personal interaction characteristic of working scientists are not entitled to special immunity from the scrutiny of social science. If bricklayers or insurance salesmen are to be the objects of vocational studies by academics, there is no reason why mathematicians or molecular biologists shouldn’t sit still for the same treatment.

Accustomed to regarding specialists in a scientific subject remote from their own with a certain courtesy or even deference, natural scientists are usually quite willing—perhaps even too willing—to adopt a similar attitude toward apparently competent scholars whose chief interests lie outside of science even as they try to come to grips with the relation between science and their own fields. Above all, natural scientists are reluctant to take a haughty and dismissive attitude toward the hypotheses and theories of outsiders merely because they seem at first paradoxical or are expressed in a recondite language. They are aware that some matters of the greatest professional concern to them may strike an outsider as abstruse, bewildering, perhaps even nonsensical. Consequently, their initial inclination is to credit the sociologist or historian of science with having reliable intellectual tools and a sense of responsibility in applying them, however jarring the language of the unfamiliar discipline.

This, at least, has been the traditional attitude. We must report, however, that it is changing to one of skepticism and even revulsion in the face of what scientists—at least those few who have so far taken a serious interest in the question—have come to see as a growing tendency among a particular breed of historians and sociologists of science to spin perverse theories. These seem often to escape mere inaccuracy and rush hell-for-leather toward unallayed twaddle. Such words may strike the reader as spleenetic; but they seem to us justified in view of certain recent developments in the social-scientific analysis of the natural sciences, specifically those that can be lumped together under the heading “cultural constructivism.”

Cultural Constructivism, Weak Form

We are all, in a commonsensical way, cultural constructivists in our view of science. Science is something that human culture has, indeed, “constructed,” after seventy thousand years or so of false starts and dead ends. Our thankfulness for it knows no bounds. Pieties aside, however, we can accept many of the views that historians and sociologists of science promulgate by way of asserting that science is, in some sense, a cultural construct. It would be idle to pretend that the projects taken on by science, the questions that it asks at any given period, do not reflect the interests, beliefs, and even the prejudices of the ambient culture. Clearly, certain kinds of research get the strongest encouragement—funding, recognition, celebrity, and so forth—in response to the recognized needs of society. To take an obvious current example, research in high-temperature superconductivity is avidly pursued by increasing numbers of physicists and chemists, not only because the subject is fascinating, beautiful, and difficult, but because the potential utility of high-temperature superconductors is enormous. They might very well have technological and economic reverberations comparable to those of semiconductors. And, at least for a time, a few years ago, when new superconductor
materials were appearing thick and fast, the work got almost as purient attention from the media as did cold fusion. To say that in this sense science is culturally constructed is tautological.

Naturally, some social theorists would extend the analysis to suggest that the topics scientists focus upon are determined by socially derived attitudes, aspirations, and biases less forthrightly instrumental, and that there are negative aspects as well—certain areas of potential research are avoided in obedience to assumptions that are rarely articulated in undisguised form. While this is not incontestable, it has some plausibility, and we would not deny it out of hand.

An even stronger assertion is that in scientific debate and in the process by which a preference for one paradigm over another emerges, attitudes of mind come into play that are in some measure dictated by social, political, ideological, and religious preconceptions. For example, Stephen J. Gould has recently argued that Darwin's view of sexual selection as an important evolutionary mechanism was slow to win acceptance because it offended the prejudice, obviously tied to prevalent ideology, that females are by nature passive and lack sufficient volition to make the crucial choices that Darwin's—extended—model required. We accept such ideas as reasonable in principle, even though they are much oversold these days. We caution, however, that the areas of science in which such direct intrusion of ideology becomes possible are few. Our reading of the history of science suggests, moreover, that theories leaning heavily on such props tend to be fragile and ephemeral, and that part of the increasing power of scientific methodology derives from always-increasing awareness of the danger that reasoning can be corrupted in this way if one is not careful. Nevertheless, we are obliged to listen with interest to historical and sociological accounts of the effect. Thus we accede in principle to what might be called the "weak" version of cultural constructivism.

Such a point of view can produce illuminating research. Like any other point of view, it can also be driven into the ground and employed in doctrinaire fashion to substitute for evidence. Good work and bad can be done in its name. A further danger, frequently in evidence in the writings we consider below, is that analyses and case histories counting as reasonable instances of weak cultural construction are slyly adduced as justifying a far more radical and dubious theory, a version of philosophical relativism and conventionalism that merits the name "strong cultural constructivism." This is another part of the theoretical woods entirely—although many historians, sociologists, and even philosophers of science are insufficiently vigilant in maintaining the distinction.

Cultural Constructivism, Strong Form—Science as Convention

In strong form, cultural constructivism (sometimes, another phrase such as "social constructionism" may be used, depending on the terminological preferences of the expositor) holds to the following epistemological position: science is a highly elaborated set of conventions brought forth by one particular culture (our own) in the circumstances of one particular historical period; thus it is not, as the standard view would have it, a body of knowledge and testable conjecture concerning the "real" world. It is a discourse, devised by and for one specialized "interpretive community," under terms created by the complex net of social circumstance, political opinion, economic incentive, and ideological climate that constitutes the ineluctable human environment of the scientist. Thus, orthodox science is but one discursive community among the many that now exist and that have existed historically. Consequently its truth claims are irreducibly self-referential, in that they can be upheld only by appeal to the standards that define the "scientific community" and distinguish it from other social formations.

It must follow, then, that science deludes itself when it asserts a particular privileged position in respect to its ability to "know" reality. Science is "practice" rather than knowledge; and practice involves convention and arbitrariness. Questions can be asked only when they conform to the modalities of existing discursive habits; likewise answers can be formulated and recognized only to the extent that they are accommodated within that template. The verification of these supposed answers is performe a discursive—in the broad sense, a linguistic—event, in that it involves dialectical manipulation of accepted semiological conventions. Even—no: especially!—the practices that most particularly embody the sacred "objectivity" of science—experiment and observation—are inescapably textual practices, meaningless outside the community that endows them with meaning.

The attentive reader will have noted that this point of view rigorously applied leaves no ground whatsoever for distinguishing reliable knowledge from superstition. Indeed, there are various contexts in which that would seem to be exactly the point of the exercise. Given the long history of progressive Western thought in which science has been linked, by and large, with the efforts of human liberation, it will seem surprising if not positively bewildering that this complex of ideas has for the most part been developed and embraced by self-identified left-wing intellectuals.

True enough, critiques of science employing a similar logic appear from time to time in defense of theistic views of the universe that place scientific materialism in its more accustomed role as the enemy of religion and revealed
truth.² Nowadays, however, the left seems far more eager to sponsor such views. Its motivation, its polemical point, seems to be as follows: Scientific questions are decided and scientific controversies resolved in accord with the ideology that controls the society wherein the science is done. Social and political interests dictate scientific "answers." Thus, science is not a body of knowledge; it is, rather, a parable, an allegory, that inscribes a set of social norms and encodes, however subtly, a mythic structure justifying the dominance of one class, one race, one gender over another. This, at any rate, is the message that permeates the culture of the academic left, setting the terms of its view of science. It is the motto on the banner flown by cultural constructivism when it functions as a political force.

A typical example of the discourse of the cultural constructivists, certain to startle a scientifically literate person who has never encountered the genre, can be found in The Science of Pleasure: Cosmos and Psyche in the Bourgeois World View, by sociologist Harvie Ferguson. He summarizes a key development of twentieth-century physics as follows:

The inner collapse of the bourgeois ego signalled an end to the fixity and systematic structure of the bourgeois cosmos. One privileged point of observation was replaced by a complex interaction of viewpoints.

