CHAPTER ONE

Science and Science Studies

Enemies or Allies?

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CONTEXTUALIZING THE "SCIENCE WARS"

This book has three aims. One of them is to try to make sense of the recent debate about science and "antiscience." We will bring light to bear on this question from a variety of perspectives: sociological, historical, philosophical, and scientific. The second aim is to open up a larger discussion about the relationship between the field of science studies and its object, science itself. What is the possible and desirable relationship between scientific practitioners and those who study their activity within science studies, or STS?[1] In the Science Wars, the relationship appeared strained; yet earlier the coexistence between these two scholarly communities could be described as friendly and cooperative. The third aim is to present some missing voices and viewpoints when it comes to the relationship between science and society, including those of two Grand Old Men of STS. Finally, this book hopes to clear up a deep confusion. Unfortunately, the proscience activists in the Science Wars tended to mistakenly collapse the social science–oriented science studies with the new "postmodern" critique of science in the humanities, two quite different academic enterprises. *Beyond the Science Wars* explicitly focuses on STS, although it sometimes broadens the discussion.

This book is directed as much to scientists and the general public as to practitioners and students of STS. The ambition is to clarify a number of issues and raise some new ones that have been suppressed by the very terms of the debate in the Science Wars. Some important things need pointing out from the very beginning. The Science Wars should not be seen as an opposition between scientists and science studies scholars per se. It has been waged by a relatively small *minority* of "proscience activists" against a *particular* school within STS, "the sociology of scientific knowledge," or SSK—a term standing for social constructivist and relativist orientations (and, when it comes to the humanities, against a particular "postmodern" school). Still, since the proscience activists often acted in the *name* of science, and since the social constructivists often were sociologists, the impression may have been given that we here had a deep opposition between science on the one hand and sociology on the other.[2] Not so. The disagreement was between science and the *"social constructivist" and relativist* type of sociology.[3]

Of course, from the proscience activists' point of view, the Science Wars probably started much earlier—some three decades ago with the rise of the social constructivist paradigm in science studies (and in the humanities, with the rise of postmodernism). In this interpretation, it was the social constructivists and postmodern humanists who were the original aggressors, and the 1990s Science Wars was only a reaction by spokesmen for the long-suffering scientists. In any case, it should be noted that during that period, there existed an internal intellectual opposition to social constructivism *within STS* itself, although the voices of these opponents often drowned in the exuberance of social constructivist expansion. This book is not the place to go into the details of this critique, but comprehensive critical analyses exist (e.g., Cole, 1992; Fuchs, 1992; Hagendijk, 1990; Laudan, 1981) and even an alternative program was developed in response to constructivist claims (Schmaus, Segerstråle, & Jesseph 1992).

When initially, in what became known as the "Science Wars," a vocal group of proscience scientists in books, articles, and well-publicized conferences accused some of their fellow academics in the humanities and social sciences for being "antiscience" the attacked parties were taken aback. They felt that it was their academic prerogative to do what they did—that is, treat science as their object of study and subject it to various types of critical analyses, as they saw fit. Moreover, the critical analysis of science had been going on for quite some time without any objections. In the search for explanations for the attack of the "science warriors" (particularly Gross and Levitt with their *Higher Superstition*, published in 1994), it was tempting to try to find the explanation in the existing problems for science, and many did so (Nelkin, 1996 a, b; Ross, 1996). A representative argument was that scientists were in trouble and were scapegoating others; they wanted back to the good old days of abundant science funding.

Indeed, at the time there had been several events of adverse publicity for science, ranging from notorious misconduct cases all the way to the

Unabomber.[4] Some of these cases should probably be considered matters of technology or decision making rather than of science—for instance the Challenger disaster and the problems with the Hubble telescope. Still, these were only some of the problems on the long list included in the special issue "Science under Siege," making the cover of *Time* Magazine in 1992. For many, the most blatant setbacks for science were clearly the closedown of the Superconducting Supercollider in 1993 and the closing of the OTA, the Congress's Office of Technology Assessment. These events seemed to send clear signals from society that the fat years of science spending after World War II and the Cold War were over and that science would be facing serious budget cuts.[5] Still, the adverse situation for science was typically not at all referred to by the proscience warriors themselves at the time. They focused on what they viewed as a threat to science coming from inside academia, from the "antiscience" attitudes that had developed within (parts of) the humanities and social sciences. Let us take a closer look at the academic developments in the sociology of science during the two to three decades preceding the eruption of the Science Wars in 1994.

THE CONSTRUCTIVIST CRITIQUE

Around the mid-1970s, traditional sociology and history of science had given way to new research programs promoting the idea that science was "socially constructed," or suggesting that science was on a par with other knowledge systems, such as Azande witchcraft. These new constructivist or relativist approaches within the newly created field of "the sociology of scientific knowledge" (SSK) postulated among other things that scientific truth had no preferred epistemological status in relation to other truth claims: science was just one among many belief systems, all explainable by social factors. The ground had already been prepared by empirical studies showing that scientists in practice did not follow the "norms of science," that very backbone of the traditional sociology of science promoted by Robert Merton (1942/1973) and his students.

