The Politics of the Science Wars
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The Politics of the Science Wars

Introduction

Stanley Aronowitz

The main fronts of the Culture Wars—Western civilization versus multiculturalism, high versus low in music, literature, art, and modernism versus postmodernism—are now joined by what may be termed the Science Wars. The defenders of science have framed the debate in terms of reason versus unreason. While the language and vocabularies of science are different from those of the arts, the animus is the same: as for those safeguarding culture and science, the barbarians are at the gates. Those who would demystify science by showing it is subject to the same cultural and social influences as any other discourse, no less than critics who excoriate science for remaining silent when its discoveries are recruited for nefarious purposes, are charged with being prophets of (take your pick) unreason, mysticism, anti-Enlightenment, and nihilism, and with being promulgators of a higher superstition.

Science controversies are by no means as esoteric as one would think. Consider the bizarre result of an FBI investigation into the identity of the notorious Unabomber who, according to the *New York Times*, has, in the last seventeen years, "killed three people and injured 23 others" (Broad 1995). An agent appeared at the New Orleans meetings of the History of Science Association in October 1994 and subpoenaed its membership records because the FBI suspected the "bomber is immersed in the most radical interpretations of the history of science." According to the *Times* report, "professors have begun reconsidering old suspicions, acquaintances and tracts to help solve the crimes." Except for Langdon Winner of Rensselaer Polytechnic Institute, most of the association members and officials the reporter interviewed were donning their detective hats and Sherlock Holmes pipes or were prone to dismiss the bomber as "marginal" in professional science studies. Winner joked he was disappointed the FBI did not consult him on the case. "I feel left out. It's like being left off the guest list for a really good party" (Broad 1995).

Defenders of science such as Paul Gross and Norman Levitt (1994) write polemics that betray philosophical naïveté; others, like the New York Academy of Sciences (NYAS), are hosting conferences and symposia in which the critical theory of science is represented as a virus that must be

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purged from any affiliation with science and as something the public must be protected against. Members of the faith are circling the wagons against what they perceive to be a serious threat to the church of reason. The practitioners of the relatively new critical studies of science, which spans philosophy, the social sciences, and the humanities, are labeled mystical, anti-Enlightenment radicals who would turn the clock back to the Dark Ages. It does not seem to matter that many social and cultural critics of science are themselves trained in natural scientific disciplines. This knowledge seems to enrage the gatekeepers even more—after all, there is nothing more dangerous to a church than its apostates.

Behind scientificity stands the awesome and the once unassailed edifice of natural science. Together with the similarly God-like house of medicine, it presents itself as both the guarantor of the Enlightenment and the measure of reason. Its methods and results should not be subject to the same withering criticism as Darryl Strawberry, Bill Clinton, a layer cake, or the promises of a politician—except by scientists themselves, and then only within the precept of rigorously established rules. While everybody, including physicists and molecular biologists, is qualified to comment on politics and culture, nobody except qualified experts should comment on the natural sciences.

Of course, the efforts by the scientific community to stifle outside criticism have a long and painful history in the frequent incursions by totalitarian and democratic states to mobilize and otherwise control science by the use of purse strings, blandishments to fame and even fortune, political pressure or, worse, the consignment of recalcitrant scientists to external or internal exile. The sad history of Soviet and Nazi science and the more recent U.S. government decisions to steer public funding to scientific projects that might prove fruitful to helping U.S. corporations meet international economic competition are sufficient reasons for scientists to be wary of outside scrutiny. Like many opponents of government regulation, scientists have assured the public that they are perfectly capable of safeguarding its interests by means of self-policing.

Although scientists are subject to many influences—cultural as well as economic and political—they also constitute a series of communities, no less than industrial workers or the American Bar Association. These communities are formed in the laboratory but also by a hierarchy of research universities and independent research companies, leading nondisciplinary journals such as *Nature* and *Science*, many disciplinary journals, and associations such as the Federation of American Scientists (FAS) and, especially, the American Association for the Advancement of Science (AAAS). These informal and formal networks serve as an arena to circulate knowledge and provide basic information about where research money is going, as a forum for debates within the community, and as a lobby to get more government money and keep outside critics and funders at bay.

On the whole, the system works on its own terms. Government, conservative and liberal alike, is obliged by the specialized nature of scientific knowledge to rely on peer panels to determine the relative merits of funding proposals. Most science writers and journalists are the willing supplicants of a scientific establishment which passes down authoritative news and opinion about the successes of science, successfully manages its failures and, perhaps most important, marginalizes or silences alternative science both at the level of explanation and at the level of discovery. The key players and their institutions are the recognized gatekeepers of what counts as science and, more broadly, what counts as truth (Birrer 1993).

At the bottom of the brilliantly successful history of science since the seventeenth century is the dogma of method. The elements of this dogma are: (*a*) that the book of nature is written in the language of mathematics (for Galileo, who coined the aphorism, it was geometry), and (*b*) that the way to legitimate and reliable knowledge is through the experimental method, the basis of which is our ability to make both observational and falsifiable statements (Popper 1959).

