Introduction

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When I submitted the idea for this volume to SUNY Press, the title I proposed was *Beyond the Science Wars: Science, Technology, and Democracy.* The series editors suggested that the first part of the title be dropped. I assume that they were thinking of the shelf life of the book. In the years to come—whether it be two or ten years down the road—if the history of the so-called science wars and even the term itself are lost to collective memory, the contributions in this collection will still be valuable, and it would be a shame if the mere title led potential readers to overlook the book. Nevertheless, the timing of this collection's genesis is not coincidental. It should be understood in relationship to that often disturbingly hyperbolic dispute between a small group of scientists and so-called critics of science—a wide range of activists and scholars in the social sciences and humanities. It was the "science wars" that prompted me to put this book together, and I believe it is the ongoing reverberations of this discursive sparring and the related social conditions in which we find ourselves that make the contributions to it particularly vital now. Thus, I provide my assessment of the science wars as a frame for this volume.

Commentators agree that the origins of the "science wars" can be found in the 1994 publication of Paul Gross and Norman Levitt's *Higher Superstition: The Academic Left and Its Quarrels with Science.* Lauded by many scientist-reviewers and sharply criticized by a large number of scholars in the humanities and social sciences, Gross, a life scientist, and Levitt, a mathematician, spend nearly 300 pages in an admittedly polemical mode (1994, p. 14) examining what they view as the "proliferation of distortions and exaggerations about science" (1994, p. 7) found in a wide array of work in the social sciences and humanities. Gross and Levitt's stated fear is that this work threatens to

"poison" the contemporary university (1994, p. 7), and they suggest that the attitudes found in it exist beyond the academy, in the environmental movement in particular (1994, p. 12).

Following on the successes of their book, Gross and Levitt and others organized a conference in the summer of 1995 entitled "The Flight from Science and Reason." Sponsored by the New York Academy of Sciences, the targets of conference participants were, if the edited collection derived from the gathering is any indication (Gross, Levitt, & Lewis, 1996), much broader than *Higher Superstition.* Sociologists of science and environmentalists were verbally tarred, but so were advocates of alternative medicine, health maintenance organizations, literary critics who challenge canonical thinking, and scholars who study gender and education. The supposed crime of which all of these people were guilty is nothing less than having lost their sense—taken a "flight from science and reason."

There was a flurry of activity in the months that followed—articles in periodicals, discussions on the Internet, and talk-show banter. But still, the "science wars" captured the imagination of only a limited public. Then on May 18, 1996—what must have been an extraordinarily slow news day—just below the fold in the *New York Times* a headline ran "Postmodern Gravity Deconstructed, Slyly" (Scott, 1996) and below the headline was a representation of the cultural studies journal, *Social Text,* until then relatively obscure beyond limited pockets of the academy. The *Times* told the story of a "hoax" perpetrated by a New York University physicist, Alan Sokal. He had written a gibberish blend of "postmodern philosophy" and physics theory, submitted it to *Social Text,* and the journal had accepted it for publication (Sokal 1996a). Sokal claimed that his "experiment" revealed the bankruptcy of much recent work by humanists and social scientists who study science. Importantly, he claimed these scholars do not understand the science on which they draw or which they analyze (Sokal, 1996b; Sokal & Bricmont, 1998).[1]

In the days and weeks that followed, analyses and responses to the so-called Sokal Affair found their way into such general circulation periodicals as the *New York Times* (Fish, 1996) and the *Village Voice* (Willis, 1996). Talk show hosts repeated Sokal's assessment, and noncognoscenti audiences were left to think that the university is filled with ignorant and dangerous scholars.[2]

The science wars have not faded with the Sokal affair. The year 1997 saw publication of a 42-essay volume, which originated in the "flight from science and reason" conference (Gross, Levitt, & Lewis, 1997). And Sokal has managed to remain in the spotlight with the 1997 publication in France of his co-authored book, *Intellectual Impostures* and its release and publicity tour in England in the summer of 1998.[3] In this book, Sokal and his coauthor Jean Bricmont attempt to "draw attention to a relatively little-known aspect [of

postmodernism], namely the repeated abuse of concepts and terminology coming from mathematics and physics" (Sokal & Bricmont, 1998, p. 4).[4]