The new relativistic viewpoint was not itself a product of scientific "advances" but was part, rather, of a general cultural and social transformation which expressed itself in a variety of "modern" movements. It was no longer conceivable that nature could be reconstructed as a logical whole. The incompleteness, indeterminacy, and arbitrariness of the subject now reappeared in the natural world. Nature, that is, like personal existence, makes itself known only in fragmented images.¹

We assure the reader that Ferguson is referring unambiguously to Einstein’s relativity theory, not to some broader and murkier notion of "relativity"! He means literally, and reaffirms throughout the book, that developments in physics are not only conditioned, but dictated by the evolution of something called "bourgeois consciousness," whose course is in turn determined, in proper Marxist fashion, by "commodity relations." People moderately expert in modern physics and minimally familiar with its history will not find such pronouncements plausible in the slightest, nor will they concede very much to the supporting arguments. The latter merely decorate a compressed version of the standard history of modern physics with bizarre assertions to the effect that throughout it all, the desperate bourgeois ego was frantically supervising developments, a sort of crazed dramaturge. Such propositions have all the explanatory power of the Tooth-Fairy Hypothesis. Still, hundreds of left-wing social theorists dote on them.

Quite naturally, cultural constructivism—in its strong form—is one of the starting points and chief ideological mainstays of the feminist critique of science. Likewise, it fuses with the spectrum of doctrine and attitude that comprises the so-called postmodern intellectual stance, when that viewpoint attempts to give its own account of the sciences. We shall, however, address both the feminist and postmodernist analyses of science in separate chapters, recognizing that this involves quite arbitrary distinctions. Here we consider strong cultural constructivism as it is practiced by historians, sociologists, and other students of natural science as a social phenomenon. Most of these are committed to a leftist political position; they regard their study of science as part of an overall program of radical analysis and demystification of bourgeois sacred cows.

We shall not attempt immediately an exploration of why this should be so; but some points are clear enough. First of all, in the face of an increasingly monolithic social and political structure, whose capacity for self-perpetuation and extension seems endless, it is difficult for radical intellectuals to accord an exceptional status to science, leaving it exempt from what they regard as the ominous tendencies of capitalism. They are highly unwilling to view science as an activity of the autonomous and unfettered intellect. It is easy to see their point. Science, after all, well integrated into the technological, industrial, and military machinery of the capitalist system; in turn it relies on that system for the material basis of its continuing progress, at least in those fields where a substantial investment of money is necessary for fruitful research. For working scientists in the belly of the beast, of course, the situation seems far more subtle than that. In fact, from a variety of perspectives, scientists and intellectuals in general might honestly (and correctly) view the present culture as a historical paragon, to the degree that it fosters and encourages autonomy of thought and freedom of ideas. On the other hand, the social critic who identifies with a long tradition of militant intransigence, and for whom positive social change invariably requires discontinuity, remains unmoved by such considerations. This critic views the scientist’s claims to independence as part of the constructed ideology that imprisons and in the end directs him. To the analyst of cultural constructivist bent, matters of scientific truth are "always and everywhere matters of social authority."

Furthermore, to the extent that conventional science can be deposed from its position as a uniquely accurate way of finding out about the world, contending perspectives, especially those arising from the demotic substructure
of society or from oppositional movements, rise at once to a higher epistemological dignity. Belief systems that, on the scientific view, are little more than superstition are at least provisionally validated by the cultural constructivist hypothesis, while the results arising from scientific investigation, if ever they appear irritating or unwelcome, become "contestable."

This is a book about politics and its curious offspring, not about epistemology or the philosophy of science; we cannot therefore refute, in abstracto, the constructivist view either in the strong form outlined above or in some of its more qualified but still erroneous versions. Nor are we obligated to do so: serious philosophers of science have been at it for decades. Nevertheless we record now, as scientists of long experience who have not been indifferent to philosophical questions, our emphatic rejection of that view. In our opposition, we undoubtedly have the concurrence of the majority of practicing scientists over a broad range of disciplines. Still, a sense of honor compels us to sketch at least one common argument against the constructivist view.

Consider how the theory itself is built up and defended. There is an obvious appeal to rather conventional veridical standards. A model of a phenomenon is proposed and given coherent logical form. Evidence for that model is adduced with every indication that it is evidence of a specifically factual kind.

This putative evidence is made to articulate with the presumptive model by means of arguments whose canons of logic and relevance are entirely exceptional. Inferences from the model—specifically, those inferences we have summarized as constituting the core of cultural-constructivist doctrine—are likewise arrived at by the presumed application of ordinary logic, that is, deduction. Thus, the cultural-constructivist case is brought into being by an intellectual process that implicitly accepts the same methodological paradigm as the empirical sciences it presumes to analyze.

This is not to suggest that the paradigm is particularly well served in this instance. In fact, the logic of cultural constructivists seems to us sloppy and full of holes in the matters under discussion, their evidence dubious, and their case corrupted by special pleading and covert appeals to emotion and the prejudices of a certain audience. These objections aside, however, we note that the very form of their argument makes the cultural constructivists self-subverting. They appeal to the same canons of judgment that their argument seeks to condemn.

There is, of course, a smug habitual rejoinder to the foregoing. We are challenged to consider that if in fact the empiricist logic that undergirds the sciences can be demolished by an application of the same logic, then there must be something wrong with empiricism in the first place. It, rather than cultural constructivism, must be the locus of self-contradiction. This is glib, but entirely unconvincing. It assumes that the arguments put forth by the cultural constructivists are airtight from both the formal and evidentiary point of view—a point, as we have said, that hardly needs to be conceded.

We invite the reader to judge where comparisons must ultimately be made. The cultural-constructivist thesis seems at first to be arguing against epistemological justifications of science, against attempts at finding foundations. Yet, inasmuch as the specific content of the thesis challenges the reliability of scientific conclusions—this is what it asserts in the final analysis, and not merely the inadequacy of foundational arguments—and inasmuch as it does so, roughly speaking, on the basis of the same argumentative paradigm as scientists use in practice, the logic, evidence, and pertinacity of the thesis must be weighed against that of specific scientific arguments.

In other words, in order to claim that they have made their case, cultural constructivists must demonstrate that their arguments for unreliability outweigh those of conventional scientific papers for reliability in the realm of phenomena addressed by the latter. They must show that their arguments are stronger than those put forth by Professor X in his paper on the role of transforming growth factor beta in the morphogenesis of the optic tectum, while simultaneously outweighing those of Dr. Y in his monograph on the classification of compact Lie group actions on real projective varieties! If they are to demonstrate that their arguments contra science are anything but sheer bluff, then clearly they must play on the scientists' court. At this point, we think a simple res ipsa loquitur is in order. There surely are scientific papers that are inaccurate for reasons more or less implicit in the cultural constructivist hypothesis, but these are on the whole rare and exceptional; they cannot be used to prove a hypothesis of such stupifying generality. To put the matter brutally, science works. 4

We do not present this as a complete critique. Many other lines of analysis are available. 5 But there is no need. The state of affairs is best summarized, probably, by the philosopher Paul Feyerabend, one of the thinkers directly responsible for initiating the chain of ideas leading to the cultural constructivist view of science (and, next to Thomas Kuhn, the most often cited), who now expresses deep reservations about the outcomes of this line of thought. "How can an enterprise [science] depend on culture in so many ways, and yet produce such solid results?" he asks. "Most answers to this question are either incomplete or incoherent. Physicists take the fact for granted. Movements that view quantum mechanics as a turning-point in thought—and that include fly-by-night mystics, prophets of a New Age, and relativists of all
sorts—get aroused by the cultural component and forget predictions and technology. 6

Whom does Feyerabend have in mind? Many, certainly, among those thinkers on the left end of the political spectrum who have developed a cultural constructivist (or relativist, or contextualist, or perspectivist) critique of science with a view to extending their general indictment of Western capitalist social structure. We turn to a series of examples, work well known in the community that views the cultural constructivist program as both path-breaking and fully credible. Our exposition must be brief, yet we hope that our objections rise to a higher level than mere amour propre.

