Taking Arms

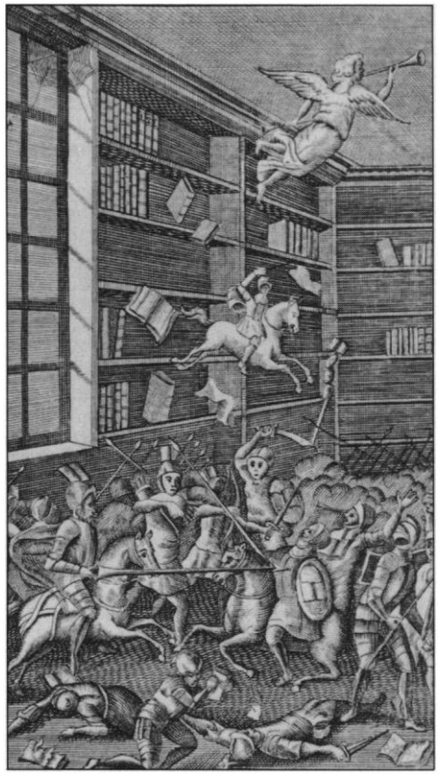
Higher Superstition. The Academic Left and Its Quarrels with Science. PAUL R. GROSS and NORMAN LEVITT. Johns Hopkins University Press, Baltimore, MD, 1994. xii, 314 pp. $25.95.

This book is an angry polemic by a scientist and a mathematician against what they call the anti-science left in our universities. The enemy includes practitioners of science studies, Marxists, feminists, "constructivists," postmodernists, multiculturalists, and environmentalists, housed mostly in departments of philosophy, history, sociology, anthropology, and literature. The nub of the book's argument is that the writings of these scholars on the history, philosophy, and sociology of science are filled with ignorant resentment against the accomplishments of the exact sciences and that they do their incompetent best to undermine the credibility of those accomplishments. Gross and Levitt disavow any connection between their argument and the war on "political correctness." They present themselves, rather, as cultivated men of science, custodians of the Enlightenment, obliged to defend the citadels of knowledge against know-nothing attacks. So they mount their white horses and ride forth to slay the dragons of the anti-science left.

Logic and evidence are their weapons of choice, but polemics are usually fun to write, and the authors do not resist drawing other, less elevated, weapons from the polemical armory: sarcasm, hyperbole, righteous indignation, *ad hominem* devices, and grave warnings about the potential damage done to sound science education by the absurdities of some of the claims of the more radical feminists, Afrocentrists, environmentalists, and other regiments of the anti-science academic left. By the end of the book Gross and Levitt seem confident that the dragon lies vanquished, its fire turned to ash by the cool and contemptuous sword of analytic scrutiny.

There are no last words or final victories in arguments of this sort, however. This dragon is a phoenix, and it will surely rise up to add heat to this already too heated dialogue. I think it is inevitable that Gross and Levitt's effort will stand as only one skirmish in the ongoing culture wars because the mean-spiritedness of the book weakens the force of the case it sets out to make. *Some* of the criticism the authors level at *some* of the texts they choose to examine is effective in revealing incompetence. They show, for example, how ignorant of mathematics are those critics who invoke the uncertainty principle or chaos theory in their efforts to question the causal, deterministic underpinnings of science. Even when their point is strongest, though, they cannot resist contemptuous scoffing. Still, though not all of the work in any field is good work, the body of research known as "science studies" and other targets of the authors' attack are, in general, neither leftist nor ignorant nor anti-science and can be regarded as such only by those whose reading of the literature is indiscriminate about what constitutes ignorance, the left, or anti-science postures.

First of all, the texts that get Gross and Levitt's attention range from the influential to the negligible to the bizarre, but they attack all of them with equal predatory delight. Stanley Aronowitz's *Science as Power*, so far as I know, has little influence in science studies, but it receives as much scathing scrutiny as the work of Bruno Latour and of Steven Shapin and Simon Schaffer, which is far more central to relativist and quasi-relativist work in science studies. Then there are odd omissions. Donna Haraway's *Primate Visions* is a major text in feminist studies of science, yet Gross and Levitt's only (contemptuous) treatment of her ideas cites only a brief interview and not her published works. Latour's *Science in Action*, a quirky book full of the French fondness for aphorism and paradox, is considered and dismissed, but omitted is any consideration of Latour's sober ethnography of a Salk Institute lab (coauthored with Steven Woolgar), which made his initial reputation, or his historical account of the achievements and influence of Louis Pasteur. Had the authors attended to these works, they might have been less incensed by the very virtues of Latour's work: his wit and charm as a writer, his love of concrete *things*, and his combination of practical empiricism and philosophical grace.