What these new sociologists had dared to do was open up also the content of science to sociological analysis. The Mertonian school had typically treated science as just any other social system, assuming that the norms of science (and their institutionalization in scientific reward and control systems) somehow guaranteed the rationality and objectivity of the knowledge produced. Even the creator of the sister field of "the sociology of knowledge," Karl Mannheim, had declared that natural science was the exception to the rule that knowledge was in general influenced by social ideologies.[6]

One of the great inspirations for this daring move had come from a particular reading of Thomas Kuhn's famous *The Structure of Scientific Revolutions*. For the new sociologists of scientific knowledge, Kuhn was sending a seemingly liberating message: science as an enterprise was not a paragon of rationality after all, it allowed for considerable irrational elements during times of scientific revolutions and scientific paradigm change, during which scientists underwent a type of "conversion" and came to see the world in a totally different way. The new sociologists of science here saw a chance to forcefully introduce *social* instead of philosophical explanations for scientific change—a marvelous opportunity, too, to steal the show from the rationalist philosophers of science who had hitherto dominated the field. With the introduction of the sociology of scientific knowledge, the new sociologists of science had effected nothing less than a self-conscious Kuhnian revolution and paradigm shift within the sociology of science itself! (Kuhn himself was very unhappy, however, with this new "social" interpretation of his thesis, as already an interview I conducted with him in 1982 clearly indicated.)

Many sociologists of science welcomed these fresh new research frameworks proposed by The Strong Programme (promoted by a group at the University of Edinburgh) and The Empirical Program of Relativism (championed by Harry Collins, then at the University of Bath). Others found the French sociologist Bruno Latour's independently developed "actor-network" theory compelling. This Macchiavellian model suggested that science be best studied in terms of a network of actors "enlisting" other actors—and machines—in strategical schemes for winning the scientific game.[7] A whole new academically lucrative research industry now got started, churning out case study after case study, surprisingly supportive of the new dominant paradigm—just as the Mertonian paradigm had earlier supported the vision of science as guided by a particular set of norms.

In all this, the suggestion that sounded most radical to the innocent ear (and to baffled colleagues of the constructivists within STS) was probably the constructivist assertion that scientific *facts* themselves were socially constructed. This view implied that what came to be counted as "facts" was really more a matter of convention or contextual factors than of inherent scientific necessity. The new sociologists of science backed up their claims by ethnographic laboratory studies, which were then cited as exemplary cases by others. (Kuhn had shown how scientists tended to be convinced by "exemplars".) Under such an onslaught of social constructivism, the physical world itself now seemed to come to pieces. Indeed, in the 1980s, many a conference of the Society for the Social Studies of Science was highlighted by ardent disputes between constructivists and realists, or rather, moderates, who dared to insist that the real world did, indeed, constrain science in some ways.[8]

The depth of the constructivist conviction remained unclear even to STS colleagues—when they said that facts were socially constructed, did constructivists mean that there *existed* no scientific facts, or was this merely a rhetorical, or perhaps methodological claim? This seemed never to be satisfactorily resolved even in open disputes. But the important point was not the ontological-sounding claim itself. It was rather that *if* it could be shown that facts could not play the determining role in science that had been earlier attributed to them—for instance, if they could not settle scientific disputes—then the door could be thrown wide open for various types of "social" interests and influences instead; science could be legitimately reduced to a power game.

This also had implications for the history of science. If it could be argued that there "could be" no scientific justification for choosing one theory over another (and there were, indeed, well-known philosophical arguments available), it would seem more legitimate to try to find the social reasons why one theory was historically more successful. The task for the historian would no longer be to describe such things as the working of scientific rationality in a particular historical context, or even the interaction between social and scientific factors—reasonable approaches that did acknowledge the importance also of social factors in science. Instead, the task became to demonstrate the fundamentally social reasons behind even the most abstract-looking scientific ideas. Thus, it was taken for granted that also the scientific convictions of scientists could be unproblematically reduced to social and political interests.[9]

While the new interdisciplinary field of social studies of science was slowly moving into an increasingly constructivist direction, emerging new fields such as cultural studies and women's studies quite independently also chose science as one of their primary objects of analysis. They were interested in studies of the Western bias of science or its inherent masculinity, usually with the implicit or explicit assumption that science as we know it could be otherwise. Students of rhetoric examined the rhetorical strategies of scientists, and the field of literary criticism, inspired by French postmodernism, started treating science as one of many "texts" to be "deconstructed."[10] Also here we had a questioning of the traditional image of science, a downplaying of science as a rational pursuit, and an emphasis instead on science as power. Thus, there was one thing that seemingly united the new science studies and the new postmodernist humanist studies: they both veered away from the idea of science having an epistemologically privileged status.[11]

What about the voice of the scientists themselves, after all the objects of these studies? Unlike earlier sociologists of science, who relied on scientists' own statements, the new science scholars largely ignored what the scientists themselves had to say about their scientific commitments and concerns, or how they judged good science from bad. It is not too much to say that a certain "Besserwisser" approach prevailed, with the sociologists smugly overruling the scientists. It was as if the sociologists were the self-appointed psychoanalysts of scientists, knowing their "true" motives, unbeknownst to the scientists themselves. Unlike earlier sociologists, many of the new sociologists deliberately chose to study scientists using various ethnographic methods. Unlike anthropologists, however, the new explorers did not think that the members of the scientific tribe themselves could act as informants for valuable insights into their world. Meanwhile, for considerable time the objects of study, the scientists themselves, did not seem to be aware of or care about these developments.[12]

1994—THE ANNUS HORRIBILIS AND AFTER

1994 can be characterized as the Annus Horribilis, the year when "the scientists" struck back. But, of course, it was not necessarily "the scientists"—it was rather Paul Gross and Norman Levitt with their book *Higher Superstition: The Academic Left and Its Quarrels with Science*. This book took issue with what it called the academic or "the cultural left," an umbrella term the authors used to bunch together a number of academic endeavors: social constructivists, postmodern humanists, feminists, environmentalists—in short, the many different academic strands engaged in contemporary critical analysis of science. This was also the year of the first of two conferences arranged by The National Association of Scholars, an organization typically working toward bringing back a traditional university curriculum, now almost exclusively engaged in combatting a purported "antiscience" threat. Eminent speakers at these well-publicized events included the Nobel laureate, physicist Steven Weinberg, and Harvard's Edward O. Wilson and Gerald Holton. Already in 1993, Holton set the tone with his essay collection *Science and Anti-Science*, which warned about the dangers of a new irrationalism in society (for a closer analysis of the whole notion of "antiscience," see chapters 4 and 5, this volume).