The history of science is written by its winners and their publicists as the story of the smooth, continuous progress of reason. In turn, according to this story, science does best when it is free of interference from the state and also from private interests of any kind, including those of the public (Merton 1973). But there are always detours, and even reversals. Recent studies of the history of science reveal there are invariably zigs and zags. None of these investigations have impugned the claims of science to have made important and valuable discoveries that have enriched our understanding of so-called natural phenomena. It is difficult to deny that science has produced impressive results: rockets do reach the moon; penicillin can treat syphilis, the once life-threatening flu virus can be rendered relatively harmless; and solid state physics has produced an unparalleled information revolution. These technologies, based on theoretical science, have changed the character of everyday life.

My claim is not that science is uninfluential, only that its discoveries themselves and its influence are not unimpeachable. The import of the new social studies of science is to have shown that none of these discoveries amounts to a steady march toward Truth (Mulkey 1990; Barnes 1974). Nor are they free of economic, social, and cultural preconditions or consequences. Although science has its own history and the more sophisticated students of the social relations of science reject the idea that there is a one-to-one correspondence between its social and political preconditions and the content of discoveries, there can be little doubt that contemporary science, no less than its predecessors, is conditioned by these circumstances (Bloor 1976).

For example, during and after World War II, most basic as well as applied science was funded through the Department of Defense (DOD).While DOD did not apply a mechanical criterion of utility in making its awards, many, if not all, funding proposals justified their requests on practical grounds. The choice of investigative objects and their promised results is ineluctably designed to persuade the funder that the payoff is worth the money. In recent years, as competition for public and private money has become even more brutal, most scientists and their organizations have faced the grim prospect that they might be deprived of the opportunity to perform research by tailoring their science even more specifically to practical ends. A recent deal between MIT and a group of private pharmaceutical and bioengineering companies, following a trend in industry-university relations since the 1970s, acknowledges that knowledge is private property and, in return for corporate research grants, assigns a large proportion of patents over to the companies.

Writers such as Dorothy Nelkin, David Dickson, and Martin Kenney, who have documented the close relationship between academically based scientific research and private corporations, are simply subjecting science to the same ruthless criticism that corresponds to the scientific ideal of self-critical inquiry. The critical theory of science does not refute the results of scientific discoveries since, say, the Copernican revolution or since Galileo's development of the telescope. What it does challenge is the notion that science and its discoveries are exempt from ideology critique, deconstruction, or historical investigation that might be trained on any other discourse: literature and art, politics, social scientific theory, and so forth (Dickson 1984; Nelkin 1979; Kenney 1986).

It is not a question of determining the *truth* value of scientific knowledge, if by that notion we designate the correspondence of a proposition to a reality independent of the knower. In the main, the critical theory of science, in conformity to the relationalism of contemporary physical science, allows that, in every stage of its development, the various natural sciences have generated a *regime* of truth consistent with the *frame of reference* within which their theories are generated (Foucault 1980a). Historians such as Shapin and Schaffer, Joseph Agassi, and Georgio De Santillana have produced case studies of leading events in the history of science, demonstrating the salience of what traditional scholars have learned to call "external" factors as precisely the frame of reference within which key elements of "truth claims" are generated (Shapin and Schaffer 1985; Agassi 1982; De Santillana 1955).

In contrast, from the perspective of orthodox positivism, for which "seeing is believing," the historicity of science must be confined to the idea that scientific theories are, at best, successive approximations of a reality existing outside the conditions of investigation. Most theoretical physicists, for example, sincerely believe that however partial our collective knowledge may be of the nature of the microphenomena that constitute the building blocks of physical reality, one day scientists shall find the necessary correlation between wave and particle; the unified field theory of matter and energy will transcend Heisenberg's uncertainty principle.

Biologists tend to regard Darwin and Mendel as worthy predecessors to a history of continuous progress in our collective knowledge of life and its forms. Clearly, for James Watson and Francis Crick, structure or form overrides function, evolutionary process, or the interaction between an organism and its environment as determinations of its essential characteristics. For these biologists, the fundamental account of the nature of life proposed by the new technoscience of molecular biology—that the DNA molecule provides the framework for life and its characteristics—is no longer a hypothesis; it has become *the* fact from which all further experimental and theoretical work should proceed (Watson 1965; Crick 1981).

Ecology, which does not dispute the importance of the DNA molecule, disputes the significance awarded to it by molecular biology. Biologists such as Richard Lewontin and Richard Levins have adopted an ecological perspective and have proposed an alternative paradigm of life (Levins and Lewontin 1985). The interaction of an organism with its own genetic structure is only one of the crucial determinants of its course of development and transformation. Its two environments—its own species and the ecosystem of which it is a part—are intrinsic to both its survival, growth, and transformation. Thus, contrary to classical genetics, both the spatiality and temporality of life forms is essentially indeterminate from the perspective of the genetic code. But since molecular biology is both discursively hegemonic—its account has won broad acceptance in the scientific community—and gets all the research grants from government and pharmaceutical corporations because it is crucially a technoscience, the ecologists are engaged in a Sisyphean struggle for recognition in theoretical terms.

In what follows, I address three distinctly different but closely linked debates: What are the uses of scientific knowledge? To what does scientific knowledge refer? And, perhaps the most complex of all questionings, what are the economic, political, and cultural influences on the content and the results of scientific discovery?

1

The very public counterattack by the scientific conservatives and their publicists is by no means unexpected. What needs explanation is why the scientific establishment, which for years ignored or curtly dismissed critics such as Nietzsche and members of the Frankfurt School as antediluvian cranks, has chosen this moment to recognize that the challenge is

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worthy of reply. The immediate reason might be that, whereas earlier criticism was consigned to the wilderness of "provocative" but ultimately unrespectable thought and was sequestered within fairly narrow circles, today critical investigations of science and technology have won some academic legitimacy and, perhaps more to the point, have begun to influence some scientists and the public as well.