Ancient versus Moderns, as depicted in Jonathan Swift's *The Battle of the Books* [University of Pennsylvania Library]

Second, there is the matter of Gross and Levitt's admittedly questionable choice of the term "left" to characterize the positions they criticize. Aronowitz indeed is and long has been active in politics that are leftist in the usual sense, but Shapin and Latour? If Shapin and Schaffer's edgy characterization of the gentlemanly status required for credibility in 17th-century science qualifies them as leftists (Gross and Levitt insist that Hobbes's rejection by the Royal Society was due to his mathematical incompetence, not his inappropriate social and political credentials), then the left is in even worse shape than leftists believe it to be. Latour and Shapin are personally no more involved in politics, left or otherwise, than any professional pursuing an academic career—though, to be sure, their work itself addresses the politics of academic careers. (I should confess an interest here: Latour is a friend and Shapin a San Diego colleague.) Moreover, although some of the environmentalist concern over ecological damage from chemical pollution and other products of science and technology has a leftist cast, probably even more of it is informed by a "conservationist" (essentially conservative) tradition going back to Gifford Pinchot in the 1920s and currently exemplified by the aristocratic style of the Sierra Club.

Third, a lot of the work at which Gross and Levitt level their big guns is anti-science only by the largely unstated criteria by which the friends of science would be limited to those who see it as near-sacred, its essential work uncontaminated by worldly influence. Much of the "anti-science" criticism by feminists, environmentalists, and multiculturalists concerns either social influences on science (funding, research arenas explored, questions asked, ethnic and gender biases in personnel recruitment, and the like) or the consequences of science and its applied technology. Gross and Levitt treat these concerns as hostility to science and dismiss this set of problems as superficial and easily corrigible ones that "everybody knows" about rather than as the kinds of laundry that scientists, like most professional groups, would rather keep in-house. No doubt, some of the more

extreme current expressions of skepticism and concern about science (say, those of "mystical" environmental radicals or animal rights activists who trash labs) may be described as anti-science. But far more of the activity Gross and Levitt address reveals awe at the power of science (with an attendant desire to control it) than hostility to it. Even the most extreme misrepresentations by "Afrocentrists" of the achievements of African science can be understood as efforts to induce blacks to identify with, and share in, the achievements of science. The efforts may be pathetic, but they reveal less hostility than naïve respect and admiration.

To their credit, Gross and Levitt are somewhat conciliatory about the complaints of environmentalists and feminists, some of which they find reasonable and humane. But they seem reluctant to credit environmentalists, for example, with having brought their issues to the attention of the public. Similarly, some of the "feminist critique" of science is found unexceptionable by Gross and Levitt; they do, after all, believe in equal opportunity and are not opposed to women and minorities in science. But the increasing numbers of women in science and math are not an occasion for them to praise the efforts of feminists to make that increase possible or to see the increase as evidence of love of science rather than hostility to it. Instead, they assert that in the American universities gender discrimination is now vestigial and that "the only widespread *obvious* discrimination today is against white males."

Fourth, although Gross and Levitt are conciliatory regarding the existence of some of the social and cultural influences on "scientific practice" (although that very phrase seems to offend them), they are insistent that none of those influences affect the validity or reliability of scientific findings. Their biggest guns are aimed at the relativism of constructivists and postmodernists. For Gross and Levitt, science is internally driven by logic, method, and quantified evidence, and any suggestion that accepted scientific truths are even partially matters of convention, "social construction," or consensus formation (that is, pieces of culture) constitutes an insidious relativism that strikes at the very foundations of science's credibility. Hence a good deal of their analysis of the various texts they consider is devoted to refuting and dismissing the "vaporous" comments of postmodernists and feminists about, for example, the influence of metaphor in conceptualization (one early feminist, not cited by Gross and Levitt, rejected "hard" and "soft" science as masculine metaphors in favor of "dry" and "wet") as irrelevant to scientific conclusions.