Science as Power

A paradigmatic example of the constructivist program may be found in the recent work of Stanley Aronowitz, a sociological theorist whose wide interests reflect many of the concerns, trends, and attitudes prevalent on the academic left. Aronowitz has long been active in the causes embraced by the left; indeed, he has the distinction, almost unique among university-based theoreticians, of having worked in the trenches as a union organizer and shop-floor politician. He is a leading figure in the Democratic Socialists of America, an editor of such theoretical journals as Socialist Review and Social Text, and a ubiquitous presence at leftist symposia. His interest in science is relatively new but characteristically sweeping and ambitious, despite the fact that he has little formal training or technical facility in any branch of it. He is a professed admirer of Feyerabend, which makes it all the more ironic that Feyerabend's strictures fit him so well.

Aronowitz's major work on science is a turgid and opaque tract entitled Science as Power. It constitutes a major attempt to justify the cultural (or social) constructivist viewpoint and is clearly motivated by the belief that since science and technology are key elements in the substructure of modern capitalism, it is one of the duties of the oppositional social critic to demystify science and topple it from its position of reliability and objectivity. The major premise from which this work of demystification proceeds is that science is "situated" knowledge, conditioned by the historical circumstances that engender it and reflective of the ideological patterns of dominance and authority that prevail in the society.

Ambition, however, is one thing and achievement quite another. Aronowitz's book is notably clumsy in its approach to argument. Its chief method seems to be to invoke from the philosophy of science as many names as possible, in as small a space as possible, and to present their views, as paraphrased by Aronowitz himself, briefly and cryptically, cementing the whole business together, finally, with a wash of the author's pontifications. Very few specific positions are analyzed at great enough length to make them coherent; names and phrases are simply run in and out of the text as props for Aronowitz's views. This is done in a context emphatically illustrative of the pertinence of Feyerabend's remarks. Science as Power certainly does get highly aroused by the "cultural context" of science; it is intent on taking the development of quantum mechanics as a solemn turning point for Western science; it is unblushingly relativistic as to underlying philosophical doctrine; and it is, when all is said and done, incoherent.

We begin by considering the treatment of quantum mechanics and its philosophical implications. Aronowitz devotes an entire chapter, "History and Philosophy of Modern Physics," to this issue. Unfortunately, this treatment is greatly marred by the author's evident (at least, this seems the obvious inference from the text) ignorance of the particulars of physics. Despite the chapter's self-confident title, the reader who knows little of twentieth-century physics going in will remain, in point of specific knowledge, equally (or perhaps more) ignorant on his way out. 8 Here and elsewhere in the book what we get, instead of a pertinent history of contemporary (that is, seventy-year-old) physics, is a series of solemn and fetishized invocations of the uncertainty principle and, more generally, the quantum-mechanical challenge to classical determinism.

Now, the uncertainty principle is undoubtedly one of the cornerstones of quantum mechanics and one of the most philosophically provocative developments in the history of science. Under Aronowitz's description, however, it seems rather to refer to a kind of epistemological and spiritual malaise, plaguing the minds and souls of contemporary physicists. The argument, roughly but accurately paraphrased (and all too familiar from New Age tracts, among other things), is that since physics has discovered the uncertainty principle, it can no longer provide reliable information about the physical world, has lost its claim to objectivity, and is now embedded in the unstable hermeneutics of subject-object relations. This, alas, demonstrates depressingly well the connotative power of words when they are allowed to drift apart from their contextual meaning. If Heisenberg and company had chosen a less evocative term, an awful lot of nonsense of this sort might never have seen the light of day. Philosophical and pseudosophistical posturing has dreadfully befuddled discussion of the issue addressed to nonspecialists.

Once obscurantism has been stripped away, we recognize that the uncertainty principle is a tenet of physics, a predictive law about the behavior of concrete phenomena that can be tested and confirmed like other physical
principles. It is not some brooding metaphysical dictum about the Knower versus the Known, but rather a straightforward statement, mathematically quite simple, concerning the way in which the statistical outcomes of repeated observations of various phenomena must be interrelated. And, indeed, it has been triumphantly confirmed. It has been verified as fully and irrefutably as is possible for an empirical proposition. In other words, when viewed as a law of physics, the uncertainty principle is a very certain item indeed. It is an objective truth about the world. (If that were not so, there would never have been so much fuss about it!)

Aronowitz’s incoherent account completely obfuscates that simple fact. He insists on adverting only to the most mystical views of the matter (those of Heisenberg qua philosopher-oracle, for instance) and ignores the particulars of the lively debate among physicists attempting to clarify what the predictive success of quantum mechanics really tells us about the physical universe. He naively echoes, for example, the view that the causal and deterministic view of things implicit in classical physics has been irrevocably banished. This is simply wrong.9 He propounds, moreover, the undocumented and egregiously unlikely notion that the source of these developments lies in a general malaise that afflicted European culture in the wake of World War I. On Aronowitz’s account, the pioneers of quantum mechanics were merely clever artificers obedient to the society’s preemptory demand for an abolition of determinism and causality. Genuine familiarity with the history and content of the work of Heisenberg, Schrödinger, von Neumann, de Broglie, and others makes such a proposal hallucinatory.

Aronowitz’s treatment, in short, gives no indication that he really understands the underlying physics and mathematics of the situation. He seems to be expostulating on the basis of dilute paraphrases or worse, vulgarizations of paraphrases. It undoubtedly seems snobbish to say so, but this field of speculation is notoriously unkind to amateurs. Feyerabend has in fact understated the case—getting aroused about the cultural component of physics has not only led Aronowitz to forget about technology and prediction, but has induced him to ignore the physics as well.

At a later point in the book we find the dictum cited as an epigraph above: “The point is that neither logic nor mathematics escapes the ‘contamination’ of the social.”10 The argument leading to this emphatic conclusion precedes it and gives us an excellent opportunity to examine the coherence of Aronowitz’s own logic, approvingly borrowed from a certain David Bloor. Bloor, a sociologist identified with the Edinburgh School of unyielding cultural constructivists, points out that by report of the anthropologists, certain tribal peoples reason thus: Everyone, without exception, is a witch; on the other hand I know lots of people who aren’t witches. Bloor and Aronowitz deny that this is a logical error; it is merely the logic of another culture! Aronowitz goes on to insist that our own culture has its own weird logic, largely unacknowledged. Here is what he supposes to be a telling example: We define murderers as people who deliberately kill people; bomber pilots deliberately kill people; yet we deny that bomber pilots are murderers. Thus our own culturally generated logic is skewed and is not the disinterested thing claimed by defenders of the idealized autonomy of mathematics!

The confusions evident in this passage are comical. Observe, first of all, that in this culture we don’t define murderers simply as people who deliberately kill people. A woman who stabs and kills a potential rapist is not a murderer in this culture, nor is a shopkeeper who dispatches a holdup man with the .38 he keeps under his cash register. A psychotic who kills a nun under the belief that she is a five-foot penguin is not a murderer either. Nor is a member of the military who, under official orders, kills large numbers of people, including innocent civilians. Indeed, in the last example, the person becomes a criminal if he refuses to kill other people. From an ethical or religious viewpoint, it is quite possible to quarrel with this exemption from the category ‘murderer’; but that dispute is about ethics: formal logic has nothing to do with it!