But if the academic field from which much of the work under attack comes—science studies—truly threatens to bring disrepute on the university and to spill over into the broader public arena, misleading citizens and poisoning public opinions about the scientific enterprise, why were the science wars just initiated in the mid-1990s? Significantly, the pathbreaking work that asked analysts of science to examine their views about how the practice of science really operates—studies that explored knowledge as a social construction—was published in the mid-1970s and early 1980s (cf. Barnes, 1974; Bloor, 1976; Latour & Woolgar, 1979; Knorr-Cetina, 1981). Furthermore, as Dorothy Nelkin has noted, science and scientists have been the butt of criticisms before, but their collective response lacked the current intensity. Scientists were unable to organize collectively in the 1970s to turn away efforts to promote "scientific creationism." In the 1980s, scientists generally left it to their directly affected colleagues to challenge animal rights activists' opposition to animal experimentation, and there was no high profile mobilization by scientists to support ongoing fetal research (Nelkin, 1996a, p. 94).

The intensity of reaction to science studies scholars and others who do not unquestioningly accept the authority of science is explained, I believe, by the decisive changes in the world in which we live. In the years from World War II through 1950 a pitched battle was fought in the United States over how research policy would be made in the period after the War. On one side was a group of populists led by a New Deal Senator from West Virginia, Harley Kilgore. Kilgore and his allies argued for a central agency that would be involved in coordinating the government's involvement in science and would include representatives from a wide array of social interests in the organization's decision making. Science elites led by Vannevar Bush argued for a more narrowly defined agency, an organization controlled by scientists and focusing primarily on support of so-called basic research. In exchange for autonomy and control over resources to support scientific research, the scientists promised science-generated progressive improvements in national social and economic well-being. In the event, Bush and his allies won the day (Kleinman, 1995a; Kleinman & Solovey, 1995). And until relatively recently that settlement—often called a social contract with science—has fundamentally defined the scientific community in the United States and the relationship between science and the citizenry. Throughout the Cold War, massive amounts of money flowed into university laboratories supporting research and training efforts that helped promote a picture of vibrant democracy in contrast to the totalitarian world of our Soviet adversaries. Decisions about the utilization of these resources were largely in the hands of certified

scientists, and more generally, "technical matters" were considered the realm of certified experts alone.

But the Cold War is over. There is no Soviet Union, and while the fiscal crisis in the United States appears to be over, politicians across the spectrum seem committed to leaner federal budgets. And with the nation facing more forceful economic competition from countries around the world than might have been foreseen in the heady days after World War II, new federal priorities are under debate. Under these conditions, the postwar arrangements for science are no longer stable and the future direction of federal funding for science is uncertain (Kleinman, 1995b; Nelkin, 1996a, 1996b).[5] In the view of some, even recent modest budget increases for federal science agencies are not sufficient to support an increasingly capital intensive enterprise. In this context, large projects, like the Superconducting Supercollider, are found on the chopping block. Hoping to avoid having their priorities set by politicians, a few years back, some scientist groups engaged in their own priority setting exercises (Kleinman, 1995a, p. 191). And a distressed head of the American Association for the Advancement of Science found it necessary in 1991 to publish a study reporting the results of an informal survey he undertook. Leon Lederman found his colleagues' morale universally low and concluded that the source of discontent was the inadequate support of academic scientific research (Lederman, 1991).

As sufficient funding from the federal government for university science becomes increasingly questionable, scholars turn to industry for support, and as one analyst suggests, "In a climate of intense competition for patents and research funds, incidents of fraud, falsification of scientific evidence, and misconduct have proliferated" (Nelkin, 1996b, p. A52). Whether such incidents have, indeed, increased in number over the past decade or two is an open question, but it is clear that when such cases make it into the spotlight (cf. Kevles, 1998) they lead to greater regulation of the scientific community, a kind of intervention with which scientists are neither comfortable nor familiar.[6]

Changes have not only occurred at the policy-level, but in society at large as well. Not only did the scientific community make promises after World War II, but it kept them. Scientists delivered weapons systems that kept us safe during the Cold War, produced "Better Living Through Chemistry," and found cures for devastating diseases. In a rapidly growing economy in an optimistic age, scientists were considered crucial to the widespread realization of the American Dream.