The absolutist view of science represented by Gross and Levitt is usually designated "realism" by philosophers of science. Social constructivism is one variety of the relativisms opposing realism. Now, I think that Gross and Levitt are correct in intuiting an anti-science animus in some of the work they consider; it's part of the long tradition of romantic distrust of science. But I also think that most of the serious relativist work in the history, sociology, and philosophy of science is inspired by deep respect for science (in its generic sense as knowledge) and can best be seen as part of the effort common to all the learned professions to get at the roots of how we know. That's called epistemology, and there are enduring controversies in it that are perhaps unresolvable by the methods of science. Since the appearance of Thomas Kuhn's *Structure of Scientific Revolutions* (and despite Kuhn's disavowal of some of his interpreters), most relativists believe that knowledge is ultimately warranted by institutionalized communities whose acceptance of an empirical claim certifies it as true. Warranting communities, of course, are social constructions, but that does not mean that they are all equally fragile. As that huffing and puffing wolf discovered, some edifices are built of straw and others of brick; some social constructions have lasted as long as human history, others collapse if you look at them funny. The humanities and the social sciences are built of less sturdy stuff than the hard sciences. The warranting communities of the former are plural and diverse; they do not typically speak with a single voice and are hence not nearly as powerful as those of science, which, unlike the humanities and most of the social sciences, have close ties to powerful institutions outside the universities and tend to be single-minded except, perhaps, at the "frontiers" (another metaphor) of knowledge. Realists like Gross and Levitt *assume* (there is no epistemological argument in their book) that science is powerful because its claims are true, and its claims are true because they accurately represent an objective nature. Relativists rest content with historical and sociological accounts of how truths come to be warranted. I know of no scientific method for "proving" the preferability of one view to the other.

Is there "bad faith" working here? I think not. Most feminists, it's true, would probably not require their obstetricians to pass a feminism test, and we relativists do not go to ditch-diggers for our root canals. But it is not necessary to be a realist in order to trust and credit (financial metaphors there) warranted scientific knowledge and its applied skills. Granted, the sensibilities of realists and relativists may be radically different. Realists usually want to justify established scientific practice as the best and purest way of discovering general truths about nature (or society). Relativists usually want to reveal scientific practice as another form of human work, more disciplined than most but shot through with the same sort of historical contingencies and human frailties that characterize other occupations and professions. Though the sensibilities differ, the two camps will more often than not find themselves agreeing on the warrant for the empirical claims in a piece of research in spite of disagreeing about the bases of the warrant. This is not hostility to science but a middlingly deep philosophical difference.

One might think that such a civilized agreement to disagree about unresolvable matters would suit Gross and Levitt's implicit image of the university they love, where learned men and women go about cultivating their fields of study, politely deferential to each other. No such luck. Gross and Levitt seem more like armed sentries patrolling the boundaries of science against intrusion by outsiders. Throughout the book their posture is one of outrage that these upstarts in transdisciplinary "studies" programs (women's studies, science studies, cultural studies, ethnic studies), without science or math credentials, have the chutzpah to pronounce on matters they know little or nothing about. Credentials, though, seem not to be the heart of the matter; Gross and Levitt express equal outrage at the credentialing process itself that rewards these enemies of knowledge with refereed publication, prestigious prizes, research grants, tenured professorships, endowed chairs, symposium invitations, and other emoluments of academic achievement, which, as Gross and Levitt see it, are not only undeserved but have degraded academic life itself since its infestation by the radical countercultural spirit of the 1960s.

That such persons do win prominent positions in the academic world, then, bodes ill, according to Gross and Levitt, for the future of serious scholarship in the humanities and social sciences. This, surely, seems to be the prime motive informing this book, and the concern that allies it with the attack on political correctness. The authors' disavowal notwithstanding, what else could such a work be? Science education for nonscientists was less than adequate long before the advent of the intellectual trends the authors deplore. The work they attack, as Gross and Levitt admit, has had no impact on the practice of science itself, and graduate education in elite American universities is still the envy of the world.