There were also various indirect skirmishes between representatives for the larger scientific community and its critics, notably in relation to the museum exhibition "Science in American Life." This Smithsonian Institution event was funded by the American Chemical Society and executed by museum curators, who (perhaps with an eye to their academic colleagues) decided to incorporate also social criticism of science in the displays. But the result was soon seen as too negative in spirit and as going against the intent of its original sponsors, the chemists (Flam, 1994; LaFollette, 1996).[13] Incidentally, "Science in American Life" also became the focus of some acrimonious exchange between the two emerging camps in the new debate about science (Gieryn, 1996; Gross, 1996).

As to direct confrontations between scientists and academic science critics, there were at least three memorable ones. One was a famous "showdown" between sociologist Harry Collins and biologist Lewis Wolpert at the British Association for the Advancement of Science (BAAS) in Loughborough, U.K., in September 1994 (see, e.g., Rose, 1996, and Fuller's chapter, this volume). At that conference, it became apparent to many that social constructivists and scientists had difficulty speaking to one another. In any case, the initial (non)exchange of views at BAAS was later followed up in the pages of the *Times Higher Education Supplement* (September 30, 1994). In December the same year, there was a follow-up conference in Durham, U.K. The aim of this second conference was expressly to bring scientists and social scientists together, a move that the organizer, Steve Fuller, himself characterized as "desperate" (personal communication). But that conference did not become the hoped for celebration of mutual understanding. A particular problem seems to have been the focus on case studies, where the two parties could not see eye to eye (for more on the Durham conference, see Fuller, 1995, and this volume).

The third occasion was a panel debate with Gross and Levitt at the Society for Social Studies of Science (4S) Annual Meeting in Charlottesville, Virginia, in October 1995, which I attended. For several reasons, this can rather be described as a nondiscussion. If the aim of this debate was to seriously address the content of Gross and Levitt's book, it was dramatically unsuccessful. Gross and Levitt mostly restated their views, Gross reading a written statement. Among the panelists, the only representative for the many academics who had been criticized in *Higher Superstition* was the feminist Donna Haraway. This part of the debate soon turned into a nasty exchange concerning Haraway's own and other feminist critics' scientific training. What was most disappointing for the ballroom-size expectant audience was that not a single constructivist appeared on the panel or spoke up from the audience. The only good thing was that Gross and Levitt in reponse to questions stated their own irritation with the contemporary "academic left" much more clearly (see chapter 4 this volume).

WHY THE CONSTRUCTIVIST POSITION BOTHERS SCIENTISTS

What happened at more informal meetings between scientists and constructivist academics? One scientist reported that he had attended a seminar where a constructivist asserted that it was in principle possible for there to exist a chemical element between hydrogen and helium in the Periodic Table. For this scientist, this was "strong" constructivism; indeed, he could not see how anyone could seriously hold such an absurd belief (this was a spoken comment in audience at the 4S conference in the session "From Proscience to Antiscience—And Where Next?," which I arranged at the meeting; see also Bauer, chapter 2, this volume). Interestingly, a more common complaint was that "strong" constructivists, when challenged, typically regressed toward the uninteresting and toothless assertion that science is influenced by social factors, that is, a "weak" constructivist stance. It seemed hard to find real, strong constructivists to argue with.

Richard Dawkins (the author of *The Selfish Gene*) appeared to have got lucky, however, since he had, indeed, been able to present a constructivist social scientist with the following question:

> Suppose there is a tribe which believes that the moon is an old calabash tossed just above the treetops. Are you saying that this tribe's belief is just as true as our scientific belief that the moon is a large Earth satellite about a quarter of a million miles away? (Dawkins, 1994, p. 17)

The constructivists' reply was that truth is a social construct and therefore the tribe's view of the moon is just as true as ours. Dawkins now went on to wonder why sociologists or literary critics traveling to conferences did not choose to entrust their travel plans to magic carpets instead of Boeings. "Show me a cultural relativist at 30,000 feet and I will show you a hypocrite," Dawkins concluded (Dawkins, 1994, p. 17).

But what was the rationale behind the constructivist or relativist position? How could the proponents of such views maintain a seeming absurdity of this magnitude? It turns out that the proponents of the new social studies of science were wielding a surprisingly *unsociological* argument as their weapon: an abstract philosophical claim. First they pointed out that science cannot be justified philosophically (they were right—there are, indeed, various problems, most famously the Duhem-Quine thesis, which says that scientific theories are always underdetermined by facts). However, from this abstract reasoning they felt free to conclude that, therefore, in *practice*, too, scientists "could" never have good enough factual evidence to convince themselves and each other, and therefore it "must" be something else that influenced scientific judgment. Social factors! QED. (For this kind of position, see particularly Collins, 1985.)