But the environment slowly changed. Many people realized that although there were numerous successes, the scientific products of the postwar decades were not uniformly beneficent—a realization deepened by such high-profile disasters as those at Love Canal and Three Mile Island. A range of local and

national political movements challenged the right of scientists to work without public scrutiny. Not only people on the political margins, but mainstream Americans criticized potential environmental and health hazards from toxic-waste dumps to nuclear power plants, for example, and threats posed to the family farm by agricultural technologies that raise costs or otherwise encourage the consolidation of holdings.[7]

It is this environment that I believe explains the timing and perhaps the form of the science wars. Indeed, some members of the scientific community appear to assume a transparent link between what they view as "flights from science and reason" and the changing science policy environment. In his 1998 candidate statement for the American Association for the Advancement of Science board of directors, Lewis Branscomb says:

> There is a movement to deny the rationality of public decisions based on science by claiming that all science is "socially constructed." Congress now demands that research agencies quantify the beneficial outcomes from their research investments. What can be done? (Branscomb, 1998, p. 4)

In the first sentence here, Branscomb dismisses by caricature recent work in science studies. His second sentence bemoans the breakdown of the postwar social contract for science, which means increased government oversight. He does not explicitly link the two, apparently assuming the relationship is obvious.

Although at some level and in some cases, the concerns of Gross, Levitt, Sokal, and others may be legitimate, there can be little doubt that these disputes constitute an effort to reinforce a crumbling boundary: a wall that divided scientists and lay citizens, a barrier that legitimated scientists' autonomy on expert matters and dictated citizen silence.[8] In this context, surveys that report overwhelming public support for science and scientists may be of little comfort to those who seek to maintain a hard and fast boundary between expert and layperson. For these same surveys suggest that by traditional measures substantial portions of the U.S. populous are "scientifically illiterate" (Lawler, 1996; see also Freudenburg, 1996). Traditional science literacy may prompt a fascination with milestones in research, but also a recognition that only scientists are equipped to deal with the complexities of the "natural world" (Kleinman & Kloppenburg, 1991). By contrast, if this "illiterate" public turns to scholars and others whose work points to the fundamentally social character of science, work that may blur the boundary between expert and citizen, for understanding, they may be led to believe that they are capable of playing a role, or entitled to intervene, in the realm of science. Scientists used to the traditional rigid division between scientists and other citizens are likely to find this prospect worrying.

Indeed, as to the question of why waste energy attacking a collection of nerds who nobody listens to, Norman Levitt is quite clear: "The catchphrases that rebound in the lecture hall one year tend, for good or ill, to be heard at rallies the next" (1996, p. 30). If scholars have the nerve to challenge the authority of scientists, it will not be long, this reasoning suggests, before average citizens feel emboldened to do precisely the same. The flip side of this logic is equally true. In their focus on academic researchers, Gross, Levitt, and Sokal have asserted that humanists and social scientists should not criticize that which they do not understand. As Sokal and Bricmont put it: "The sensible conclusion . . . is that sociologists of science ought not to study scientific controversies on which they lack the competence to make an independent assessment of the facts . . ." (1998, p. 90).[9] It is only a short step from that claim to the contention that nonexperts have no right to intervene in the realm of science. Indeed, Gross and Levitt make that argument quite explicitly in an article they published in *The Chronicle of Higher Education.* According to Gross and Levitt, "Scientific decisions cannot be submitted to a plebiscite; the idea is absurd. Applied to science education, for example, letting people vote on what should be taught would give us countless schools in which 'creation science' would replace evolutionary biology" (1994b, p. B2). The image rendered here is of a mass uninformed public voting on matters in the absence of information or a commitment to understand the issues at stake.

But in the vast literature on science, technology, and democracy, I have found no one who advocates such an absurd situation. Importantly, a recent exercise has shown that "people who don't ordinarily keep abreast of scientific issues can quickly learn about their critical aspects" (Doble & Richardson 1992, p. 52), and several cases discussed in this volume confirm this finding. At the same time, during a period in which the impacts of science and technology are felt in the daily lives of citizens throughout the world, principles of democracy dictate that we at least consider the plausibility of increasing citizen involvement in the realm of science. It is this claim—and my belief that it is time to move beyond the hyperbole that have dominated the science wars—that led me to bring together in this book the work of an illustrious group of activists, scientists, and science studies scholars. I required that contributors accept no particular definition of democracy or citizen involvement in science. Instead, the premise is that in some fashion citizens can be involved in decisions concerning science and technology.

Overview

This collection is divided fairly evenly into two relatively distinct sections. In the first, authors describe and assess real life examples of specific cases of

democratic participation in matters of science and technology. The essays in the second part of the book enter into the discussion from a different angle, pondering large questions. The contributors consider, for example, what constitutes expertise and how we can distinguish between efforts to democratize science and technology and contemplate what kinds of policies and institutions might help us move in the direction of increasing or improving citizen involvement in matters of science and technology.