Undoubtedly, the growing skepticism about the unqualified benefits of science and technology was fueled by the wholesale corporate and government despoilment of the values of scientific disinterestedness and what Robert Merton once termed "communism"—the ethic of knowledge sharing and the concomitant renunciation of private property in knowledge. The claim of science to social neutrality is subject to increasing incredulity since the veritable subsumption, after 1938, of much of U.S. natural science (physics, chemistry, and biology) under the military; since the despoilment of the environment by science-based technologies such as plastics and genetic engineering; in the shameless use of the social sciences in the service of pacification programs in Vietnam; in the dismantling of the welfare state in the name of "public policy;" in controlling workers through industrial psychology; and since the feminist charge that not only have women been occluded when not entirely excluded by science and technology institutions, but that scientific knowledge is itself "gendered" (Harding 1986; Keller 1985).

The genealogy of these critiques may be traced to post-Enlightenment philosophies such as Nietzsche's and Spengler's but also to the various critical theories whose roots are in the deep pessimism of the interwar period engendered by the slaughter of more than twenty million soldiers and civilians during World War I and the subsequent failure of the great powers to make a durable peace on the basis of the democratic and egalitarian precepts of the American and French revolutions to which they were ostensibly committed (Forman 1971). The recognition, especially by postwar intellectuals, that Western scientific and technological culture had wrought contradictory results—weapons of unequaled mass destruction and the material conditions for liberation from work—raised sharply the question whether science was a force for liberation or human annihilation. Of course, the atomic destruction of Hiroshima and Nagasaki did nothing to restore faith in the ineluctable link between science and progress. In the aftermath of the first and last wartime uses of nuclear weapons in 1945, many scientists, who in the wake of Hitler's rise to power participated in their development and urged President Roosevelt to fund the building of atomic weapons, organized an international movement to persuade governments and public opinion that atomic war was unacceptable. But, the genie had been let out of the proverbial bottle. Nuclear weapons have become a major instrument of power; if no government will voluntarily surrender power, no government will lay down its nuclear weapons.

In the 1930s, one of British Marxism's most important institutions, the Social Relations of Science group, included figures prominent in British science, such as the biologist J. B. S. Haldane, the mathematician Lancelot Hogben, and especially the Oxford physicist John Desmond Bernal. Bernal's *Social Function of Science* (1939) and *Science and History* (1957) stand together with Joseph Needham's *Science and Civilization in China* (7 vols., 1954–1985) as perhaps the monuments to the fundamental thesis that scientific knowledge depends, inescapably, upon the social context within which it is produced. Employing a Marxist theory of historical periodicity, they argued that economic, political, and cultural contexts within which knowledge is acquired could not be viewed within the framework of conventional "external" and "internal" distinctions. For them, the development of science and technology crucially depended on the ideology and practical demands of a given *mode of production* and, insofar as these pursuits were "work," were aspects of it.

The New Left, which played an enormously significant role in the early movement against U.S. intervention in the southeast Asia wars, was not merely an activist core committed to such specific goals as peace, participatory democracy, and university reform. It was also an intellectual movement which, in many respects, carried on the tradition of the Social Relations of Science group without collectively being aware of it.

Of course, there are major differences between the Old Left's view of science and technology and that of the generation that came to politics in the 1960s. Bernal, Haldane, and other leaders of the Social Relations of Science were proud legatees of the Enlightenment's celebration of scientific knowledge as a means of human emancipation. For them, scientifically wrought forces of production would, under socialism, be set free from the shackles of capitalism. In short, despite their effort to link scientific development to its historical conditions, they were definitively not critics of knowledge; they were critics of the economic system that thwarted its full development.

In contrast, the New Left was skeptical of science because of its visible *effects*: Rachel Carson's *Silent Spring* became a central text, in part, because it showed, with remarkable literary skill, how scientifically based technologies threatened the survival of life forms of all sorts; as William Gibson has shown, the Vietnam War was preeminently a technological war, a lesson not lost on antiwar activists (Gibson 1986) and, of course, the ever present threat of nuclear conflagration loomed over the lives of the generations born after 1940 for whom the A-bomb was a defining feature of social and psychological worlds.

What Marxism endows to the critical theory of science and technology is its insistence upon a *social* theory of science. After Bernal, Needham, and the Soviet historian of science B. Hessen's account of the "Social and Economic Roots of Newton's Principia" (1931), which inspired Robert Merton's early work in the sociology of science in the 1930s, it is difficult to imagine a purely internal account of scientific discovery (Hessen 1931). The intellectual legacy of the social relations of science approach has encompassed a wide range of knowledge domains: literature, history, the social sciences, and the natural sciences. In the sciences and technology, organizations that exposed the complicity of scientific institutions with the military and the corporations sprung up in the late 1960s under the rubric of social responsibility. To our own time, groups of physicists, biologists, computer scientists, technicians, and physicians have, in their own disciplines, continued to challenge their colleagues to consider the ethical implications of their work and its uses. They have ferociously criticized the doctrine of scientific neutrality as a ruse and a serviceable alibi.