But one scientist put his foot down in response to this kind of reasoning—and that even before Gross and Levitt. That was Lewis Wolpert, in his book *The Unnatural Nature of Science* (1992/1993). This book appeared to be responding directly to the claims of Collins's "empirical program of relativism," which programmatically refused to grant science any epistemological privileges. (According to that program, the burden of proof was rather on *science* to demonstrate its superiority over common sense

[Collins, 1982, 1985]. We can now see why Wolpert locked horns with Collins at the BAAS in 1994). In his book Wolpert insisted that science *was* indeed different from utility-oriented common sense—science's very wish to understand the world was already "unnatural." Meanwhile, the purported philosophical obstacles for science did not bother Wolpert at all. He simply declared that philosophy of science was of no help for scientists anyway, since scientists had their own criteria for judging scientific theories: such things as parsimony, comprehensiveness, fruitfulness, even elegance. These rules of thumb might not be philosophically justified, but they *worked*, and that was what mattered! (Wolpert 1992/1993).

What probably most upset scientists in general, however—not only the proscience activists—was the suggestion that science was not the objective enterprise it purported to be, or worse, that it could not be objective. This sounded like a combined epistemological and political assault on traditional science, and did indeed appear very similar to the points made by the postmodern and cultural critics of science within the humanities. It may have been on these grounds that Gross and Levitt (1994) classified both postmodern humanism and social constructivism under the common term "cultural constructivism." But while the perceived *effect* of the critical analyses of science may well have appeared to be the same for Gross and Levitt, in reality the nature of the criticisms of these two groups were quite different. What, then, were the differences?

When postmodernist humanists and various types of "standpoint epistemologists," such as a particular brand of feminists, said that science was socially or culturally constructed, they were primarily interested in *values and ideology*. Moreover, the claim was not merely that political, cultural, and personal values *affected* scientific theories—science was seen as *inherently* value laden. For some standpoint feminists, for instance, the very idea of objectivity became a masculine conspiracy (e.g., Harding, 1991).[14] For others, since they saw any theoretical framework as necessarily implying a particular ideological stance (something that could always be demonstrated through a close analysis of a theory's underlying assumptions), even a seemingly theoretical discussion automatically referred to a wider political discourse. Science, therefore, *de facto* became politics, since there existed no objective external arbiter for judging between competing research frameworks or paradigms (e.g., Longino, 1990).

In contrast to this—although Gross and Levitt did not recognize it—the core concern of the sociology of scientific knowledge and its various constructivist and relativist ambitions was not really values and ideology at all. SSK had all the time been primarily interested in *epistemology*, and in demonstrating that a traditional rationalist philosophical explanatory model for science could no longer be justified.[15] The perceived opponents of the social constructivists were in fact the philosophers of science with their rationalist claims. This is why the workers in the new social constructivist paradigm spent enormous energy on showing just how social factors actually enter the knowledge production process, from the involvement of "social interests" (rather than rational judgment) when it came to theory choice, to social (rather than rationalist) explanations for closure of controversies, to social construction or "negotiation" of scientific facts in laboratories, and to the social foundation of all knowledge, including science. (The agenda changed somewhat over the paradigm's lifetime, cf. e.g., Shapin, 1995.)

When it came to values and ideology, however, the sociology of scientific knowledge was in fact sometimes internally criticized for being *insufficiently* concerned with these matters (e.g., Chubin & Restivo, 1983; W. Lynch, 1994, and the discussions in the special issue of *Social Studies of Science*, May 1996, "Politics of SSK"), or uninterested in important social problems of science and technology (see Bauer, chapter 2, this volume). One reason why Gross and Levitt so disliked social constructivism and tended to believe that it was politically motivated, was probably the sharp distinction they themselves made between science and ideology. Science is objective while ideology is socially influenced—or "socially constructed." For them, therefore, *saying that science is socially constructed was the same as saying that science is inherently ideological*—absolutely anathema to their view of science as an objectivist and universalist oasis. This may be why they, in their book, so unproblematically collapsed social constructivism with postmodern humanism, and suggested that "ideology" (or "Theory") were driving both. (For further discussion of science as universalism, see chapter 5.)

Still, it is clear that, independently of its *intent*, social constructivism may well have political *effects*. But what kinds of effects should we assume? Is a book like Harry Collins and Trevor Pinch's recent *The Golem: What Everyone Should Know about Science*, where scientific knowledge is described as fundamentally "contestable," a harmful or positive contribution to the public understanding of science? Fuller in chapter 9 presents these authors as actually wishing to *help* science, by scaling down people's unrealistic expectations of this enterprise. In their book Collins and Pinch themselves, too, appeared to consider it a *democratic* thing to declare that science is "contestable" (they compared it with DNA fingerprinting, which they presented as both unreliable and causing convictions of innocents). People like Gross and Levitt, however, would regard it as a *weakness* if society's progressive forces did not have at their disposal reliable science. For them, the force of the Left is a fundamentally moral one, whose claims about social injustice and inequality rely on potential backup by incontrovertible facts (more on this in chapter 5, this volume).

Indeed, for the many who believed that the scientists who were raising the specter of "antiscience" were social conservatives the best evidence to the contrary might have been the fact that Gross and Levitt among "acceptable" social analysts of science were willing to count Stephen J. Gould, an outspoken left-wing political critic of science. In fact, in Gross and Levitt's book, Gould came off as something of a model social analyst of science! But, if anything, Gould was well-known for his political criticisms of science. How could this be explained? The explanation may be that Gross and Levitt's criterion for an acceptable social study of science was that the researcher should be willing to acknowledge that science was a realm *analytically* separate from politics, even though in practice social values might influence science (Gould's writings often seem to reflect just this kind of position).