In chapter 1, Steven Epstein uses the case of AIDS treatment activism in the United States as a tool to explore the conditions under which it is possible for grassroots activists to challenge the traditional social organization of expertise. Although activists were able to infiltrate the biomedical community and ultimately captured "seats at the table," playing important roles in, for example, developing clinical trial protocols, their story does not provide a simple road map for other activists interested in gaining access to research decision-making. Epstein clearly illustrates that it was the particular demographic profile of AIDS treatments activists (they were primarily white, male, and highly educated) as well as the movement infrastructure developed by earlier gay activists that provided the foundation for treatment activists' entrée into the world of biomedical science. Epstein speculates that lower-status social groups will have less luck in their efforts to gain voice in matters of biomedicine. In addition, Epstein points to the kind of unintended consequences that can befall movements seeking to influence biomedical research- and practice-related decision making. On the one hand, for example, such movements are likely to reproduce in their own organizations the expert-lay divide that they seek to undermine in the larger scientific arena; and, on the other hand, Epstein suggests that knowledgeable clinical trial participants may not abide by trial rules, thus muddying study results.

In chapter 2, Richard E. Sclove explores a very different kind of citizen participation. While AIDS treatment activism has extra-institutional origins and seeks to promote intimate lay involvement in research practice, the consensus conferences Sclove describes are vehicles for citizen influence in larger matters of science and technology policy. Sclove traces the history of consensus conferences in Denmark and their spread to other European nations and most recently to the United States. He details the practical mechanics of this organizational form and provides evidence suggesting that laypeople in these environments can grapple intelligently with highly technical matters and that the reports produced by these bodies can influence business decision-making. In the last portion of his contribution, Sclove reflects on the first U.S. consensus conference—an effort in which he was intimately involved. He suggests that the success of this first-of-its-kind effort should make us optimistic about the potential viability and efficacy of regularizing use of this decision-making model.

The existing writings of Epstein and Sclove already constitute important sources for readers interested in issues of science, technology, and democracy. In chapters 3 and 4, we hear from new voices. Neva Hassanein's contribution—chapter 3—adds a site not often discussed in debates about knowledge and democracy. Drawing on data collected over two years in Wisconsin, Hassanein discusses and analyses two farmer-to-farmer networks, which in their distinct ways illustrate the importance of "local knowledge" and of sharing that knowledge. Hassanein begins by illustrating the ways in which the social organization of power has shaped agricultural science and limited farmer input into agricultural science agendas and research. She goes on to explore the two networks she studied: one that works to develop and circulate knowledge of relevance to a sustainable agricultural practice called rotational grazing, and the other a network of women farmers interested in sustainable agriculture who work to democratize knowledge about social relations in agriculture. Hassanein suggests that the successes of the networks she describes constitute an important challenge to "the inequitable power relations characteristic of the dominant system of agricultural knowledge production and distribution."

In the final chapter in the book's first section, Louise Kaplan offers a history of citizen involvement in decision making on issues related to nuclear power, nuclear weapons, and nuclear waste disposal. Kaplan traces the "slow motion" transformation of the citizenry near the Hanford nuclear facility in Washington state from passive bystanders who accepted the professional qualifications and consequent assurances of experts concerning the safety of the Hanford site to informed and outraged participants who illustrated their competence in discussions about regulation of the Hanford facility. Kaplan's study points to the power of the idea of scientists as neutral experts and the reality that the perspectives of experts reflect a distinctive set of biases. In addition, like other chapters in the section, Kaplan's essay challenges naysayers who assert that laypeople cannot intelligently grapple with highly technical matters.

Daniel Sarewitz's contribution leads off the second section of this volume. Sarewitz explores why an Enlightenment view of science lies in tension with the ultimately uncontrollable character of nature and the similar lack of predictability and controllability that gives democracy its vitality. Sarewitz begins by describing the pervasive impact of the Cold War on science practice and policy in the United States and then explores what he calls the "Enlightenment Program." The core of the essay is an outline of eight diverse science-related problems that we currently face. Sarewitz suggests these are intimately linked to science practices shaped by the particular manifestation of the Enlightenment view that guided science in the United States during the Cold War. He concludes by arguing that "the organizational structure

and knowledge products of today's [scientific] enterprise are often not suited to addressing . . . [these problems] productively" and calls for a new approach to federal science policy and practice better suited than the current system to promoting human needs and well-being.