On the other hand, examples of the error made by the cited tribesmen can easily be found in this or any other culture. An anthropologist from the Death Planet, in disguise as one of us, recruiting informantists from the shoppers at a typical suburban mall, will surely find someone who says something like the following: All politicians are crooks, without exception; President Clinton is certainly a politician; but President Clinton is not a crook since he’s the commander-in-chief. The point of course is not that logic (and therefore mathematics) is culturally contaminated, but that cogent, self-consistent, logically coherent thinking is not ubiquitous. Many people shrink from it, in fact. It is an art and a skill that must be mastered; and it requires patience, diligence, humility, and intellectual energy. That it is not universally practiced is a fact that does nothing to uphold Aronowitz’s thesis.

There is a further point to be made about Aronowitz’s argument, however—rhetorical rather than logical. The example about the bomber pilot is advanced precisely on account of its capacity for moral intimidation. The lurking suggestion is that if the reader fails to accept this argument, then he is implicitly condoning the bombing of innocent, Third World civilians by imperialist U.S. pilots. In left wing circles, such forensic flim-flam has con-
siderable weight, despite its being worthless in logical terms. This sort of trick alone accounts for much of the popularity of cultural constructivism on the academic left.

Apart from his book, Aronowitz continues to speak out on questions regarding science. His newest statements do not reflect a chastened or cautious mood. He remains a true believer in the strongest form of the cultural constructivist dogma. A recent lecture, for example, was studded with bizarre pronouncements unsupported by evidence or plausible argumentation, but delivered with that combination of blithe self-assurance and moralistic bullying that audiences with strong political sentiments but weak backgrounds in science find so intoxicating—or exhilarating. To take but one example from among several dozen—and we stress that it is entirely typical in its pugnacious illogic—we hear Aronowitz denounce the idea that Einstein conceived special relativity by sitting around the cafes of Bern and meditating upon the Michelson-Morley experiment. It is an idle legend, Aronowitz asserts, a mystification designed to conceal how the social, political, and economic currents of late-nineteenth-century European capitalism and imperialism brought to birth the theory now famously associated with Einstein. Similarly, he tells us how the development of celestial mechanics by upper-class gentlemen-scientists such as Newton cannot be understood apart from an analysis of seventeenth-century mercantilism. Aronowitz is probably right on a couple of points—but for reasons that do him no credit whatsoever. As it happens, it is most likely that Einstein did not know of the Michelson-Morley experiment at the time he was working on special relativity. Aronowitz is childish wrong on other points (the class origins of Newton, for instance). But these are minor matters. What is staggeringly silly is the essence of his thesis.

The history of calculus, mechanics, and relativity and the biographical details of Einstein's and Newton's lives have been studied and documented at sufficient length for us to know very well that the "Bern cafe" myth is true in its essentials, as is the story of the solitary Newton's incredible burst of creativity during the Plague Year, 1665, when he invented modern mathematics and modern physics simultaneously. This is apparently not good enough for Aronowitz; for him, it is vital that both these achievements be seen somehow to encode, in a quite literal sense, ideological and social perspectives. He never makes it clear how this is supposed to have worked. Neither his book nor his lectures set forth a coherent account of the putative transcription process.

What is needed to make a theory of this sort even weakly plausible is a demonstration of specific intellectual correspondences between the details of the physics and the hypothetical complex of "social" or "economic" attitudes. As well, some kind of argument would have to emerge that these correlations were at least comparable in importance to the internal logic of physics and mathematics as it influenced Einstein's thought or Newton's. No such case is to be found in Aronowitz's work, if only because the physics is terra incognita to him. What takes its place is the arrogance of the dogmatist. This is a common failing in cultural constructivist histories of scientific achievement.

The notion that such work as that of Newton and Einstein was "needed" by the technological infrastructures of their respective societies is plain nonsense. Seventeenth-century merchants and navigators needed innovation in the form of an accurate chronometer, not an explanation of Keplerian ellipses in terms of the inverse square law. Turn-of-the-century industrialists were not sending out desperate requests for a more subtle understanding of the invariance of physical law under change of inertial frame. To offer this sort of "explanation" as an account of profound intellectual developments is to show unlimited contempt for the very notion of explanation, as well as a boundless ignorance of the phenomena one is trying to explain. Aronowitz's thesis is no more than an unsupported dictum that declares, in effect, that by some mystifying process, the Zeitgeist Fairy of 1665 contrived to tickle Newton's brain cells with her magic wand, while her counterpart of 1905 did the same for Einstein! This is not intellectual history, sociology, philosophy, or anything else worth a scholar's serious attention. We leave the last word on Aronowitz's work to Michael Sprinker, a sophisticated Marxist who still hopes to retain a place for Marxist notions in discussions of the philosophy of science. He has the good sense to understand that this involves putting oneself at a considerable distance from the naive and uninformed constructivism evinced in Science as Power. His caustic review of that book goes to the heart of the matter. "If we are to doubt the findings of the empirical sciences, we need to be given better reasons than that they have arisen from and been a necessary adjunct to capitalist social relations."

**Explanation**

In general, even when cultural constructivists make a more serious effort to put forth an account of what is supposed to go on during this process of cultural construction there is a strong flavoring of circularity. It is assumed ab initio that cultural construction has taken place. Thereupon, the historical and scientific record is subjected to a strained and arbitrary reading that decodes it with the help of a great deal of interpretive contortion and hermeneutic ko-ko-ko. At last an account is produced that "explains" how
the culture has constructed the theory. This is then put forth as a confirmation of the cultural constructivist hypothesis. In form and soundness, this procedure closely resembles the methodology through which the tenets of psychoanalysis are "confirmed" by the interpretive prowess of the psychoanalyst. In both instances, circularity and special pleading rule the day, and little worthy evidence emerges.

In saying this, we are not trying to deny that social interests and nonscientific belief systems often enter into the very human business of doing creative science, sometimes to catalyze the process, more often to retard or deflect it. The work of Stephen J. Gould (who must be recognized as holding strong leftist views) is replete with incisive essays on examples of this, presented in minute detail. But Gould's well-informed work is by no means comparable to the cultural constructivist program. Gould knows perfectly well that in the long run logic, empirical evidence, and explanatory parsimony are the masters (with apology to our feminist friends for the metaphor) in the house of science. In this he echoes Thomas Kuhn, whose work has so often been vulgarized and distorted by the cultural constructivist school.

Cultural constructivism, at least in the full-blooded version of ideologies like Aronowitz, is a relentlessly mechanistic and reductionistic way of thinking about things. It flattens human differences, denies the substantive reality of human idiosyncracy, and dismisses the ability of the intellect to make transcendent imaginative leaps, in a way that O'Brien, 1984's master manipulator of consciousness, would cheerfully approve. According to the constructivist canon, all are puppets of the temper of an age, and science is just another inadvertent ratification of its ideological premises. Only the cultural constructivists themselves (of course) are licensed to escape the intellectual tyranny of this invisible hand. For their part, mathematicians, physicists, chemists, and biologists must all succumb.

Aronowitz represents cultural constructivism with all its philosophical and political cards on the table, so to speak. His program is maximalist in both respects and forthrightly asserts its prescriptive ambitions even as it makes its sweeping descriptive judgments. Other theorists and publicists of the constructivist school are more circumspect in their claims and cagier in their tactics. They are content, for the time being, to conduct an irregular guerrilla war on behalf of their doctrine, while Aronowitz insists on undisguised frontal assault. Typically, in the face of all-out challenges from scientists and philosophers armed to do intellectual battle, they edge away from the strong version of the constructivist claim and retreat to the proper territory of sociology or history. In the presence of a different audience, one primed to hear science contextualized, relativized, and revealed as the deformed offspring of capitalist hegemony, the constructivist claws come out once more.