Why, then, such agitation over these peripheral activities? I think it's because the

trends the authors detest have shaken up the academic status order, leaving many representatives of the traditional disciplines feeling a bit besieged, fuddy-duddified, and out of the intellectual loop. There's precious little explicit vision in this book of what the humanities and social sciences properly *do*; what little there is suggests that they are, well, *ornamental* to academic life. Science has exclusive dominion over truth; the rest of it is mere culture, properly devoted to helping others (scientists included) to live a more genteel and cultivated life. Is it any wonder that postmodernists *et al.* resist this patronizing image of their function? Ultimately, Gross and Levitt shout in the faces of the hated relativists and deconstructionists, Science works!—as if that disposed of the matter. Indeed it does work. But the humanities and social sciences work too. If Richard Rorty (Gross's colleague at the University of Virginia) is right that the mark of success in these fields is the achievement of changes in the language and character of the conversation among intellectual elites, then Gross and Levitt's ignorant anti-science left has earned its prestige. The culture wars are now a permanent part of the social process and ideological work a major occupation. Let me welcome Gross and Levitt to the fray.

Bennett M. Berger
Department of Sociology,
University of California at San Diego,
San Diego, CA 92093

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An Actor in Recent History

James B. Conant. Harvard to Hiroshima and the Making of the Nuclear Age. JAMES G. HERSHBERG. Knopf, New York, 1993. 950 pp. + plates. $35.

James B. Conant, who entitled his autobiography *My Several Lives*, was a distinguished chemist, president of Harvard University from 1933 to 1953, Manhattan Project administrator and postwar science adviser, U.S. High Commissioner to occupied Germany and first ambassador to the Federal Republic, and educational statesman who contributed a major reassessment of the nation's high schools to a Sputnik-traumatized America. His life provides a window on the American establishment during a defining time in the country's history. It was an era of engagement in issues stemming from the war-forged nexus between U.S. science, universities, government, and the military; the postwar policy of global interventionism; the conflicts and controversies surrounding the Cold War abroad and at home; and the angst provoked by the awesome possibility of nuclear annihilation.

Hershberg's *James B. Conant* is both less than a full biography and more than a "life and times." After a brief treatment of Conant's early life and career Hershberg, beginning at about the time World War II broke out, weaves Conant's life into a tapestry that includes, among other things, the making of the atomic bomb and the decision to use it against Japan; early efforts at internationalization of atomic energy; the personalities and politics surrounding the decision to proceed to the "Super" (the H bomb); McCarthyism and the academy; the Cold War and the militarization of American science; the labyrinthine politics of occupied and divided Germany; and the tortuous hammering out of American policy vis-à-vis the Federal Republic and the place of Germany in a Cold War world.

This makes for a very long book, at first glance one that is far too long. Plenty of unnecessary description, background information, and detail could have been cut from its 755 pages (929 with notes). That having been said, the book would not be so interesting had it been written very differently. Conant was an archetype of the establishment intellectual/adviser, moving freely between the academy and Washington, mingling easily with power brokers in science, government, the military, and the media, and helping shape the society and state that emerged out of World War II and the Cold War. He was engaged with some of the most complex issues of his time, and he had complex opinions and views about them. Drawing on research in a prodigious number and variety of sources, including the papers of a large array of characters, 68 personal interviews, and a vast secondary literature, Hershberg presents his subject in the thickness of the period's history.

Conant is difficult to write about. He was reticent to a fault about his personal life and feelings (he neglected to include his marriage in the first draft of his autobiography) and was frequently characterized as dour and cold. The inner man is a mystery. After living with Conant, as it were, for more than 10 years, Hershberg reports that to this day he does not know whether or not he likes him. In some respects liberal, in others conservative, Conant's general outlook defies categorization.

Who is the figure, then, who emerges from Hershberg's study? Conant was as responsible as anyone else for the atomic bomb's development and use. (In fact the Interim Committee's fateful recommendation that the first bomb be targeted on a "vital war plant employing a large number of workers and closely surrounded by workers' houses," quoted by Hershberg on p. 225, was Conant's suggestion.) But he never suffered a guilty conscience. To him war was the evil, winning it an awful necessity, and no one means toward that end more immoral than another. Yet he felt a great responsibility to ensure that the destructive force he helped unleash not blow up the world. To Conant the H bomb, too powerful to confine to military targets, was a genocidal instrument; he played a major role in moving the Atomic Energy Commission's General Advisory Committee, whose weapons subcommittee he chaired, to recommend against its development. (His role in the H bomb debate earned him the enmity of a group of right-wing scientists, who Hershberg believes likely were behind the failure of Conant's candidacy for president of the National Academy of Sciences.)

Conant initially believed that the Soviet

"Harvard's president in his Massachusetts Hall office after World War II. On Conant's desk, his souvenir from the Trinity test: a glass-encased fragment of ground zero." [From *James B. Conant*]