Also, it would be incorrect to believe that the scientists on the war path against social constructivism and relativism were uniformly set against all kinds of social studies of science. Indeed, the very same scientists who were opposed to constructivist or postmodern approaches in social studies of science pointed out that they were *not* against philosophical, sociological, and historical studies of science as such. For instance, the physicist Steven Weinberg even declared himself a "history of science buff," and said that he found some types of sociology of science useful and its results plausible. What he disliked was the suggestion that the demonstration of social influences on scientists would affect the *truth* of their theories (Weinberg, 1992, 1996b). Gross and Levitt, too, declared that they approved of traditional philosophy and history of science (Gross & Levitt, 1995; Gross, 1997). (These qualifications became more visible, however, after the impact of their original attack, which could be easily seen as directed against all kinds of historical and social studies of science.)

In a polarized climate, little distinction will be made between, say, constructivist sociologists and nonconstructivist ones—everybody gets tarnished by the same brush, including sociology as a field. And worse, in this kind of conflict the very attempt to *analyze* the situation gets easily misunderstood—by both sides. I have personal experience here, already from two occasions. My sociological analysis of the sociobiology controversy has often been seen by partisans in this debate as either support for sociobiology or for the critics of sociobiology, not as an attempt to understand the debate itself. In other words, the very analysis of a controversy became identified with its object. A vivid example of this phenomenon in regard to the Science Wars was my own attempt to provide a forum for general discussion about the reasons for the seemingly sudden attack on "antiscience" at the Annual Meeting of the Society for Social Studies of Sciences in Charlottesville in 1995. For some reason, the organizers made my session one of the opening ones of the conference (perhaps because it featured sociologist Bernard Barber, who was later to receive the society's Bernal Prize). However, *Academic Questions*, the organ for the National Association of Scholars, later presented my session as but the first of the many constructivist sessions to follow (Zürcher, 1995).

My particular panel ("From Science to Antiscience—And Where Next?") did not feature a single constructivist, something that should have been quite obvious. Indeed, the provocative title of one of the papers, "Antiscience in STS," should have outright pleased the National Association of Scholars' rapporteur. However, in her article, she simply omitted this and other inconvenient data points. She even succeeded in making Grand Old Man, proscience sociologist Bernard Barber seem like a dangerous-sounding feminist! Indeed, all this construction work may well have been necessary, given the title of her article: "Farewell to Reason: A Tale of Two Conferences," which contrasted an "all bad" 4S conference with the "all good" "The Flight from Science and Reason" conference. (Perhaps not by coincidence, the author of this blatant data-selection job later became the Research Director of the National Association of Scholars, I am sorry to report.)

WHO HAS THE RIGHT TO CRITICIZE SCIENCE? A HIDDEN ISSUE IN THE SCIENCE WARS

I now want to move on to one of the fundamental hidden topics in the Science Wars. That is, the question Who has the right to criticize science? Or, put in more technical terms, Who is a competent critic of science? Other scientists in the same field? Any scientist? (Scientists themselves have divergent views on this, see, e.g., Segerstråle 1993.) The public and its representatives? Social scientists, literary critics, and others whose object of study is science? Furthermore, does an academic analyst of science have to master science? (Gross and Levitt kept pointing out that their critics did not know science and, like Wolpert, emphasized the difficult and mathematical nature of science.) What about the public? Does a member of the public have to know science in order to voice criticism?[16]

The proscience warriors seem to have been particularly irritated with the temerity of those who did not have formal credentials in science to criticize their enterprise. From their point of view, what we had here were "outsiders" to science presenting themselves as competent critics of science. How could this be explained at all, except as sheer arrogance? Here it is important to distinguish between the "postmodern" and cultural critique, and the social constructivist or relativist epistemological one. In regard to the former type of science criticism, science was seen as simply part of general culture, ineluctably permeated by social values and ideologies and had no particular basis for its claims to objectivity. In such a situation, it would seem legitimate for "outsiders" to comment on science, because they would no longer be outsiders.

The situation was quite different, however, for the social constructivists and relativists (many of whom, incidentally, did have science degrees). They, too, saw themselves as radical critics of science, but as radicals of an *epistemological* rather than political kind. To quote a spokesman for the "post-Mertonian" sociology of science, sociology of scientific knowledge had seen itself as "new" and "radical," because it was tackling the very *content* of natural science and mathematics, unlike both the "old" sociology of science and traditional Mannheimian sociolology of knowledge. But, this spokesman observed, arguing for the social construction of, say, mathematics, did *not* imply any determined correlation between a *specific* type of mathematics and a *specific* type of social "power arrangement!" It was rather aimed at "counteracting *metaphysical* claims about an 'asocial' foundation for the practices and results of mathematics" (M. Lynch, 1992; my emphasis). Also, Collins in a review article (1983) noted that a typical motivation of the Strong Programmers of the Edinburgh School—the leaders within the new sociology of scientific knowledge—was dissatisfaction with existing paradigms for explaining science, such as Mertonian norms or rationalist philosophy; that is, they were interested in carving out a new *intellectual* niche, rather than in criticizing science per se.