Science as Power Struggle

In this respect we cannot avoid citing the work of Bruno Latour, a sociologist, anthropologist, and social philosopher whose work on science as social practice has been as much of an inspiration to the constructivist camp as that of Thomas Kuhn. In contrast to Kuhn, however, this does not reflect any inadvertence on Latour's part. He clearly relishes his role as self-appointed heretic and gadfly. His reputation and the substance of his claims rest on his record as an "anthropologist" of science, who does fieldwork at research facilities rather than among the denizens of New Guinea. He is not loath to let it be known that he has brought back amazing tales from his sojourn among the troglodytes. He claims, with no particular modesty, to be the first modern thinker to discover what scientists actually do, as opposed to what they say they do or think they do. His tools are those of the microsociologist; in his primary research he concentrates on small groups and personal interactions in which quirks, prejudices, and local hierarchies obviously play a role. Consequently, unlike theorists such as Aronowitz, who derive from a Marxist tradition that reifies such grand abstractions as "relations of production" and who impute far-reaching powers to them, Latour works in the interpersonal tradition of Erving Goffman and his disciples, as well as that of field anthropology in the classical mode.

What provokes and titillates in his work is that he places full-fledged members of the scientific and technological elites in the "object-of-study" position usually reserved for inhabitants of the Trobriand Islands or the headwaters of the Orinoco. For Latour, the Heart of Darkness is the solid state physics laboratory. Notwithstanding the specificity and locality of his direct investigations, Latour is eager to emerge with far-reaching generalizations and epistemological laws. These are embedded in an expository style as unconventional as the theses it propounds. His major work, Science in Action, is studded with aphorisms, diagrams, cartoons, and doodles, and is characterized by a mercurial, gnomic wit; but his purpose is seriously iconoclastic. Here, for instance, is his "Third Rule of Method": "Since the settlement of a controversy is the Cause of Nature's representation, not the consequence, we can never use the outcome—Nature—to explain how and why a controversy has been settled." This would seem to be an instance of unbending relativism and antirealism. What it seems to say is that nature is purely a social
convention, and that scientific controversies are settled by a dialogic process within a scientific community resulting in a general agreement about the details of that convention. Thus, to read this as it applies to a concrete situation, we must believe that William Harvey’s view of the circulation of the blood prevailed over that of his critics not because blood flows from the heart through the arteries and returns to the heart through the veins, but because Harvey was able to construct a “representation” and wheedle a place for it among the accepted conventions of the savants! In other words, it is not to be admitted that nature might provide a template in conformity to which these “representations” are tightly molded.

A homely example will serve to clarify this point. Imagine that a few of us are cooped up in a windowless office, wondering whether or not it’s raining. Opinions vary. We decide to settle the issue by stepping outside, where we note that the streets are beginning to fill up with puddles, that cars are kicking up rooster-tails of spray, that thunder and lightning fill the air, and, most significantly, that we are being pelted incessantly by drops of water falling from the sky. We retreat into the office and say to each other, “Wow, it’s really coming down!” We all now agree that it’s raining. Insofar as we are disciples of Latour, we can never explain our agreement on this point by the simple fact that it is raining. Rain, remember, is the outcome of our “settlement,” not its cause! Baldly put, this seems ridiculous. Nevertheless, if we accept the validity of Latour’s putative insight, we are ineluctably obliged to accept this analysis of a rainy day.

It is clear that a rather light-footed style is needed to get away with such stuff, which drives more earnest and responsible philosophers of science into paroxysms of disgust when confronted with it. (Scientists themselves, less oppressed by a professional obligation to grapple with every piece of gaudy nonsense that comes down the highway, simply go about their business.) The idea that Latour’s reports on the activities of scientists are to be accorded factual status, while scientists’ reports on nature are not, involves a metaphysical conceit (in both senses of the word) of astounding proportions. True, no one can object to the observation that the world of science is a human world, and that “laboratory politics” plays a significant role in it from time to time—although this is hardly a new insight. But Latour’s sanctimonious insistence that such politics accounts for science as such and is the real story behind the emergence of scientific theories is in itself a signal instance of politics dictating theory.

Latour’s picture of science is bleak and ominous: a war of all against all! Science is presented as a savage brawl in which, from day to day, the dominant chieftain is he who assembles, by dint of wealth, prestige, and warrior cunning, the biggest and nastiest gang of henchmen (i.e., a “network,” in Latour’s parlance). We must remind ourselves—with a pinch if necessary—that this process is alleged to account for the emergence of celestial mechanics, Maxwell’s equations, the periodic table of the elements, plate tectonics, the genetic code, algebraic topology, quantum mechanics, massive parallel processing, and a million other insights and advances, modest as well as exalted. Empirical verification is dismissed as a species of bluster, or as a kind of collective hallucination of the power-crazed.

From the example above, it is easy to see why Latour has frequently been classified as an unreconstructed constructionist. Nonetheless, to say this is to miss an important aspect of his intellectual cunning and his seductive charm. Latour is always ready to recast and, in effect, retract what he has previously said. In other contexts he will, with an apparently straight face, admit that there is a natural universe “out there” and that scientific theories are shaped by it in important ways. Simultaneously, he will censure rigorously the dogmatics of strict cultural constructivism. Just as he pictures (literally) the mind-set of science as a Janus-faced dualist, he too is constantly springing from one side of a dichotomy to the other.

An interesting instance comes directly to hand in connection with the very example (used by Stanley Aronowitz) we evaluated above, that of the supposed parity between the tribal people who can’t reason syllogistically from the proposition that “everyone is a witch” and Westerners who exhibit the same deficiency with respect to the proposition that “all who kill are murderers.”20 Latour criticizes the analysis on which Aronowitz relies, and he does so by dint of arguments that overlap, in most ways, the ones we advanced above.20 He acquires the Westerners of illogicality on the very same grounds we cite. In fact, he goes further and acquires the “primitives” as well, asserting that it is the anthropologist’s ignorance, rather than that of his subjects, that engenders the example! On this view, the ethnographic researcher has been guilty of insufficient familiarity with the nuances of the culture, of the implicit subtleties and unstated exceptions that his informants tacitly invoke when they discuss certain cultural categories. Thus, to some extent at least, Latour seems to be arguing that canons of logic really exist, and—though one might regard this as rather hopeful—that all sorts of people are reasonably good at adhering to them. Understandably, one might tend to view this position, of itself, as an argument contra relativism. But then, putting a characteristically paradoxical twist on the matter, Latour pushes on to the conclusion that people are hardly ever irrational! In particular, he argues that refusal to accept scientific argument or evidence is virtually never grounds for impeaching a position or those who hold it as “irrational.” Thus his so-called Fifth Princi-
ple: "Irrationality is always an accusation made by someone building a network over someone else who stands in the way; thus, there is no Great Divide between minds, but only shorter and longer networks; harder facts are not the rule but the exception, since they are needed only in a very few cases to displace others on a large scale out of their usual ways." The net result, of course, is that the most indulgent relativism is now back with a vengeance, and adherents of faith healers, palm readers, cancer quacks, and "creation scientists" may now go their way—courtesy of Latour and the Harvard University Press—in the full assurance that they are every bit as rational as their scientific critics.

This example illustrates an important aspect of current intellectual life, especially among the trendier doctrinal movements to which the academic left has proved susceptible. Self-consistency is no longer considered to be much of a virtue; and logical coherence, in the version that working scientists are obliged by their peers to honor, is viewed as a chimera. One must understand that a large part of the reason for Latour's success and celebrity is rhetorical. He provokes and challenges with his insistence on paradox and contrarian whimsy. His reader is constantly reminded that to reject Latour's maxims is to mark oneself as hopelessly stodgy, humorless, and tradition bound. It is no accident that his style stands in such contrast to the single-minded, rather ponderous linearity of the papers and monographs of the scientists he studies. He is, despite his proclaimed fascination with science and technology, a Panurgical imp, come to catch all those solemn scientists with their pants down, a project that delights his largely antiscientific audience.

