Chapter 1

Rip van Winkle Awakes

In the early summer of 1996 my wife and I were spending a few weeks in Brighton where I was visiting a colleague at the University of Sussex and writing a now nearly forgotten paper on weighted Sobolev embeddings. Each morning it was my habit to gratify the local Pakistani newsagent by purchasing the three major British newspapers, the *Guardian*, *Times*, and *Telegraph*, supplemented if I were in a raffish mood by the *Sun*. I enjoyed savoring the various ideological wars conducted in these newspapers, or to use postmodernist language the "incommensurable discourses" found in them. On a particular June morning a large photograph of Thomas Kuhn was on the front page of each newspaper with the announcement that he had passed away at age 73. Multi-column obituaries followed.

This brought back vivid three decade old memories. Between 1958 and 1963 while a student at Berkeley I had taken both an advanced undergraduate course and a graduate seminar from Kuhn. Although I was an immature somewhat uneven student, Kuhn treated me with consideration and patience. We were not close friends, but I had been to his home, briefly worked as one of his research assistants, and had frequent conversations with him in his office. On one of these occasions in 1963 the telephone rang with an offer from Princeton. Prior to the publication of *The Structure of Scientific Revolutions* in 1962 Kuhn, although respected for his earlier book *The Copernican Revolution* (1957), was a relatively ordinary member of the Berkeley faculty, holding a joint appointment as Associate Professor in the History and Philosophy Departments. He had come from Harvard in 1957 where he had failed to achieve tenure. Probably Michael Polanyi (1891–1976), Stephen Toulmin (1922–), Norwood Russell Hanson (1924-67), and Paul Feyerabend (1924–94)—who after 1958 was also in the

Berkeley Philosophy Department—were then more established than Kuhn.[1] I can recall clearly when *Structure* was published in 1962. Like many of Kuhn's students, I immediately bought a copy which I still own. I think that I understood it imperfectly, but could tell that it had a highly original thesis. I remember arguing about it with other graduate students. It is fair to say that none of us, probably not even Kuhn himself, realized that the book would become the single most influential work in the history of science in the twentieth century.[2] I left Berkeley in 1965, spent a few years of teaching history and philosophy at small institutions, and soon lost contact with Kuhn. By 1967 I had turned to the serious study of mathematics, and had almost forgotten about the history of science. The only evidence that I had ever studied the subject is a book collection in my library that is reasonably up to date as of 1964.

By 1996 I had been a mathematician for nearly 30 years and was pursuing a quite conventional and modestly successful career of teaching and research at a Southern university. The life, in spite of the endless cycle of teaching calculus to a mostly captive audience of uninterested and ill prepared students, was not a bad one. There was, after all, the occasional good student who, however briefly, made teaching seem worthwhile. Telling students about derivatives, integrals or related rate problems was less stimulating than telling them about Robespierre, the Committee of Public Safety, or the September Massacres as I had done decades earlier. On the other hand, although I took my teaching duties seriously, the very difficulty of the subject meant that the average student in my classes could directly experience something resembling the atmosphere of the Republic of Virtue and Terror without having to read about it. The grading was also easier than in the humanities. One did not have to spend endless hours reading mind rotting essays—or in the rare case that they were of some worth trying determine if they had been downloaded from the internet or purchased from a term paper mill.[3] A red pen and the political will to write a large number of zeros in the margin was all that was required. My teaching duties thus allowed me time to concentrate on my research which gave me great satisfaction. The job also entailed many travel opportunities. Because my previous interests had in part been directed to European history, I especially sought out mathematical contacts and travel opportu-

[1] Fuller (2000), p. 5.

[2] Karl Popper's *Logic of Scientific Discovery* probably ran second.

[3] Popular ones at Persepolis State include "CheatHouse.com", "School Sucks.com," or "Paperstore.net."

nities in both Eastern and Western Europe. Yet, although I enjoyed looking into the political and cultural history of the places I happened to visit,[4] I was—like most mathematicians and scientists—unconsciously living in a hermetically sealed mental world. My subject was difficult, perhaps more so for me than for younger colleagues, since I had begun graduate work in it at the relatively late age of 28. Teaching, refereeing, committee work, not to mention the occasional *coup d'état* plotted or narrowly averted in the Department, plus thousands of hours spent writing research papers consumed my time and energy. Even though I had started out in the humanities and knew a little intellectual history I was totally unaware of the current Zeitgeist of many humanistic intellectuals. From the newspapers I had seen the word "postmodernism." I knew that it was controversial, had something to do with the humanities, and in some fashion was associated with an occult, possibly disreputable, practice of English faculty known as "deconstruction." Beyond this, it was just a word; I had no more awareness of its meaning than of some obscure technical term found in a chemistry or accounting textbook. In this ignorance I was only slightly better off than most of my mathematical and scientific colleagues. I may have remembered Tom Kuhn as a person quite clearly, while only a few other members of the Department had even heard of him. But none of us were aware of Bruno Latour, the Edinburgh Strong Program, or could define "social constructivism."[5] We all certainly knew about "paradigm shift," but only because it functioned as a cliché in automobile or deodorant advertisements, or as a phrase much employed by political op-ed writers.[6]

