

# Preface

This book is a history, analysis, and criticism of what the author calls “postmodern interpretations of science” (PIS)<sup>1</sup> and the closely related “sociology of scientific knowledge” (SSK). This movement traces its origin to Thomas Kuhn’s revolutionary work *The Structure of Scientific Revolutions* (1962), but is more extreme. It is characterized by a belief that science is a socially constructed, “situated,” historical product whose theories are generated by contextual factors such as class interest, ideology, or laboratory politics rather than nature. Moreover, instead of being a glorious and progressive achievement of the Western world since the seventeenth century it has been a deeply flawed enterprise which has degraded the environment, oppressed women, minorities, the Third World, and is presently a tool of corporate capitalism and the military industrial complex. These views are often joined to a profound anti-realism. Since nature cannot be interpreted independently of the conceptual structures scientists bring to the task, there is *no* “way the world is” apart from these structures. Scientific theories are not “caused” by nature. Instead, they *constitute* nature. So-called “scientific facts” do not correspond to mind-independent properties of the universe, but merely represent the biases of scientists. And since these biases inevitably reflect class, gender, race, or other sociopolitical factors so does what we call “nature.” It follows that vaunted “objectivity” of science is an illusion. Science is an ideology like any other. At best it may have some pragmatic or technological value, but there is no epistemological reason for it to be preferred over “other ways of knowing” such as religion and myth.

Such views, which radically conflict with the positive vision of science dominant in the West from the seventeenth century at least through the

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<sup>1</sup>We use the same abbreviation in either the singular or plural case.

1950s, have become quite fashionable in the humanities since the 1970s. They are now particularly to be found among faculty in English, Cultural, Women's, and Post-Colonial Studies, as well as in the relatively new discipline of Science, Technology and Society (STS). Not even the History of Science has been immune. For example, Arnold Thackray who in the 1970s became editor of *Isis*, the preeminent American journal in the history of science,<sup>2</sup> thought that the central questions to be decided in the history of science were:

Can logical and historical distinctions between science and magic be sustained? Is science culture free in any but a trivial sense? Is progress or truth a useful concept around which to organize historical work? . . . Is science essentially an ideology or form of oppression?<sup>3</sup>

Examination of several years worth of issues of *Isis* and other journals in the 1970s and 1980s supports the view that such questions (with negative answers to all but the last) were popular areas of investigation.

SSK/PIS has been attacked by Paul Gross and Norman Levitt,<sup>4</sup> Noretta Koertge,<sup>5</sup> Alan Sokal and Jean Bricmont,<sup>6</sup> and others. The resulting conflicts comprised the so-called "Science Wars of the 1990s". These authors view the entire movement as an intellectual freak show and concentrate on demolishing it. They emphasize "extremists," especially feminists, Afrocentrists, deep ecologists, and radical social constructivists, or in the case of Sokal and Bricmont the more exotic and unintelligible representatives of French post-structuralism (such as Derrida, Foucault, and Lacan). While the author basically agrees with this point of view, this book is intended to be more than another broadside in the now nearly exhausted topic of the Science Wars. The authors cited above have done their job well, and it is not necessary to repeat it. Instead, we will try to understand the emergence of a family of attitudes towards science which represents as great (though hopefully not as potent) an intellectual change as did the Enlightenment. What are its intellectual origins? What were its causes? Was it the product of some social or political transformation peculiar to the United States or

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<sup>2</sup>Equivalent in prestige, say, to the *Transactions of the American Mathematical Society* in Mathematics.

<sup>3</sup>Quoted in Novick (1988), p. 537.

<sup>4</sup>*Higher Superstition* (1994)

<sup>5</sup>*A House Built on Sand* (1998)

<sup>6</sup>*Intellectual Impostures* (1998)

Europe in the 1960s? Is SSK/PIS an early warning of possible political consequences in the same sense as the writings of the *philosophes* of eighteenth century France were?