Questions must be raised, however, not only about the depth and accuracy of Latour's claimed insights, but about the soundness of his own observational technique. This is best illustrated, in our view, by lectures, dealing with his work, sponsored by the French government, on a social analysis of the "Aramis" project. "Aramis" was the glamorous code name for an ill-fated attempt to build a high-tech public transportation system for Paris. The basic idea involved the construction of an elaborate network of trackage, full of switches and crossovers. This were to accommodate not trains but a fleet of six-passenger self-propelled tram cars, each controlled by computer. The idea was that the traveler, upon entering a station, was to signal his presence and his destination to the central computer. Thereupon, a nearby car with a compatible itinerary would come by to pick him up and carry him to his target station, picking up and dropping off other patrons along the way as convenience and capacity dictated. Thus, Aramis was a kind of automated valet service.

Attractive as the idea might be on paper, Aramis was from the start a troubled and problem-plagued project. Yet it continued under high-level funding for about a dozen years before being supplanted by a much more conventional urban railway system. Why was it so durable? For much the same reasons that any boondoggle hangs on long past the point at which its wastefulness becomes clear. It provided jobs for technical and industrial workers, research funds for high-tech consortia, power for bureaucrats, and photo-ops for politicians eager to show their commitment to a high-tech future. One would think that a highly politicized sociologist like Latour would positively salivate to sample the delights of this particular pork-barrel, and to tell us where all the money went. But he barely touches the issue; certainly not enough to embarrass anyone. By the usual muckraking standards, this is a pretty poor performance.

Why did the project fail? To us there seem to be two obvious reasons (there may be many others, of course). The first is technical. In a proposed system like Aramis, the chief headache will inevitably be software. Real-time algorithms must be devised for running such a system efficiently, which means minimizing station-to-station travel time for each passenger, maximizing utilization of each car, and avoiding the sort of instabilities that cause cars to bunch up in one region, leaving others bereft of service. This is a formidable undertaking! It involves all the notorious difficulties of the "traveling salesman problem," the paradigmatic holy grail of combinatorics and operations research. Compared to this, the "hardware" problems of building trackage and computer-controlled cars are trivial. How does Latour deal with this (or any other relevant technical question)? He ignores it completely.

There is, on the other hand, an important social reason for the impracticability of Aramis that the reader without any technical background will recognize immediately. Picture yourself riding, late at night and all alone, in one of these little Aramis cars. It stops at a station and two men get on, nasty-looking types with what look like ten-inch lengths of lead pipe bulging in their pockets . . . We rest our case. What has the sociologist Latour to say about such inherent problems of social interaction? The matter does not seem to have occurred to him at all.

Well, what, then, does interest Latour about Aramis? He seems very excited about the semiological aspects of the thing, the fact that "information" and "control" are such important metaphors for it. He is intensely aroused by the fact that the cars are to be connected by "information" but not "physically," as though he'd never heard of radio-controlled toys. He studies the evolving shape—even the color—of the prototype cars as aspects of the social representation of technology. In short, he indulges in all the ex-Gallic, jargonistic mumbo-jumbo about signification, and about social metaphor,
that the devotees of cultural criticism have come to expect, without saying much of interest about the scientific or social reality. Eventually, his epistemological conceits emerge, decorated with the usual doodles and diagrams. It is hard to see what they have to do with Aramis—or any other episode in the history of science and technology—but they are very dear to Latour.

Some of the glaring gaps in Latour's analysis of the Aramis project are characteristic of his work as a whole. Mathematics is a symptomatic weak point of his. His discussion of Aramis avoids it completely, as we have seen, but, even worse, his discussion in *Science in Action* of the mathematical nature of scientific theories, and the invocation of formal mathematics in order to express them, is naive and obtuse—he has a tin ear for mathematics. His account completely fails to resonate with the thought of mathematical scientists—a term that goes well beyond those formally described as mathematicians—and is deaf to how they reason with and persuade each other. The one reference to an actual piece of mathematical research manages to misunderstand an anecdote utterly. The brief discussions of correlation coefficients and Reynolds numbers are mere occasions for sneering that completely avoid serious engagement with the deep, and enormously fruitful, concepts involved. Indeed, Latour fervently minimalizes and trivializes formalization, abstraction, and mathematization. His discussion of the matter is a series of flippancies, whose intended point is that the deep and surprising predictions about the real world that emerge from exacting logical analysis of abstract models are really no more than tautological parlor tricks. Here, Latour's resentment of science seems to become overpowering. It should hardly need saying that this stubborn inability to deal accurately, comprehensively, and honestly with this central and most characteristic aspect of modern science effectively disbars the most grandiose claim of Latour's book—that it instructs the sociologically sophisticated "how to follow scientists and engineers through society."

We recall Latour's own imprecations against the anthropologist who failed to grasp the nuances of a tribal people's categories of "witchhood" and "non-witchhood." His own evident failure, as a would-be "anthropologist of science," to grasp the categories in which scientists think and through which they judge and decide convicts him of a similar offense, and on a much larger scale. Latour's work is thus a very inadequate prop for any radical attempt to rethink scientific epistemology, or to indict science for unwitting relativism or perspectivism. Its appeal is almost wholly a matter of style, not of substance. It is a prime example of Feyerabend's cantankerous description.

---

**Plutocrats**

Cultural constructivist theories of science have lately infested the usually staid domain of the history of ideas. One well-known example is the work of Shapin and Schaffer, whose book *Leviathan and the Air Pump* has a wide circle of admirers. This work is rather more orthodox, on a superficial level, than Latour's. It is an intellectual history of some of the resounding disputes that surrounded the birth of "experimental" science—physics in particular—in the last half of the seventeenth century. What particularly concerns Shapin and Schaffer is the quarrel between some of the most prominent founders of the Royal Society—Boyle, Hooke, and their circle—and the philosopher Thomas Hobbes, author of *Leviathan*. This is the fulcrum upon which they attempt to push the case that, contrary to its flattering image as a uniquely wide-open and tolerant enterprise, welcoming of all new facts, information, and ideas that bear upon its investigations, modern science has been from the first the province of a tightly organized, well-insulated coterie, jealous of its prerogatives and hostile toward outsiders who intrude without the proper credentials. Moreover, this self-appointed scientific aristocracy is seen as organically connected to the ruling elite of Western society. Its views are derived, albeit subtly, from the dominant metaphors of that elite. By the same token, its prestige, authority, and epistemological monopoly are guaranteed by the power of the state and the social formations it principally serves. The argument between Hobbes and the adherents of the Royal Society is offered as an instance of this phenomenon:

The restored regime [i.e., that of Charles II] concentrated upon means of preventing a relapse into anarchy through the discipline it attempted to exercise over the production and dissemination of knowledge. These political considerations were constituents of the evaluation of rival natural philosophical programmes [i.e., that of the Royal Society's experimentalists, as opposed to the a prioristic rationalism of Hobbes].

Thus the disputes between Boyle and Hobbes became an issue of the security of certain social boundaries and the interests they expressed.

The heart of the matter, as far as Shapin and Schaffer are concerned, is that the confrontation illustrates the degree to which Boyle and his friends were concerned not only with scientific issues, in the narrow sense, but also with the question of credentials. Their supposedly empirical rules, it is said, constituted a specific social practice. They were preoccupied with the question of who should count as a scientific authority, whose judgment was to be respected in scientific disputes, whose evidence was to be accepted as reliable,
whose minds were to be acknowledged as sufficiently unpolluted by common prejudice that their observations could be taken at face value.