In a medium-sized University such as mine, there may be a thousand or more faculty in the various Departments of the College of Arts and Sciences, many of them intelligent and worth knowing. The increasingly difficult academic market in the 1970s in almost all the core subjects necessarily and rapidly increased the quality of those who managed to land (and keep) tenure track jobs. Even at second-tier institutions like mine the new faculty were easily comparable to many (if not to the very best) of the faculty hired at higher status universities in the period from 1940 to the late 1960s.[7]

[4]For instance, the origins of the Iron Guard in Jassy Romania, the deadly politics of 1930s Bucharest, or Goering's authorization of Oberwolfach in 1944.

[5]This ignorance was not just a characteristic of science faculty at my university; I also found it among colleagues at Wisconsin or Pennsylvania State.

[6]As in "Bush's Iraq policy represents a paradigm shift ...." An occasional Dean or Provost was also fond of the term to describe his "reforms" or latest Five Year Plan, which in practice meant more meaningless busy work for the faculty.

[7]In the mid-1960s academic jobs were absurdly easy to get, especially in mathematics

But unless one was on some official committee there was neither the time or opportunity to meet them; in my case as I aged other departments, located on the far side of campus from the mathematics building, gradually became too distant to visit on foot, and parking on an ever more crowded campus became impossible. My declining energy, moreover, had to be restricted to research and teaching to the exclusion of socialization. For most research active faculty in the sciences or mathematics, one's friends were mainly colleagues in the same research area. Only a few at most would be at one's department or university. Instead, they might be found in such places as Kent State, Texas A&M, Lima, Prague, Manchester, Plovdiv, Sussex, Zagreb, or Novosibirsk. Mutual contact was restricted to meetings, exchange of preprints, email, and in a few cases sabbatical visits.[8]

Presumably a similar situation must have been true for people in the humanities. But they usually had not the training to understand the *technical* content of science and mathematics, and so were probably even more isolated from the science faculty than the science faculty was from them. This is not intended as a criticism of humanists. An equally profound mutual isolation is often true of scientists with respect to technical research areas other than their own, and is an unavoidable consequence of the incredible specialization and opaqueness of high level research within science. A common joke among mathematicians, for instance, is that two individuals could have exactly the same AMS-MOS research classification number and still be totally unable to understand each other's work. This phenomenon means that the reasons for attending mathematical conferences are usually quite different from their officially stated purpose. One does not participate in order to listen to or absorb the papers being presented by other mathematicians. Instead major (but unacknowledged) goals include securing an invitation as a plenary speaker (a valuable sign of academic worth to administrators), the informal exchange of ideas and gossip with old friends, hiring and networking, the consumption of interesting food and drink in exotic foreign locations,[9] and perhaps of greatest value a brief Cinderella-like escape from students, administrators, and the micropolitics of one's department.

In theory, a scientist or mathematician can, provided he can spare the

and science. As a consequence, many of the people hired then were by all objective measures weaker than those struggling to find academic jobs a generation later.

[8]The miracle of email in particular meant that one could conduct joint research or even write a paper with someone one had *never* physically met.

[9]Very valuable in this respect were the wine lockers (situated in the lecture rooms) and afternoon teas at the German mathematical institute Oberwolfach.

time, appreciate an occasional production in history or philosophy if it is written in some approximation to normal literary English. But the same is just not possible for the humanist vis à vis, say, mathematics or organic chemistry except on the most popular and non-technical level. Even so, this is a rare accomplishment for a scientist. In the first place, he hardly ever *has* the time. Secondly, many of the humanities have (aping the sciences) developed an opaque, painful to read, jargon of their own. As a consequence, the average scientist or mathematician is more on the level of the *History Book Club* or *American Heritage* magazine than the *American Historical Review*. In his spare time he might read a novel by Hardy or Conrad, but not Terry Eagleton's or Frank Lentricchia's books on literary theory. Thus it is not easy for him to be aware of important intellectual currents in the humanities; C. P. Snow's "two cultures" are evidently still alive, well, and almost totally failing to communicate with each other. Despite my own background in the humanities I, in particular, did not have the slightest idea of what was going on either in the humanities departments of my university or in the disciplines they studied.