If there is a common theme to these critiques, it is that the dominant ideology of American science is influenced by what C. Wright Mills once termed "the American celebration"—the idea that in this, the best of all possible worlds, science, literature, and other forms of culture flourish because this is truly a pluralistic society in which European-type struggles linked to class, race, and gender have yielded to compromise and consensus, the vehicles through which social progress may be forged.

I want to insist that the convention of treating natural and human sciences according to a different standard be dropped from the perspective of prevailing science studies. Since *scientism* or positivism dominates both social and natural sciences, I want to treat the controversies within each domain as aspects of the same general problematic: How are the objects of knowledge constructed? What is the role of culturally conditioned "world-views" in their selection? What is the role of social relations in determining what and how objects of knowledge are investigated?

If my thesis is correct that scientificity permeates *all* knowledge domains, including the humanities, the distinctions between the natural and human sciences are not as significant as their similarities. The rigorous separation between them was the product of nineteenth-century physicalism that *reduced* social life to its biological and physical aspects. From the important theoretical posit that humans and their interactions are part of natural history, and therefore that our biological being could not be abstracted from social theory, writers such as Herbert Spencer and Darwin's cousin Francis Galton concluded that social life was merely an efflux of biological behavior, a theme revived in the late twentieth century by B. F. Skinner, Edward O. Wilson, Robin Fox, and Konrad Lorentz, to name only the most notorious among latter-day social Darwinians.

Rejecting this doctrine, German neo-Kantians such as Wilhelm Dilthey insisted that the natural and human sciences be separated because they obey different laws. While the predictability of physical phenomena on the basis of precise measurement may be achieved on the basis of strict causality, the indeterminacy of social life produced by interaction and consciousness demanded a different although no less rigorous scientific algorithm. Evidence in the historical sciences was not commensurable with that in the natural sciences, since observation and experiment were not possible, except in animal psychology. The method of the natural sciences was, inevitably, historical and hermeneutic. One reads the texts of the social world rather than relying on the laboratory. Moreover, mathematics has little or no role in the human sciences, because the human sciences necessarily abstract quantity from quality and address themselves to extension.

My argument is not only grounded in the pervasive positivism of all academic disciplines but depends on one of the more important interpretations of quantum theory suggested by physicists and philosophers as diverse as Neils Bohr, David Bohm, and Roy Bhaskar. In opposition to interpretations of the Heisenberg uncertainty principle, according to which the problem of measurement of the electron is chiefly epistemological, that is, following Kant's famous doctrine that knowledge of the real is inevitably mediated by the categories of mind, Bohm, for example, offers an *ontological* interpretation of quantum theory. According to Heisenberg, the conditions of measurement prevent a precise determination of position and velocity at the same time. Hence, even if the particle exists objectively independent of the process of knowledge, we cannot derive *meaning* without taking account of the measuring instrument. There is no warrant to predict simultaneously with *certainty* the velocity and the position of a particle, since calculation is inevitably probabilistic.

Bohm's solution to the posit of a split between knowledge and its object, which dominated classical physics and remains among the most influential interpretations of quantum mechanics, is to argue for

> the *undivided wholeness* of the measuring instrument and the observed object. . . . Because of this it is no longer appropriate, in measurements to a quantum level of accuracy, to say that we are simply "measuring" an intrinsic property of the observed system. Rather what actually happens is that the process of interaction reveals a property involving the whole context in an inseparable way. Indeed it may be said that the measuring apparatus and that which is observed *participate irreducibly* in each other, so that the ordinary classical and common sense idea of measurement is no longer relevant. (Bohm and Hiley 1993, 6)

For Bohm and others who attempt to overcome the dualism of the observer and the observed, the field is not constituted by objects whose antinomy is the subject-observer. While carefully framing his statement to refer to *measurement* rather than an independent subject, it is clear that the knower and the means by which the known become intelligible are intrinsic to objects or processes. For this reason, Bohm argues that probability is not a "defect" of science or its instruments but a property of the universe, a position that corresponds to recent work on complexity in which order and chaos are understood as inextricably linked.

This interpretation of quantum theory has profound implications for the human sciences. In the first place, the attempt to model social sciences on the methods of the old natural sciences is entirely misplaced. Second, the neo-Kantian presuppositions of social constructionism, which, under the sign of anti-essentialism, have refused to acknowledge that biological and physical being are aspects of social being, must similarly be refused. Rather, the continuity as well as the difference between natural and social history, which are the foundations of the biological theory of *integrative levels*, constitute an alternative to that of both physical reductionism and social constructionism. That is, life, in both its evolutionary and structural aspects, is organized within each organism by its physical, biological, and social levels, and the higher levels integrate but also *negate* the so-called lower levels. Here, negation is used in the Hegelian sense: that, in human societies, for example, the social and cultural do not *cancel* physical and biological aspects. After all, through our physical, chemical, and biological organization we are in an unavoidable relationship with the universe which, as we know, is a condition of our existence.

Roy Bhaskar's orientation is, in its essentials, congruent with Bohm's insofar as he agrees on an ontological solution to the problem of knowledge raised by quantum theory. Bhaskar's polemic with positivism and empiricism has been among the most vigorous in contemporary philosophy of science. He has argued against the Kantian constructionist view that science refers to its own conditions of knowledge production rather than to an independent external reality. However, consistent with his philosophical Leninism, he insists on epistemological arguments for the objectivity of the material world. That is, against the idealism's "irrealism," which Bhaskar ascribes to the Kantian idea that science refers exclusively to the conditions of *knowledge,* he retains objective reality as a(n) (indeterminate) referent independent of the processes of knowing—a distinction which, like Louis Althusser and Gaston Bachelard, remains sympathetic to this Cartesian formulation upon which all epistemological accounts of science ultimately rest (Bhaskar 1975, 1986, 1993).