If we are to believe the Shapin-Schaffer thesis, worthiness to participate in learned discussion of experimental philosophy was closely correlated to rank, wealth, religious orthodoxy, and, in terms of Restoration doctrine, political reliability. This exclusivity was reinforced not only by the money, status, and political connections of many of the members of the Royal Society and their patrons, but in addition by their exclusive possession of the physical instruments of the new experimental method. The air pump of the title was not a common device. Only a handful existed during the 1660s, and thus the possibility of investigating experimentally the emerging theories of the weight and pressure of gases, now associated with Boyle's name, was limited to the corresponding handful of people who had access to one.

Hobbes, ever the gadfly and eager controversialist, was only too happy to point out this flaw in empiricism. The viciousness of the response to his challenge is to be explained not simply by the theoretical threat it posed to the self-assumed authority of Boyle, Hooke, Oldenburg, and the rest, but, as well, by Hobbes's dark reputation as atheist, philosophical materialist, and general subverter of the sanctity of authority. He was the natural target of distrust because of his lingering reputation as a duplicitous sycophant, willing to flatter either crypto-Catholic king or radical Protestant regicide as the opportunity of the moment suggested. Even more important was his enmity toward religious orthodoxy, and therefore toward the stability of a hierarchical society. The attempt to exile him from the realm of Natural Philosophy therefore must be seen as an act of political prophylaxis.

On this, the Shapin-Schaffer view, the nascent Royal Society was, from the first, the creature and deputy of a political and social viewpoint. The society's supposedly objective science is thus to be read, in large part, as a construction of its ideological commitments, which rejected simultaneously the republican sentiments and leveling enthusiasm of the most radical Puritans and the unconstrained absolutism of the Stuart monarchy. Shapin and Schaffer accept the idea that Hobbes was identified with both kinds of threat. As a defender of absolutism, he could be read as the proponent of a government of unconstrained sovereign power. Yet his fierce independence, which devolved at times into a taste for rancorous disputation, was reminiscent of the intellectual licentiousness of the religious and social radicals of the Civil War period. Given this perspective, the scientific community led by Hooke and Boyle, which echoed the aspirations of a moneymade class that sought immunity from the whims of royalist autocracy while casting a sus-
picious eye on the tumultuous mass of the unpropertied, had no place for the likes of Thomas Hobbes.

It is not hard to transcribe this view to a contemporary context, as Shapin and Schaffer undoubtedly wish us to do. The analogies are clear. Modern orthodox science is also obsessed by "credentials" in the shape of formal training, academic degrees, and a long period of acclimation to the reigning "paradigms." It polices dissidence and safeguards its monopoly by an elaborate educational system and a forbidding insistence on "peer review." It flourishes with the connivance and support of the organized forces of wealth and authority as constituted in the state, in huge corporations, and in supposedly philanthropic foundations. It has exclusive control over the instruments of empirical investigation, some of which—like multibillion dollar particle accelerators and orbiting observatories—are far less accessible to the uninitiated than was Boyle's air pump. And it has its heretics.

Here is Shapin and Schaffer's last word on the general epistemological principle that their particular historical study is supposed to illustrate: "As we come to recognize the conventional and artificial status of our forms of knowing, we put ourselves in a position to realize that it is ourselves and not reality that is responsible for what we know." So, in the end, we come back to the dichotomy—fallacious in that it posits total opposition between "reality" and "convention" where there is, in fact, intense and continuing interaction—so favored by Latour and other constructivists.

The questions raised by Leviathan and the Air Pump are serious and genuine. No intellectually astute history of the interplay between science and its supporting social matrix could afford to ignore them. The flaw, however, lies in attributing a deep and irreducible source of error to what is ephemeral, local, and inconsistent in its operation. Let us examine the particular picture of seventeenth-century scientific life offered us by Shapin and Schaffer.

Were the panjandrums of the Royal Society really so rigid and intolerant in deciding who was to be accepted as a Natural Philosopher in good standing? Was it true that "the social order implicated in the rationalistic [i.e., a prioristic] production of knowledge threatened that involved in the Royal Society's experimentalism?" It's hard to believe! Recall the roster of thinkers, Continental and English, who were heard with deep respect in the scientific and philosophical debates of the period: In addition to Boyle and Hooke, we have Descartes, a French Catholic; Spinoza, a lapsed Jew doubt

stant of fanatical intensity. Newton's singular religious views, be it recalled, prevented him from seeking ordination in the Established Church, a step he diplomatically avoided by becoming Lucasian Professor at Cambridge. Newton was, in fact, an abjurer of the doctrine of the Trinity and thus from many points of view a heretic. He was a hostile, secretive, jealous recluse suffering intermittently from mental instability, an unrelinquing enemy to the Stuart monarchy in its attempts to sponsor Catholic scholars at Cambridge (thereby opposing its attempts to exercise "discipline . . . over the production and dissemination of knowledge"). To top it all, he was probably homosexual.

Yet consider the celerity with which he was not only embraced but virtually deified by the English intellectual elite, once it became clear that his incomparable mathematical skills had led him to those insights into the nature of physical reality that to this day remain staggering to comprehend. Consider, in particular, the rather touching story of the publication of the Principia. It will be remembered that Halley had to drag it out of Newton by main force (imagine a comparable situation involving a contemporary scientist). And Halley, a man of no wealth, put up his own money to see the work through press, taking his compensation in the form of copies, which he had to sell himself. Recall, once more, that Halley was, in fact, an atheist, while Newton, on his own testimony, hated atheism above all things! Clearly, there is more to be said about rigidity and latitudinarianism, intolerance and liberty of opinion, in the seventeenth-century scientific community than that the Royal Society constituted a kind of thought police.

Consider again the question of Hobbes’s banishment from the circle of the scientific elect. How accurate and complete is Shapin and Schaffer’s analysis of the dispute between Hobbes and his foes in the Royal Society? From time to time, they advert to Hobbes’s drawn-out fight with the Oxford mathematicians Ward and Wallis, as though its technical aspects were peripheral to their central thesis. They note the existence of the acrimony, and the readiness of the devout Wallis to bring Hobbes’s ostensible irreligion into it; but they say nothing about the mathematical substance, claiming that it would carry them too far afield! But of course this is a central and highly illuminating question!

Hobbes, be it recalled, had little mathematical training in his youth. He took up the study of Euclidean geometry for the first time in his forties (mathematicians are often said, with some justice, to be washed up at the age of forty) and was an old man at the time of these controversies. Wallis, on the other hand, was, aside from Newton himself, the greatest English mathematician of the seventeenth century. A partisan of Parliament during the Civil War (in anticipation of Alan Turing, he served as code breaker for the Puritan forces), Wallis was an ordained cleric, though of Presbyterian, rather than radical Puritan, leanings. He opposed the execution of Charles I, however, and migrated politically to a position of support for the Restoration settlement. Politics and theology aside, Wallis was a superb, creative mathematician, in contrast to Hobbes, who was, to put it bluntly, incompetent—utterly out of his depth in dealing with subtle mathematical matters.

The controversy between Hobbes and Boyle on the weight and pressure of air must be viewed to a considerable extent as an episode in the twenty-year wrangle between Hobbes and Wallis. It was Wallis who published the most pointed rejoinders to Hobbes, not Boyle himself. The animus between the old philosopher and the Oxford mathematician had arisen from Hobbes’s futile criticism of Wallis’s mathematics—particularly his great work on infinitesimal series—which antedates the “experimental philosophy” dispute by a number of years. Subsequent to the attack on Boyle’s physics, Hobbes once again turned his guns on Wallis’s mathematics. But the most revealing, as well as the most comical, quarrel arose when Hobbes published his incorrect solutions to the ancient problems of “squaring the circle” and “duplicating the cube.” Wallis, of course, demolished the poor old philosopher’s pretensions, and Hobbes compounded the sin, in the eyes of posterity, by being unable (or unwilling) to see the point of Wallis’s refutation.