To properly deal with such questions we would ideally require something equivalent to E. J. Dijsterhuis' magisterial *Mechanization of the World Picture: Pythagoras to Newton* (1961), but devoted to SSK/PIS instead of to the development of the mechanical philosophy in the sixteenth and seventeenth centuries. This is beyond our power; perhaps also the ignobility of the subject is unworthy of such effort. Instead, we will mainly concentrate on offering a series of "snapshots" of people whose doctrines illustrate what we think are the main intellectual stages in the progression to full-blown SSK/PIS—roughly in the period 1925 to 1975. To this end, we will look at the thought of Karl Popper, Karl Mannheim, Ludwik Fleck, Thomas Kuhn, Paul Feyerabend, David Bloor, Steve Shapin, Harry Collins, Bruno Latour, and several others, as well as PIS-like doctrines in mathematics. We will also describe various philosophical contributions to PIS ranging from the Greek sophists to 20th century post-structuralists. Our focus on the most important actors, however, means that detailed consideration of several thinkers who are at least "fellow travelers" of the movement will be omitted. We will have only a few remarks, therefore, concerning French poststructuralism, the American philosophers Richard Rorty and Willard Quine, and the wider phenomenon of postmodernism as it has affected other areas of culture.<sup>7</sup>

But PIS is not only an event in late twentieth century intellectual history, it is also deeply political. Therefore, an equally important goal will be to understand the social and political environment that allowed it to flourish. Ironically, our method here will be similar to that which SSK uses to deconstruct science. To use the language of Karl Mannheim we wish to "unmask" PIS and reveal the hidden "extra-theoretic" goals and needs it serves. Specifically, we will argue that the disturbed political atmosphere of the Vietnam war era was a critical catalyst in the rise of PIS. This is hardly a novel thesis, but the documentation for it is ample and worth presenting.

The motivation for writing the book is autobiographical. From 1958 until 1963 I had been in fairly close contact as a Berkeley history student with Thomas Kuhn.<sup>8</sup> My interests, however, changed from history to mathematics in 1967, and I have had a conventional, reasonably successful research

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<sup>7</sup>But see Chapter 13.

<sup>8</sup>My generation of graduate students included Paul Forman, J. L. Heilbron, and Karl Huffbauer, who all went on to distinguished careers.

oriented career in this discipline until my retirement from the University of Alabama in 2002. In 1996 the Sokal hoax and the Gross and Levitt book came to my attention, and in the summer of that year I was saddened to learn of Kuhn's death and impressed by lengthly obituaries he received. These events made me aware of the Science Wars. As was the case for most scientists, they had been completely invisible to me as my intellectual interests were mostly limited to my own narrow technical research. Despite my background, I no longer paid any attention to what was going on in the humanities. Therefore it was a shock to learn of the profound and negative change—representing a kind of counter Enlightenment—in the vision of science on the part of many humanistic intellectuals which had developed since my days as a graduate student in history over 30 years earlier. This stimulated my curiosity and made me want to find out what exactly had happened and why.

The effort to write the book has been immense. It required catching up in a year or two on forty years of unread books and articles—clearly a daunting task. Much of it I found to be junk, which to quote an apocryphal mathematical review filled “a much needed gap in the literature”—despite in some cases having been written by holders of endowed chairs at the better universities.<sup>9</sup> Few of the ideas in this literature had the difficulty of even elementary mathematical concepts found in undergraduate textbooks. Most, in fact, if not nonsense, were fairly trivial—their appearance of profundity being due to the obscurity of expression rather than to any inherent worth. But there was a lot of material to sift through! To my amazement I soon discovered that thousands of articles and hundreds of books with a PIS point of view had been written since the 1960s. In the face of this astounding mass of verbiage, I sometimes felt like a ragged urchin—whom one sometimes sees in news magazine photos—picking through an immense pile of garbage, near some third world city, but finding little of value. Yet at the same time, some of the earlier writers who seemed to have inspired PIS had articulated ideas that needed serious attention, regardless of one's opinion of their validity. There is a difference between the work of thinkers such as Mannheim, Fleck, or Kuhn and the tirades of some feminist or Afrocentric assistant professor who may cite them.

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<sup>9</sup>The quotation has often been attributed to a review of a bad mathematical paper by a famous mathematician, for example Paul Halmos. It does not however ever seem to have appeared in print. For its amusing history see Jackson (1997). Branding something “junk” does not mean that I merely disagree with it. I have read much brilliant high quality scholarship with deeply offensive theses. Rather, “junk” means that the work is unintelligent and would not have earned a B- in a Berkeley seminar c. 1960.