Bhaskar's "transcendental realism" explains the source of knowledge as the "generative mechanisms" of the objects, but he is not in basic agreement with conceptions such as Bohm's that place processes of investigation within the "real." This leaves him with a self-described realist metaphysics. And, although he invokes historical as well as natural determination to explain the characteristics of scientific knowledge, there is no content to this claim; Bhaskar does not refer to the Marxism of the Social Relations of Science group, to the wealth of historical accounts of the close link between Enlightenment scientific knowledge and power, or to the relation of science and domination suggested by the Critical Theory of the Frankfurt School. In short, Bhaskar is firmly ahistorical, entrenched in the conventions of the Popperian philosophy of science. His own discourse abounds in scientistic formal logic. Even when he writes somewhat sympathetically of Hegel's dialectics, the influence of English positivism and empiricism remains heavily on the page.

2

Critical investigations into the history, philosophy, sociology, and anthropology of science did not, at first, have the powerful effect of parallel efforts in literature and the social sciences for fairly clear reasons: the natural scientific community was far more unified, it was amply funded for what it was doing, and it claimed not merely a nationalist legitimation but the mantle of universal validity. Since the seventeenth century, when Robert Boyle vanquished his opponents by building a scientific community that counterposed the evidence of "seeing" to that of speculation as the criterion of truth, science has seemed to be the common sense of legitimate knowledge. Moreover, especially in Britain, New Left historians just did not see scientists as important social and cultural agents. You don't, for example, find any discussion of science in essays like Perry Anderson's "Consequences of the National Culture." Nor did Raymond Williams, perhaps the most important figure who linked the New and Old Lefts, write about science in relation to culture. However, Williams did study the cultural and industrial impact of television and communications, but from the standpoint of their reception.

Scientists and their organizations have been on the defensive since the antiwar movement exposed the complicity of perhaps the entire science establishment with the Department of Defense (DOD) and since others exposed the degree to which scientists and their institutions had been subsumed by large corporations concerned only with practical applications of scientific theories to the bottom line of profit. Why did scientists accept funding from the military and permit corporations to own the patents to their discoveries? With private as well as state funding, why have they engaged in massive experimentation with and production of artificial organisms in the face of evidence generated by other scientists that genetic engineering—a form of industrial production—in densely populated areas could endanger public safety and health? Why have many scientists remained silent in the wake of the decline of ecosystems essential for life? Why did the majority of scientists fail to follow the post-Hiroshima advice of some of their most eminent leaders—Albert Ein-

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stein, Leo Szilard, and Phillip Morrison among others—not to engage in research that would lead to the further development of nuclear weapons? (Lanouette 1992, 259–80).

Indeed, many of these questions have a long history. Under the influence of publicity given to major environmental controversies—most recently, the widely reported cases of many cancer deaths in the western region of the country resulting from radiation contamination during postwar nuclear tests—issues once confined to national security circles are today taken more seriously by broad layers of nonexpert opinion. Although I would not want to deny that an element of panic attends the emergent survivalist ethic that has accompanied frequent reports of imminent or long-term environmental disasters, there is no doubt that practices that went unnoticed and unopposed as late as thirty years ago—such as using incinerators to dispose of garbage; dumping toxic waste, including nuclear materials, into bodies of water and landfill sites; pollution connected with ordinary industrial production techniques; and environmental threats, such as global warming and the holes in the ozone layer—are today under microscrutiny.

The favorite response to these questions by scientists and their academic and journalistic acolytes is that the public has been entirely misled. Most scientists maintain the public position that science is not political but is in the main a disinterested inquiry into the nature of things. Universities and other research institutions claim they remain committed to scientific *discovery* and pay little or no attention to the commercial, military, or other uses to which their work is put. When some research scientists contract patents for their discoveries to corporations in return for funding for pure as well as applied research, the intention remains to advance the single cause of science: knowledge. Scientists portray themselves as vehicles facilitating the advancement of learning and, by application, the progress of humankind. If corporations are willing to provide resources to advance knowledge, scientists have little choice but to accept these resources if they wish to continue their investigations in the face of severe cutbacks of government funding.

No doubt many scientists were, and are, ambivalent about the role of the military in postwar scientific research. Scientists point out that, since the military-industrial complex was (and is) the main source of funds for basic research, many recent breakthroughs in physics and biology that have made life better would have been impossible without this money. For example, they say, even when establishment science agreed to cooperate with the looney tunes Star Wars program, most of the money advanced pure scientific knowledge and had little to do with the actual antiweapons project. In short, scientists are not in power; they cannot control who and what is made of their discoveries. Yet, somehow scientists believe in the integrity of their work, of the scientific community which, despite violations of its autonomy, remains the ultimate arbiter of the worth of a theory or empirical finding. This contradiction plagues science: while it is not in power it controls the conditions of the production of knowledge. We can ascribe this paradox either to naïveté or to bad faith. If the latter, it may help explain the profound animosity of some scientists to their critics.