In chapter 6, Stephen Schneider does not question the view, supported by the contributions in part I, that laypeople are capable of understanding complex scientific arguments and the character of debates between scientists. He does suggest, however, that such a level of understanding demands a kind of commitment that few lay citizens are likely to make. Consequently, Schneider proposes the creation of a "meta-institution" that will provide citizens and their elected representatives with assistance in evaluating scientific credibility. Citizens would witness the proceedings of this organization, but membership on the body would be dominated by persons nominated by scientific societies. Without such a body, Schneider fears citizens put off by "baffling technical brouhaha" are likely to abdicate to experts their role in the "value-laden" policy selection process. It is worth noting that Schneider assumes something questioned by several other contributors to this volume: that it is possible to separate the value and factual components of technical matters. Schneider sees a role for lay citizens in the value aspects of technical matters. By contrast, in differing ways, the other contributors to the collection point to the inseparability of fact and value and suggest a role for lay citizens in what Schneider might take to be the technical/factual core of science.

Like Daniel Sarewitz, in her chapter, Sandra Harding asks us not to limit ourselves to an Enlightenment model of science. Harding's critique focuses not on the issue of control, but instead on the problematic claim that social and political neutrality can and should characterize sciences' internal, cognitive, technical core. She suggests that we need to be attentive to "how social and political fears and desires get encoded in that purportedly purely technical, cognitive core of scientific projects." In particular, Harding examines the universality ideal embedded in much contemporary science and suggests that this underlying principle leads to the devaluation of cognitive diversity, legitimates the acceptance of less well-supported claims over stronger ones in some instances, and creates blindness to some of the most cogent criticisms of particular scientific contentions. Still, Harding does not want to abandon the universality ideal entirely, but instead wishes to retain its valuable components and reconceptualize others in a way that will permit us to realize important democratic ideals.

In the final chapter of book, I contribute to the debate on citizen involvement in the realm of technoscience. I suggest that discussions of democratic involvement in science and technology are often marred by lack of clarity and consequent misunderstanding. In an effort to bring lucidity to these exchanges, I outline several dimensions across which it is possible to distinguish instances

of democratized science. Drawing on cases considered in this volume and elsewhere, I contend that laypeople can grasp the subtle content, difficult concepts, and methodological complexity of science, and consequently, I suggest that this is not a valid basis for a priori rejection of efforts to democratize science. I suggest that the real obstacles to the democratization of science are rooted in widespread social and economic inequalities and an unexamined commitment to expert authority. I conclude by providing some rudimentary proposals for overcoming these hurdles.

At the Starting Line

After Alan Sokal's hoax was made public, discussion of what is at stake in the science wars—some of it quite useful—proceeded faster and more furiously than it had until that point. In venues from the Internet to letters to the editor, discussants—from cultural studies celebrities to unknown graduate students—expressed their views. In his letter to the editors of *Lingua Franca,* Rutgers University's George Levine made a comment that might usefully serve as a guiding premise for this collection. "The key," suggested Levine, ". . . is that the public should have a responsible *and* intelligent relationship to science" (1996, p. 64). I hope this volume contributes to such a relationship. As such, it should constitute not the last word on the science wars or on the relationship between citizenship and science and technology, but rather, I hope the chapters that follow will prompt productive dialogue.

Notes

1. See the series of responses to Sokal in *Lingua Franca,* July/August, 1996.
2. For a perceptive analysis of Sokal's "experiment" and why it does not show what he claims, see Hilgartner (1997).
3. I was surprised to see *Intellectual Impostures* among the "best sellers" when I happened into a Waterstones bookstore in Cambridge, England in January 2000. The science wars continue.
4. It is my view that the written work by the science warriors does not provide a fair or damning critique of science studies. See Kleinman (1995b) and Kleinman (1999). See also Lewenstein (1996) and Guston (1995).
5. Readers wishing to follow the recent history of federal budget battles as they relate to science and technology should turn to back issues of *Science* magazine.
6. Congressional involvement in discussions about (mis)conduct in science can be traced in *Science* magazine.
7. This paragraph and part of the previous paragraph are taken from Kleinman (1995b).

8. In the epilogue to his recent book, Thomas Gieryn (1999) points out that it is not only scientists in the science wars who are engaged in boundary construction, defense, and expansion. Many in science studies are involved in the same exercise.

9. The question is who decides what counts as competence. Levitt, Gross, Sokal, and Bricmont would presumably assert that only certified scientists are qualified to assess the competence of nonscientists. If this is the case, any criticism of science can only occur on scientists' terms.

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