The relevance of these facts to the Shapin-Schaffer hypothesis is that this long and (to Hobbes’s admirers) lamentable history provides a concrete and substantive reason, in contrast to an ideological one, for Hobbes’s Notariety in scientific circles. So far as mathematics is concerned, Hobbes was simply dead wrong in these exchanges, as any competent mathematician would have seen. It is then no wonder that his authority to pass judgment on scientific matters was not well regarded, even if those matters had nothing directly to do with squaring the circle or the like. He was, after all, a strenuous advocate of a rational-deductive methodology based on that of synthetic geometry, as an alternative to the emerging experimental empiricism. Shapin and Schaffer emphasize this fact, but unaccountably fail to link it to the question of Hobbes’s doubtful mathematical competence. His grotesque failures as a would-be geometer, however, can hardly have been irrelevant.

Leviathan and the Air Pump would have been a rather different book had it addressed these matters directly. The image of Hobbes as brilliant and devastating iconoclast would have taken some hits, at the least. Moreover, Shapin and Schaffer would have put themselves in the position of conceding the existence of sound, objective reasons for deciding at least some scientific controversies—that between Hobbes and Wallis being an important case in point. Inevitably, they would have been led to concede that there are reason-
ably valid criteria for deciding the scientific competence of individuals, for distinguishing, in most instances, between worthwhile theorists and cranks. After all, in terms of mathematics, Hobbes was a crank. Such concessions, however, do not sort well with a relativist or conventionalist position, especially one grounded on a radically antileftist politics. Shapin and Schaff has sidestep the issues that might entail such admissions by insisting that all such disputes are ideological.  

Leviathan and the Air Pump is exhaustively and meticulously researched as a narrative of events and personalities during a short span of time. Nonetheless, the ideological perspectives of its authors make it an exercise in tunnel vision. To concentrate on the idea of empirical science as a manifestation of cultural and political imperatives is to omit important dimensions of the story, both human and philosophical. The efforts of Boyle and his colleagues to put science on a solid experimental footing and to restrain the impulse toward a priori speculative systems was a project facing substantial practical difficulties at that stage. It is one thing to embrace "empiricism" in the abstract, quite another to find practical and reliable methods for developing and extending concrete knowledge. The early experimental philosophers were confronted with the necessity to minimize the effects of human fallibility and bias, and it is shortsighted to condemn them out of hand for addressing the difficulties in language that occasionally smacks of snobbish or political insecurity. The verdict of history must be that they succeeded magnificently in sketching the broad methodological outline by which the physical and biological sciences have attained their present scope and power. To put it another way, irrespecive of the "social" grounding of their ideas, what more could they possibly have done, short of inventing the theory of experimental design and developing the techniques of mathematical statistics and error theory that underlie it?

Furthermore, it is false to read their rejection of Hobbes as a blanket denial of the value of speculative and deductive thought. Such reasoning was eagerly received when it was the product of genuine intellectual competence, as in the case of Huygens and, of course, of Newton himself. The singular genius of the period was to exploit new and powerful mathematical reasoning in the service of physical science without falling into the trap of contempt for mere experience. The authors concede that they have not quite come to understand how the experimentalism of Boyle was made to dovetail with the mathematical science of Newton; but this may well be because they have been celebrating the wrong hero. Hobbes, the mathematical dilettante and bumbler, simply does not belong in the same pantheon with Descartes, Huygens, Newton, Leibniz, and Bernoulli. His misadventures are tiresome and, in the last analysis, uninformative.

A final word about the rhetoric of the book: Once more, we find an argument designed to appeal to a certain kind of readership on grounds other than strict logic and evidence. To side with Boyle and the Royal Society crowd, as the book presents them, is to side with snobbish, purse-proud, rank-conscious plutocrats in their fear of the disorderly masses. If Hobbes cannot be construed as radical democrat (as indeed he cannot—his motivations are markedly authoritarian), then at least he can be made to stand for the voiceless and excluded masses, and the intellectuals without serious scientific training—to whom science is an inaccessible mystery, seemingly beyond human control. Thus we are forced in our reading of the book to see it as a parable, whose fulsome celebration of Hobbes conveys the implication that "philosophers" who are not professional scientists (for which we must read "historians" and "sociologists") should have the authority to pronounce, or even to prescribe, on scientific questions. As we have observed before, this kind of stacking of the emotional deck has great persuasive force on the academic left, irrespective of the soundness of the argument that encodes it.

Cultural Constructivism as a Political Code

We close by once more denying any covert program on our part to exclude historians, sociologists, or even anthropologists from the study of science and technology as social phenomena, whether on the grand scale or on the level of interpersonal relations. We cheerfully allow that such work might well incorporate a passionately held leftist (or, for that matter, rights) point of view. The fact that much of the best of it comes from natural scientists such as Stephen Gould does not inevitably put similar effort by social scientists in the shade. Historians with a left-wing perspective, such as Marc Bloch and Gar Alpertowitz, to take but two vastly different examples, have certainly made their respectable mark. But a political point of view is one thing; the pursuit of philosophical phantoms in order to give leverage to doubtful ideological claims is quite another. We insist on making the distinction. The central ambition of the cultural constructivist program—to explain the deepest and most enduring insights of science as a corollary of social assumptions and ideological agenda—is futile and perverse. The chances are excellent, however, that one can account for the intellectual phenomenon of cultural constructivism itself in precisely such terms.

The doctrine, whether nakedly asserted without much attention to histor-
ic and scientific detail (as in Aronowitz), or built on a minute, but overly restrictive, examination of the social and historical record (as in Shapin and Schaffer), is clearly designed to flatter a certain political perspective, and to assert the sovereignty of a certain kind of political alertness over the domain of history and philosophy of science. Even apart from "ideology," in the narrow sense, it functions politically (as universities understand these things) to redress the grievances of the social scientists, and to elevate their knowledge claims to the level historically enjoyed by physicists and chemists. Thus, the insights of the constructivists are ripe to be turned against them. One must scrutinize their precepts and their practices for signs that their theories are "value-laden" to a considerable, perhaps an unacceptable, degree. The evidence is there, we submit, and reveals far more about the nature of the constructivist program than that program has ever revealed about the nature of science.

**CHAPTER FOUR**

The Realm of Idle Phrases: Postmodernism, Literary Theory, and Cultural Criticism

---

If you’re anxious for to shine in the high aesthetic line
As a man of culture rare,
You must get up all the germs of the transcendental terms
And plant them everywhere.
You must lie upon the daisies and discourse in novel phrases
Of your complicated state of mind,
The meaning doesn’t matter if it’s only idle chatter
Of a transcendental kind.

W. S. GILBERT, BUNTHORNE’S SONG FROM PATIENCE

---

The Ascent of Postmodernism

Future historians, composing a chronicle of the life of the mind in the United States during the period 1975–90, may well feel obliged to pay close attention to the role of academic humanists and social scientists. Assuming that they do, they will have to contend with the curious phenomenon of postmodernism, a stance that has infected the thinking of hosts of scholars in these areas. Postmodernism flourishes chiefly in departments of English, comparative literature, art history, and the like; but anyone familiar with contemporary American universities is well aware of how far it has spread into such unlikely areas as sociology, history, political science, anthropology, and philosophy.

To give a concise statement of postmodern doctrine would be an almost impossible task. It is too variegated and shifting to allow easy categorization, and too willfully intent on avoiding definitional precision. There is even a risk of misleading in calling it a body of ideas, for postmodernism is more a matter of attitude and emotional tonality than of rigorous axiomatics. Nonetheless, as critics of postmodernism in one of its currently most vigorous