Of course, some scientists are enthusiastic supporters of U.S. foreign policy; some sincerely believe in the social value of bioengineering and even minimize its dangers just as others have vehemently denied that the industrial uses of scientific research may be injurious to the health of workers and their communities. But the bulk of scientists remain self-styled neutrals, depicting themselves as civil servants of knowledge.

Writers like Dickson (1984) and Kenney (1986), who have profusely documented the extent of the subordination of the scientific community, including university-based research, to instrumental ends dictated by the exigencies of policy or profit, have focused on the issues surrounding the *uses* of science and scientifically based technologies. In the main, their work belongs to the genre of literature that addresses the question of whether the selection of the scientific object is free of considerations having to do with its applications.

3

While the history of science can be told through "internal" accounts of scientific discovery—this or that experiment or this or that mathematical equation providing the basis for this or that "breakthrough"—putting a finger on economic, political, or cultural *influences* on, let alone *determinations* of, the content of knowledge is a far trickier business. Until recently, many historians and philosophers of science, negligent of the work of Bernal and Needham, denied any significant relationship between the historical and social context of scientific discovery to the key elements of what constitutes science itself. Karl Popper, whose *Logic of Scientific Discovery* (1935) remains among the most influential works of the philosophy of science in this century, and Robert Merton, perhaps the leading figure in the development of the sociology of science, acknowledge the importance of the political and cultural environment for encouraging or repressing free inquiry. Merton argues that democratic regimes provide the best environment for science because they are committed to pluralism and freedom. He allows that the constraints of the scientific community, the state, and economic interests might retard the forward progress of science, at least temporarily. Conversely, economic changes might place new demands on science. For example, Merton's first major study in the social relations of science supported Hessen's contention that seventeenth-century science, of which Newton's magisterial discoveries were the crowning achievement, responded to three crucial historical developments: the demands for a new science of navigation spurred by the global adventures of the British navy and merchant fleets; mining to provide raw materials for the new manufacturing; and the emergence of new mechanical weapons and means of transportation of war. But, it is one thing to argue that war, industry, and commerce, which accompanied the bourgeois revolution in England, provided an underlying cultural framework for the development of science and another to claim that this framework was, itself, crucial for determining its substance. While necessity might be the "mother of invention" insofar as it provides the motive force, is it not science itself, its spirit, algorithms, theoretical debates, and the interchange among members of the scientific community that constitute scientific knowledge?

Popper is prepared to stipulate that scientists are frequently motivated by ideological considerations: they want to improve the human condition by choosing biological research in order to find the basis for treating diseases which shorten life, and their nationalist feelings might inspire them to participate in their country's war effort by offering their talents to weapons development. Certainly, in the face of the rise of fascism, many physicists were recruited to help develop the atomic bomb, often against their own pacifist beliefs. But, he argues in the last analysis, in order to be taken seriously by the scientific community, every scientific proposition must be subject to the criterion of falsifiability, since (he acknowledges) verification suffers many pitfalls.

The "new" social studies, philosophy, and critiques of science have focused as much on scientificity as on politics, as much on processes of scientific work as on the uses of professional science, especially by industry and the state. But social research on science has been extended to a close examination of what goes on in the laboratory, the iconic site of the production of natural scientific knowledge. Bruno Latour argues, for instance, that the laboratory is, in addition to a major site for knowledge production, among the new sites of social power and that culture reflects what happens there as much as the reverse. Latour, Andrew Pickering, Sharon Traweek, and others have accumulated a fair amount of ethnographic documentation about what happens in physics and biology laboratories (Latour 1985; Pickering 1984; Traweek 1988). They have discerned that the abstracted picture of the process of scientific discovery according to which agreement is reached on the basis of rigorous algorithms of proof is, in fact, quite messy.

Following the precept that laboratory life is directly relevant to the results as well as to the process of discovery, Latour and Woolgar (1979) identified three important types of interaction: conversations between scientists, their interpretations of written reports, and the machinery that mediates their perception of what they have actually "seen." All are relevant to what counts as legitimate knowledge or truth, to which the informal influence exercised by leaders of the research project must be added. Indeed, as Thomas Kuhn and others have shown, the judgments that notables in the scientific community pass on results of research are as important as formal procedures for determining validity. The criteria for rejecting scientific theories or discoveries depend as much on relations of power and influence within the scientific community as they do on procedures (Kuhn 1962, 1969).

Rejected theories might offer equally plausible explanations to those that are ultimately accepted. But, lacking the political support or the context of a large university or of independent scientific organizations, the work of many falls by the wayside. Rejected or marginal sciences such as parapsychology, the study of clairvoyance, and, in the wake of the triumph of molecular biology, ecological and evolutionary biology, are just a few examples of the evidence that the scientific "community" as a site of power determines what counts as legitimate intellectual knowledge, even when the results of the marginalized sciences are obtained by traditional methods.

Working in the late 1950s, Kuhn suggested an alternative theory of what he called scientific revolutions to contest the commonly held belief in the idea of scientific progress. Following the arguments of American philosopher Charles Sanders Peirce, Kuhn argued that paradigm shifts occur when the leaders in the scientific community accept a new set of explanations for anomalies that appear in the course of doing what science always does, puzzle solving. When enough puzzles, such as the Michaelson–Morley experiment and Brownian motion, cannot be solved inside the prevailing paradigm, a new paradigm is proposed. But its adoption by the scientific community depends, ultimately, on whether the major figures accept it. The new paradigm does not necessarily stand on the shoulders of the old one but eventually displaces it by its capacity to explain data that were in conflict with the accepted view, but only on the condition that leaders in the scientific community accept it. Kuhn remains agnostic with respect to the truth value of the new paradigm but, tacitly, accepts a highly relativistic version proposed by Peirce: the truth is what those who are qualified say it is.