Looking at some of the early publications of that school, however (e.g., Barnes & Shapin, 1979; MacKenzie, 1981), it is hard to consider the Strong Programmers' early concentration on social 'interests' and their connection to social classes as *merely* a sociological analysis without any political connotations whatsoever. For some leading members of the new sociology of scientific knowledge the very act of undermining and "sociologizing" science's claim to a special epistemological status undoubtedly *also* had the connotation of undermining "elitist" scientific expert power in favor of "democratic" common sense knowledge. This was especially true of the Epistemological Program of Relativism (Collins, 1982; Collins & Pinch, 1993). And finally, although the statements by the *originators* of the sociology of scientific knowledge may have represented an apolitical interest in the foundations of knowledge, the *followers* of the SSK program seem to have often regarded their own mission as intellectual-*cum*-political.

Whatever the constructivist intent, for Gross and Levitt and other proscience activists, it was the *consequences* that mattered. And it was is hard to deny that there were potential social consequenses of promulgating a "postmodern" or a constructivist/relativist view about the nature of science. If an impression was created that science as a knowledge system had no particular privileged status in relationship to other ways of knowing, then this could be seen as a de facto legitimation for, say, creationism, or a direct endorsement of "junk science" in the courtroom. In such a climate scientists would have to defend their requests for research money even more fiercely. In practice, therefore, whatever the intellectual content of the Science Wars, it was at the same time a contest about which side, the proscience warriors or the new epistemological critics of science, would be able to define itself as superior in the eyes of society.[17]

We had, then, an interesting situation with two totally different visions of the nature of scientific knowledge pitted against each other. The discussion seemed completely locked. At the same time, it was hard to imagine an external arbiter of some kind—What kind of person could that conceivably be? For the militant scientists, on the one hand, a nonscientist was not competent to speak about science; for the postmodernist humanists and social constructivists, on the other hand, requiring scientific competence might have easily been dismissed as a defensive move or the mystification of expert power. What, in this situation, might conceivably have the power to cut through the Gordian knot of the Science Wars?

THE MEANING OF ALAN SOKAL'S HOAX

One thing that, arguably, did this was the famous Sokal Hoax. The physicist Alan Sokal found a way, despite the impasse, to communicate between the scientific and postmodern worldview, and at the same time conduct what at least for many scientists looked like a crucial experiment. (Note, however, that Sokal's target was the postmodern and cultural criticism of science rather than the social constructivist one within science studies. Still, wide-ranging conclusions for all types of critical analyses of science were typically drawn from the Sokal Hoax.)

In an article in *Social Text*, a leading cultural studies journal, Sokal declared that he as a physicist wished to take the "deep analyses" of certain cultural critics of science one step further by linking them to recent developments in quantum gravity. He wrote about a conceptual revolution with "profound implications for the content of a future postmodern and liberatory science" (Sokal, 1996a, p. 218). For a postmodern piece, his article was suitably entitled "Transgressing the Boundaries: Toward a Transformative Hermeneutics of Quantum Gravity." But in a different journal, *Lingua Franca*, Sokal simultaneously revealed that it had all been a hoax: he just wanted to test whether a piece that was written in the right style and espoused the correct political views, would be accepted as genuine (Sokal, 1996b). And on the face of it, Sokal did prove his point: his article did get published by unsuspecting editors, who were seemingly taken in by his writing. For instance, the following passage was well-tailored to meet the beliefs of the cultural left:

> It has thus become increasingly apparent that physical "reality," no less than social "reality," is at bottom a social and linguistic construct; that scientific "knowledge," far from being objective, reflects and encodes the dominant ideologies and power relations of the culture that produced it; that the truth claims of science are inherently theory-laden and self-referential; and consequently, that the discourse of the scientific community cannot assert a privileged epistemological status with respect to counterhegemonic narratives emanating from dissident or marginalized communities," (Sokal, 1996a, pp. 217–218)

The hoax made the first page of *New York Times* and triggered an avalanche of discussion in that paper and elsewhere. But what were the real implications of Sokal's hoax? For some it suggested that editors of cultural magazines were not capable of distinguishing serious from nonserious reasoning as long as the *form* was right and the article reached expected political conclusions. For others, it indicated that the editors may have been pleased by an apparently postmodern contribution coming from a scientist. Still for others, it suggested that Sokal's political qualifications as a leftist (he represented himself as having worked in Nicaragua during the Sandinista regime) had misled the editors about his true convictions (that he was not a cultural leftist). Indeed, in their response, the editors wrote about deception and break of trust—a surprisingly nonpostmodern complaint (Ross 1996). (See also Stanley Fish, 1996, speaking for cultural studies.)

Now Sokal's hoax may be used as a just-so story for many things. For some, it seemed like the ultimate test of the possibility of communication between the Two Cultures, while it highlighted an important asymmetry in each culture's capability to assert the academic power of its own position. Did the success of the hoax perhaps suggest that physicists could figure out what it took to get published by humanists, but not vice versa? Or did the result demonstrate a point often raised in the Science Wars, that those who passed critical judgment on science did not know its content—in this case, not even what passed for a reasonable scientific argument? (Sokal had made sure that his scientific documentation was impeccable, but he had made deliberate "funny" mistakes in scientific inferences, not clearly perceptible to nonexperts.) If so, in both cases, Sokal would in fact have supported the received view that science *was* more "difficult" than the humanities—a view already conveyed by Gross and Levitt.