Despite its serious intentions, some—to use fashionable terminology—may find that my book is “transgressive.” It is written by an outsider and violates disciplinary boundaries. Its author is neither a sociologist, philosopher, or historian of science and has had only a slight exposure to these disciplines in the 1960s. This means that there will be errors. The author has no doubt that important sources have been missed and others misinterpreted. These shortcomings as well as his general point of view may offend specialists. For such errors and naiveté, the author apologizes and takes full responsibility. Yet, he would be prepared to defend the overall truth of the main theses of the book, and consoles himself with the fact that his errors may not be as bad as those made by postmodernists when they discourse on such things as nonlinearity, Gödel’s theorems, Relativity, Chaos Theory, Quantum Mechanics or Topology. These forays into the sciences and mathematics are almost always at least technically wrong and often pure nonsense.<sup>10</sup> More generally, those who attempt to create a “science of science” and who argue that science is “socially constructed” and should not be “epistemologically privileged” over other cultural practices such as literary criticism or shamanism generally commit worse errors than any the author has in his book. I have never ceased to be amazed that those without *any* hands on experience of scientific or mathematical research—or even any concrete knowledge of the details of a technical field can write with absolute self-confidence and authority on the nature of science or mathematics.<sup>11</sup> Such chutzpah exceeds anything in this book!

To sketch the organization of the book: we begin with a semi-autobiographical chapter which explains in greater detail how the book came to be written. This chapter also summarizes the theses of SSK/PIS in the form of fifteen propositions to which we will often refer. Chapter 2 discusses the positive image of science in the first two decades of the Cold War as well as logical positivism, the philosophy supporting that image. We

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<sup>10</sup>This has been convincingly demonstrated to the satisfaction of anyone who has any understanding of these subjects in the book in *Higher Superstition*. See also Sokal and Bricmont’s *Intellectual Impostures*. Especially enjoyable examples of technical incompetence may be found in Bruno Latour’s treatment of Relativity (Latour (1988b)) or in N. Katherine Hayes’ book *Chaos Bound* (1990).

<sup>11</sup>A historian of science once justified this practice to me by claiming that “it is not necessary to be old to be specialist in geriatrics.” I leave the reader to decide if this analogy is apt. There are some honorable exceptions, however. Kuhn himself was a well trained physicist as was his student J. L. Heilbron. In fairness we should also admit Steve Shapin and David Bloor with whose ideas the writer strongly disagrees have also had scientific training. Yet in comparison with the vast army of SSK/PIS advocates they are exceptions that prove the rule.

also describe the heady post-Sputnik scientific climate of the 1960s and its abrupt decline after 1970. Chapter 3 is a survey of some of the philosophical resources exploited by PIS together with the intellectual changes in the early twentieth century that may have prepared the way for it. Chapter 4 focuses on Karl Popper who has been accused of being the hidden father of PIS. We disagree and see him only as a relatively indirect harbinger of the changes to come. Chapter 5 shifts emphasis away from both philosophy and the philosophy of science to sociology where we think the main inspiration for PIS lies and centers on Karl Mannheim whose sociology of knowledge (SK) originated many key concepts which SSK was able to apply to science. There follow chapters on Ludwik Fleck, Thomas Kuhn, and Paul Feyerabend. We see these individuals, together with Mannheim, as constituting the most direct route leading to PIS. Only in Chapter 9 do we attempt to unravel the tangled nest of ideas that characterizes SSK and PIS. We concentrate on Michael Mulkay, Steve Woolgar, Bruno Latour, Harry Collins, and particularly on the creators of the so-called “Edinburgh Strong Program,” principally David Bloor and Barry Barnes. Attention is also given to Steve Shapin and Simon Schaffer in the form of an analysis of their provocative book, published in 1985, *Leviathan and the Airpump*.<sup>12</sup> We then give several examples to show how standard PIS attitudes have negatively affected the images of “great scientists,” especially Newton and Pasteur. In Chapter 10 we look at the application of PIS to mathematics. This focuses on the work of David Bloor, Sal Restivo, Imre Lakatos, Paul Ernest, Reuben Hersh, and the pronouncements of Ludwig Wittgenstein on the nature of mathematics. Through Chapter 10 we have tried to present the ideas involved in PIS or SSK as straightforwardly and fairly as possible. Although our unfavorable opinion of many of them is probably evident, serious criticism is reserved for the last three chapters. Since PIS is also linked to perennial philosophical issues, we look in detail at some of these in Chapters 11–14. Our attitude is one of semi-agnosticism. We agree with Colin McGinn, that no serious philosophical problem can be “solved.” This means that the anti-realism SSK and PIS generally invoke can neither be refuted or proven, as indeed is the case with scientific realism. But although this is so, we personally feel that the consequences of the philosophy behind PIS are more confused and unbelievable than those of scientific realism. More fundamentally, we argue that anti-realism considered as a philosophical doctrine will not support the weight PIS puts upon