Philosopher Paul Feyerabend (1976) took skepticism one step further. Contrary to the accepted view, he argued that a dominant paradigm of scientific truth may not owe its success to its superiority over previous theories when measured by traditional Baconian and Popperian criteria but to a variety of factors that are external to experimental or mathematical calculation. Perhaps his most controversial claim is that the "Copernican revolution" and its crowning achievements, Galileo's "proof" that the earth was in constant motion around the sun, and Newtonian mechanics were not "superior" to Ptolemaic science. Using the criteria (*a*) that knowledge be obtained through observation and experiment and (*b*) that theory explain a broad range of phenomena, he showed that Ptolemaic cosmology was not radically different from that of Copernicus, and that far from being purely speculative, Aristotelian physics obeyed much of Karl Popper's rule that scientific theory be refutable by an inductive algorithm.

The sum of these investigations is to bring science and scientificity down to earth, to show that it is no more, but certainly no less, than any other discourse. It is one story among many stories that has given the world considerable benefits including pleasure, but also considerable pain. Science and its methods underlie medical knowledge, which, true to its analytic procedures, has wreaked as much havoc as health on the human body; and it is also the knowledge base of the war machine. Science has worked its precepts deep into our everyday life. Science as culture is as ubiquitous as is science as power.

4

Heisenberg's inclusion of the observer in the field of observation corresponded, as Paul Forman (1971) has argued, to developments in philosophy and culture during the Weimar period of German history. Forman's thesis is that the development of quantum mechanics was crucially linked to the loss of confidence of intellectuals, among them scientists who, during World War I, had given their hearts and their minds to the imperial aims of the German government. The transformation of the zeitgeist was evident in art, politics, and culture. Forman shows that Oswald Spengler's best-selling *Decline of the West* was not only emblematic of the pessimism that afflicted culture but was deeply influential on its development in the 1920s. In particular, the first (and most controversial) chapter of that book argued, on the basis of available anthropological evidence, that the conception of numbers characteristic of Western culture was entirely conventional and, contrary to the common sense belief that our system of numbers was universal, Spengler insisted it be viewed a cultural creation.

The originality of Forman's work is placing the scientific community in the context of these cultural/historical shifts. Rather than view them as a self-contained knowledge community, Forman insists that scientists, especially in turn-of-the-century Germany and Austria, were a major component of their respective intellectual classes. The most difficult aspect of Forman's account is his attempt to find the mediations between culture and scientific knowledge. Amassing a large quantity of historical documentation, he shows that scientists shared in the pessimism that afflicted the rest of the intellectual elite and links this pervasive mood among them to the shift from the old quantum mechanics, which retained large elements of classical physics, to a theory that, in its own reflection, was pushing Einstein's own relativity theory to its limit.

In their study of the controversy between Thomas Hobbes and Robert Boyle in the seventeenth century, Shapin and Schaffer (1985) argue that the proposition according to which reliable knowledge required as its precondition the evidence of the senses and a procedure whereby such evidence may be tested by means of experiment was severely contested. They show that the modern scientific method is entirely conventional and forged in the context of heated intellectual debates, and that Boyle's triumph over Hobbes and other alchemists and hermeticists was due not only to his ability to constitute a scientific community that supported his thesis that his opponents' claims were suffused with speculation masked as scientific knowledge but also to his construction of a machine, the air-pump, which abetted Boyle's claim to have adduced objective results from experiments. Boyle's triumph was also due to his great capacity to use the tools of popular writing and speech to extend his influence. Despite the difficulties with the experiments conducted by Boyle and Robert Hooke, they were able to prevail because they captured intellectual hegemony, not so much on the basis of their experimental results which proved to be significantly flawed but perhaps more by their social and literary skill. Shapin and Schaffer (1985, 77) argue that

> the role of Boyle's literary technology was to create an experimental community to bound the discourse internally and externally, and to provide forms and conventions of social relations within it. The literary technology of virtual witnessing extended the public space of the laboratory in offering a valid witnessing experience to all readers of the text. The boundaries stipulated by Boyle's linguistic practices acted to keep that community from fragmenting and to protect items of knowledge to which one might expect universal assent from items of knowledge that generated divisiveness. Similarly, his stipulations concerning proper manners in dispute worked to guarantee that social solidarity that produced assent in matters of fact and to rule out of order those imputations that would undermine the moral integrity of the experimental form of life.

"Seeing is believing," the common sense of everyday life, became the common sense of science as well. The burden of the study of the Hobbes-Boyle controversy is to historicize the requirement that scientific propositions contain observational statements. As De Santillana (1955) showed, Galileo's rise to prominence at the turn of the seventeenth century owed its success to his literary and social abilities much more than to what turned out to be fairly dubious experimental work. That is, he was able to disseminate his *ideas* to other practitioners who supported his struggle against the academic establishment, which derided his argument that the earth was not stationary, and he achieved his greatest notoriety only after he resigned his academic position in Padua and placed himself under the patronage of the powerful Florentine Crown.