Sokal himself affirmed that he had got inspired exactly by reading Gross and Levitt. As he explained at the conference "Science and Its Critics" at the University of Kansas in late February 1997, when reading *Higher Superstition*, he had wondered if the quotes of postmodernists were really representative or if they had perhaps been taken out of context. So he had gone to the library to find out, he said—and lo and behold, the quoted passages were even worse in context! Sokal assured the audience that his hoax was "really very, very funny," but noted that the most hilarious part of his article was not even written by him—he had simply quoted the silliest quotations he could find. "Don't miss my footnotes!" he said and giggled to the audience.

In the same talk, Sokal also professed his surprise at the fact that his prank had reached the front page of the *New York Times*, and that "his name has now become a verb." According to Sokal, the whole thing had taken on a magnitude ten times bigger than expected. He also said that he disliked the term "Science Wars." Still, his hoax was undeniably fed into an ongoing debate, and young Sokal could hardly have failed to realize that he had now de facto aligned himself with the science warrior camp.

Obviously, too, the aim of his article was not only to poke fun at postmodern jargon. Sokal's *Lingua Franca* revelation shows an attitude quite similar to that of Gross and Levitt (who were, indeed, very pleased with him; Gross & Levitt, 1996). Sokal described his hoax as an "experiment" to test the intellectual standards of a certain academic subculture. According to him, the results "demonstrate, at the very least, that some fashionable sectors of the American academic Left have been getting intellectually lazy." But why did he choose the medium of parody? Why not simply demonstrate to the postmodernists that they were wrong? Sokal explained that the subculture "typically ignores (or disdains) reasoned criticism from the outside," so parody was the only way to get through to them:

> In such a situation, a more direct demonstration of the subculture's intellectual standards was required. But how can one show that the emperor has no clothes? Satire is by far the best weapon; and the blow that can't be brushed off is the one that's self-inflicted. I offered the *Social Text* editors an opportunity to demonstrate their intellectual rigor. Did they meet the test? I don't think so. (Sokal, 1996b)

Through his action, however, Sokal did not only hoax the editors of *Social Text*. What is less known is that he in this way subverted the radical intent of that whole issue of *Social Text*. That particular issue entitled 'The Science Wars' was in fact exactly intended as a cultural left response to Gross and Levitt (Ross, 1996a). In that issue, the Sokal piece ended up somewhat tagged onto the other pieces, which were all dealing with the recent rift between science and cultural studies of science. (Sokal's piece was duly omitted in the later book version of the special issue; Ross, 1996b.)

And there were further implications. The hoax soon became a convenient vehicle for those wishing to prove points about cultural studies of science. Indeed, Sokal himself in his early response had epitomized the "hardline" scientific attitude in regard to cultural studies. He stated categorically: "*Social Text*'s acceptance of my article exemplifies the intellectual arrogance of Theory—that is postmodern literary theory—carried to its logical extreme," where "[i]ncomprehensibility becomes a virtue; allusions, metaphors and puns substitute for evidence and logic" (Sokal, 1996b, p. 63). Against this he asserted his own position:

> There *is* a real world; its properties are *not* merely social constructs; facts and evidence *do* matter. What sane person would contend otherwise? And yet, much contemporary academic theorizing consists precisely of attempts to blur these obvious truths. (Sokal, 1996b, p. 63)

It was only later that Sokal started backing off from the intent and implications of his hoax. For instance, he began his talk at the Kansas conference by pointing out that "not much" could be deduced from the fact that his hoax was published. He said it did not prove, for instance, that intellectual standards were lax in general, and so on, "only that *one* journal published an article that they *admitted* that they could not understand, solely because it came from a 'credentialed' person." This was obviously a sound conclusion by a scientist who had, after all, only one data point!

This raises an interesting question when it comes to the proscience activists' attitude to Sokal's hoax. To be consistent, should not Gross and Levitt have treated the Sokal hoax in the same way that they treated instances of scientific fraud—declaring it an isolated case, proving nothing? Gross for example, downplayed scientific fraud as "based upon no frequency data, absolute or relative to other professions" and as merely "a few, highly publicized misconduct cases, some of which have been dismissed" (Gross, 1997). But when it came to Sokal's hoax, the interpretation was quite different. The hoax was not regarded as a lonely data point, calling for real frequency data. Instead, it was seen as a type of *legal precedent*. In the Sokal case, hard-line scientists had made a surprising move from their usual quantitative scientific standards toward a case-oriented legalistic attitude! This was reflected, for instance, in Gross's own contention that the targets of the Sokal hoax ought to have learned a lesson from the hoax:

> You might imagine that with egg on their faces, cultural studies entrepeneurs would have repaired to the ladies' and gents', respectively, to wash up, resolving to do better or at least to attend henceforth to the content of the science they study. But no. Academic scholarship used to be like that; now it is not. Some parts of academic life are a political game—as the best players insist—like all other human activities. So the spokespersons for STS deal not with the arguments of the opposition but with its motives. (Gross, 1997)

(Note here the surprising slide from "cultural studies" to "STS," a move that we will return to later. Although the Sokal hoax was making particular fun of the way in which postmodernists were treating science and had nothing to say about social studies of science, his hoax was generally seen as proving a more general point in the Science Wars.)

Whatever Sokal did or did not intend, the deed was done. After this, the Sokal hoax took on a life of its own, while its perpetrator was off to new pastures. After an extensive academic lecture circuit Sokal began working on a critique of the way French postmodernists (mis)use physics together with the Belgian physicist Jean Bricmont, resulting in the book *Impostures Intellectuelles* (Sokal & Bricmont, 1997).[18] Meanwhile, the debate about the meaning of the hoax continued. What Sokal did not know was that his hoax would later become the subject of an article by Steven Weinberg in the *New York Review of Books* (Weinberg, 1996a), which in turn would create its own reaction.