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<sup>12</sup>Recently awarded the Erasmus Prize by the Dutch Government.

it; it is perfectly compatible with a view, e.g., in its Machian or Copenhagen interpretation form, that scientific theories are tools serving to organize experimental data, “save the appearances,” and make predictions. Further, we think that the characteristic PIS theses that observations are completely “theory-laden,” that carefully done experiments cannot falsify a scientific theory, that science consequently is the product of “political” negotiation, that nature is a social construct, etc., etc., are simply false. Chapter 13 examines the problem of historical causation. Why has SSK/PIS achieved the influence it did when it did? Why did its ideas which had been anticipated for a long time “jell” to become a political force only after the mid-1960s? To answer this question it is necessary, as we have already remarked, to apply the sociology of knowledge to the ideas of PIS or SSK, that is, to explain the social and political functions they serve and why they have emerged. Having done this, we are licensed in true SK fashion to “unmask” or “disintegrate” them, without taking the arguments of their defenders too seriously. In Chapter 14 we review some of the points made earlier and close with a speculative (and deeply Machiavellian) analysis of the possibility of a new outbreak of the Science Wars resulting from a collision of interests between PIS and institutional science. Such an outbreak will become inevitable if, as we fear is already happening, science bureaucrats, funding agencies, and activists exploit the ideology of PIS for political ends.

After completing most of the composition of our book, we discovered that it covers some of the same ground as John H. Zammito’s magisterial *A Nice Derangement of Epistemes: Post-Positivism in the Study of Science from Quine to Latour* (2004). By “post-positivism” Zammito means roughly what we regard as the union of PIS and SSK. Zammito seems to have read virtually every primary and secondary source concerning these movements and offers a post-Quine intellectual history whose thoroughness and detail we cannot match. Yet, there are differences in point of view, emphasis, and content between our work and his. Zammito seems to regard the emergence of post-positivism as purely a matter of change within the philosophy of science. With the exception of a few perceptive remarks<sup>13</sup> he more or less omits the political aspect. Also, he sees the work of Quine as critical, while we view Quine as a secondary figure. Instead, as we have already mentioned we trace the origins of SSK/PIS to Karl Mannheim and analyze his thought in some detail. Zammito does mention Mannheim in

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<sup>13</sup>For example, Zammito (2004), p. 124.

several places,<sup>14</sup> but for him Mannheim's importance is not central. We also have chapters on Ludwik Fleck whom Zammito omits and mathematics, a subject which Zammito does not consider. Nevertheless, Zammito's book is excellent! We recommend it as a supplement or corrective to our own.

Two final remarks: the University called "Persepolis State" which will appear fairly frequently in this book is *not* the author's own University. It is intended as a Platonic archetype of a standard public university or redbrick found in the US and UK. As such, it is a distillation of several well-known institutions: for instance, Paterno University in College Stadium, Pennsylvania, Volunteer State University in Fort Sanders, Tennessee, Hogarth Polytechnic and the University of Rummidge, which are located respectively in Royal Pavilion and Rummidge, UK, and even a bit of Euporia State University's campus by the bay in Plotinus, California. Likewise, any reference to a Professor, Chairperson, Dean, Provost, or other administrator does not describe any person living or dead, with or without tenure.

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<sup>14</sup>*Ibid.*, pp. 124, 126, 138–140.