These examples from the history of modern science may not satisfy those for whom science is the single inquiry whose methodological rigor makes its actual results, as distinct from its motive force, immune from social and cultural influences. As Sir Karl Popper retorted to similar arguments advanced by Theodor Adorno, perhaps the quintessential critic of positivism, the question is not whether culture or history influences science but whether such influences can be filtered out of scientific theories and experimental results by scientific method (Adorno et al. 1971). Forman is acutely aware of this challenge and shows that one may render a plausible account of the uncertainty principle that Bohr insisted inhered in both inquiry and the processes it studies by understanding the historically specific frame of reference of Weimar culture.

Forman's work takes the social relations of science hypothesis to the level of culture or Weltanschauung (worldview). His meta-argument is that profound changes in scientific knowledge are produced not only in the laboratory and by mathematical calculation, or, as Latour and Shapin claim, by social, literary, and material technologies, but also by the zeitgeist. While Popper and the positivists scoffed at such assertions, since the posit of cultural despair is not subject to refutation, the great mathematician David Hilbert, the leading wave mechanics theorist Ernst Schroedinger, and Einstein were all outraged and ultimately renounced the Bohr-Heisenberg thesis of a probabilistic universe, which became one strong interpretation of the uncertainty principle. But, there is little doubt that Bohr and Heisenberg finally won the day. Was it because the theory was unrefutable? Or is it that the "proof" of such a theory ultimately resides in whether influential scientists accept it? What are the multiple determinants of acceptance?

One could speculate in a similar fashion about the triumph of molecular biology. Manuel de Landa (1991) has argued that the history of scientific and technological advances is intimately connected to militarism. No war gives greater credence to this thesis than World War II. The fundamental meta-advance was the close integration of science and technology. From von Neumann's explorations of mathematics, which were linked to the work on radar and atomic weapons, to the emergence of information sciences by Shannon and Weaver emanating from Turing's work and the development of plastics, by the end of the war, technoscience was already an adolescent. But, molecular biology and solid state physics bring technoscience into full maturity because the theory itself is a technology. Like Ernest Lawrence's development of "machine physics" in the work on the atomic bomb, Watson and Crick's discovery of the double helix structure of the DNA molecule was intrinsically linked to the production of new forms.

Perhaps the most sweeping critique is advanced by feminists who claim that science tends to be a masculinist discourse. In the words of Elizabeth Fee, feminist critiques "[expose] how the foundations of our [scientific] knowledge have been built on the assumptions of male domination" (quoted in Bleier 1986). It is not only Evelyn Fox Keller's historical account of the marginalization and exclusion of women from the natural scientific disciplines or the more or less systematic male denial of the value of the work of those who breached the barriers. Philosopher Sandra Harding, biologist Donna Haraway, and others claim that aspects of scientific knowledge are gendered. Science, in Haraway's phrase, is "politics by other means," a proposition that strikes to the heart of the guiding ethic of science: its cultural neutrality and disinterestedness (Haraway 1989).

The question is whether science can evade what every other discourse must face: its dependence on, as well as struggle for autonomy from, culture. Feminists, ecologists, AIDS activists, and those who, from a scientific standpoint, have examined the use of imputed racialized genetics to explain differences in school performance are acutely aware that much of established science remains in a state of deep denial regarding these issues. The debates between established science and its activist critics (including some scientists) are increasingly well-known. The theory of global warming is still hotly contested in scientific circles; the relation of the HIV virus to AIDS and, perhaps more urgently, whether research aimed at finding a "magic bullet" to stop the disease is more effective than broadly based epidemiological investigations remains uncertain; and even though most biologists scoff at the evidence for racialized genetics, many still accept the underlying idea that some genetically derived intelligence exists, independent of the social and cultural conditions that affect school performance.

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At the end of the day, the many questions of scientificity and science and its influences cannot be settled by means of a fail-safe method of inquiry. Einstein's relativity theory was subjected to official skepticism twenty years after the publication of his Special Theory article in 1905; and equally passionate partisans of wave and matrix explanations for the behavior of electrons were unable to reach agreement for decades. Similarly, the dogmatists of internalism—Gross and Levitt and so on—who posit scientific method as ruthlessly self-critical and science as ultimately self-contained, are likely to remain unconvinced, even though some of the more prominent writers in science studies have trained in physics (Pickering, Kuhn, Keller, Feyerabend) or biology (Haraway). But the vituperation that accompanies the dogmatists' defense, not only of science but of empiricism and positivism, is rather quaint. Unlike Sir Karl, they are not usually sufficiently philosophically sophisticated to grasp, let alone refute, their opponents' claims.

Underlying many of these disputes is the pervasive fear, by many scientists as well as by the economic and political elites, that the experience of ACT-UP will spread. ACT-UP, an organization of gay and straight activists concerned with the spread of the AIDS epidemic, began by demanding more funds for AIDS research and now intervenes in scientific disputes as to the best treatment for the disease. By force of circumstance as much as by conscious knowledge, it was obliged to question many of the precepts of its own membership, namely, that the doctor knows best. Gradually, members became sophisticated in many areas of the history and philosophy of science. Imagine a polity capable of challenging the uses and truth claims of scientific and technological research. Imagine a new scientific *citizenship* in which democratic forms of decision making were shared between the scientific community and the public. With ghosts of Nazi and Soviet calumnies in their imagination, scientists tend to cringe at the prospect. But they have to face their own conviction that, as far as their work goes, democracy is only appropriate for the few.

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