WHO OWNS THE HISTORY OF SCIENCE? THE AFTERMATH OF THE SOKAL HOAX

Weinberg's article was ostensibly devoted to bringing out the really funny parts of Sokal's hoax, lest the layman, ignorant of modern physics, would miss out on the joke. In his article, however, Weinberg did not only explicate some fine points of physics but also clearly asserted the difference between what he called the "inner logic" of science and (what he took to be) the social constructivist position. This article drew several responses. Two Yale professors, together teaching a course on literature and science—one from the viewpoint of comparative literature, the other from the perspective of science—accused Weinberg of assuming a mantle of purity, while boiling down "science" to the work done by particle physicists (Holquist & Shulman, 1996). They objected that outside Weinberg's "reductionist temple" would be found not only sociologists, historians, philosophers and postmodern theorists, but also many famous physicists and whole scientific fields. They noted that Weinberg's "obsessive dualism," where the timeless laws of science were posited against the social world of culture, reminded them of the dualism separating the profane from the sacred.

In the same issue the director of the Center for the Critical Analysis of Contemporary Culture at Rutgers, George Levine, protested against Weinberg's contention that the conclusions of physics could have no cultural implications. He called this "an extraordinary, a profoundly irrational claim" and retorted that it was hard for laypeople *not* to draw cultural inferences from, say, Bohr's complementarity principle. The gist of Levine's critique was that the counterattackers of postmodernism and science studies had themselves become irrational and unscientific in the very name of science (Levine, 1996).

A similar thought was echoed by the Princeton historian of science, Norton Wise (Wise, 1996). Wise accused Weinberg of presenting "an ideology of science, an ideology which radically separates science from culture, scientists from 'others' and splits the personalities of scientists into rational and irrational components." According to Wise, the history of physics, particularly quantum mechanics, was full of scientists who had been motivated by philosophical, political, and other beliefs. He ended by asking whether Weinberg was trying to promote a cultural agenda of his own, attempting to rewrite history.

This remark could have cost Wise the position of special Research Professor in Science Studies at Princeton's Institute of Advanced Studies in May of 1997. At least, this is what was suggested by a report in the *Chronicle of Higher Education* (McMillen, 1997). The article theorized that a campaign by active proscience warriors succeeded in blocking Wise's appointment, just in the same way it had done years before, when Bruno Latour had been a candidate for the same position. It further suggested that Wise's recent polemics with Weinberg might have played an important part in this matter (McMillen, 1997). The outcome was that the position of Research Professor in Science Studies was not filled at all—just as had happened in the Latour case.

Pursuing this line, we find that Wise had in fact a longer record of "offenses," such as a sharp and much noted review of *Higher Superstition* in *Isis* (Wise, 1996). And he was, of course, the Chair of the Department of History of Science at Princeton, in turn directly associated with Gerald Geison's controversial *The Private Science of Louis Pasteur* (Geison, 1995), a book which in 1996 produced a sideshow to the Science Wars in the pages of the *New York Review of Books*. There molecular biologist Max Perutz criticized the book severely as "bad" history of science, suggesting among other things that Geison made too much of Pasteur's supposedly unethical behavior because he did not know chemistry (Perutz, 1996). This critique in turn led to a heated interchange between Perutz and supporters of Geison (Summers, 1997; Perutz, 1997).

Meanwhile, Weinberg himself in his response to his critics (Weinberg, 1996b) made the whole situation largely sound like a misunderstanding. He declared that he had no quarrel with most historians, philosophers, and sociologists of science, but was, rather, concerned with "the corruption of history and sociology by postmodern and constructivist ideologies." He said he accepted the idea that people could be *inspired* by scientific metaphors, but not the idea that science had any clear cultural *implications*. He also pointed out that he was not speaking for science in general, only for physics. In an important passage, however, Weinberg now also pinpointed what he himself believed to be the fundamental difference between himself and Wise and other letter writers. The latter's "agenda," according to Weinberg, was "to emphasize the connections between scientific discoveries and their cultural context." Weinberg agreed that scientists might well draw inspiration from cultural influences, but cultural influences later got sifted out. They did not become a permanent part of scientific theories:

> Whatever cultural influences went into the discovery of Maxwell's equations and other laws of nature have been refined away, like slag from ore. Maxwell's equations are now understood in the same way by everyone with a valid comprehension of electricity and magnetism. The cultural backgrounds of the scientists who discovered such theories have thus become irrelevant to the lessons that we should draw from the theories. (Weinberg, 1996b)

The issue for Weinberg, then, was "not the belief in objective reality itself, but *the belief in the reality of the laws of nature*" (italics added). For Weinberg, that specifically meant the lack of "multiplicity," that is, the existence of different laws for different cultures. At the same time, he saw the belief in multiplicity as a logical consequence of a cultural contextualist stance.

The polemics around Weinberg's Sokal article and its aftermath nicely clarified many of the issues that divided the two camps in the Science Wars. We see that we are not only dealing with an opposition between a constructivist/relativist and a realist outlook on science and the world. Weinberg's statements could be interpreted so that he (together with some other hard-liners) would wish to move in on the very territory of the humanists! *There seemed to be an open struggle between the two camps as to who "owned" the history of science!* And the fact that opposing Wise's Princeton appointment were not only scientists but also historians and historians of science (McMillen, 1997) suggested that the dividing lines went deep indeed in the Science Wars, also in regard to the history of science. (There was later a countermove by historian of physics Silvan