Like almost all scientists and mathematicians, I had a simple and straightforward view of the nature of science. Any perturbation caused by reading *The Structure of Scientific Revolutions* more than thirty years earlier had long since dissipated. It was clear to me that science produced objective, reliable knowledge about a world whose properties exist independently of both the scientist and the ambient social, religious, or political atmosphere in which he or she is immersed. Neutrons are exactly the same for liberal democrats, voters for George W. Bush, Muslim fundamentalists, and North Korean communists. To be sure, on rare occasions science has been contaminated by ideology, for instance, under both communist and fascist regimes; but this has been universally acknowledged to have been fatal to its integrity. Science, furthermore, achieves its goals by reliance on "scientific method." I was aware that philosophers could and did quibble about its details as well as other issues such the ontological status of theoretically posited but unobservable entities; yet as a whole, the method was fairly obvious. Scientists, by careful observation and rational reflection, accumulate evidence and formulate theories in order both to explain known and to predict new phenomena. Often, especially in the physical sciences, this involves the construction of mathematical models which are idealizations of the concrete situation in which the essential features become

mathematical abstractions and the less important are ignored.[10] These theories and predictions are then tested by well-designed experiments. If they fail such tests they are rejected. If experiments verify them they are provisionally accepted. While no amount of experimental evidence can ever absolutely verify a theory, the better the verification, the greater in some sense is the probability that the theory is correct.[11] Especially well verified theories may be regarded as yielding "laws of nature." Furthermore, some discrete claims about nature are so well verified, such as e.g., the earth is not flat and is approximately spherical, the blood circulates, or the moon is about 60.263 earth radii from the earth that they become absolute truths or "facts." Both laws of nature and facts represent permanent features of the world which existed before human beings discovered them, just as the North American continent existed before 1492 or the earlier Viking voyages. These features are also independent of the human mind. Mt. Everest or positrons are what they are regardless of our thoughts or belief structure about them. Of possibly greater importance, however, than the exact details of scientific method is the moral *objectivity* of science. This demands that the scientist separate his politics, desires, ego, gender, social position, and all other extraneous features from his work. In particular, theories should never be judged in terms of their ideological consequences. These may indeed be present, but they have nothing to do with scientific adequacy, which can only be tested by observation and experiment. On occasion, this neutrality of the scientist concerning social or political factors will be difficult or even impossible to attain. But an honest attempt must be made; only in this way does the resulting science have any chance of universality.

[10] For example, the mathematical model of projectile motion essentially due to Galileo yields the equations

$$H(t) = (v_0 \sin\theta)t - gt^2/2$$
$$D(t) = (v_0 \cos\theta)t,$$

where H, D, θ, v_0, and g stand respectively for the height, horizontal distance traveled, angle of release (with respect to the D axis), velocity of release, and acceleration of gravity. This model ignores air resistance, the mass of the projectile, the attraction of the moon, the inverse square law of gravitational attraction, the curvature of the earth, temperature, and many other factors. Hence, while the equations are easily solved the solution is not very accurate, except for heavy dense projectiles at low velocity and close to the earth's surface. But the point is that they can be modified to obtain a model as accurate as we wish. In this case computer methods are necessary for solution.

[11] Considered logically, however, this inference is invalid. If T denotes a theory and O a predicted observation, we cannot go from $T \Rightarrow O$ to $O \Rightarrow T$, which is an elementary logical fallacy.

By such means science develops, facts accumulate, and our understanding of the natural world and its laws approaches the truth. To say this in a different way, the collective human mind becomes a more and more accurate mirror of nature. I recognized, of course, that scientific beliefs especially at the research frontier can turn out to be mistaken and change when additional evidence becomes available. Yet even if this is true, most (if not all) contemporary science *probably* consists of true and well justified beliefs. And because it at least *attempts* to faithfully reflect reality, scientific knowledge is different from the claims generated by other belief systems. The existence, say, of neutrons has a different epistemological status than the theological claim that the Holy Ghost proceeds from the Father to the Son, dialectical materialism, or the theses of American neoconservatives, Salafist Muslims, African witch-doctors, professors of cultural studies, and so forth. To express this more bluntly, science (and mathematics) represents the best and most reliable knowledge the human race can possess; it is the most rational enterprise of which we are capable. The other areas of human intellectual activity are mostly a confused (though sometimes interesting) mixture of cultural myth, ideology, and value judgement. Their existence is explained by social and historical contingency rather than rational criteria.

It follows as a result of the emphasis put on scientific inquiry in the last three hundred years that the human race in the modern world possesses far more accurate knowledge about nature than in the Middle Ages or in the ancient world. This in turn implies that one of the most obvious and uncontested features of science has been its cumulative progress. With respect to applications, science has made modern technology possible which in turn has greatly improved communication, the ability to travel quickly and (for the most part) safely, human economic possibilities, and (unfortunately) also weaponry. The application of the scientific outlook to medicine has also improved the human condition by eliminating many of the ancient scourges of mankind. With antibiotics or vaccines we can cure TB and no longer die of smallpox, diphtheria, typhus, cholera, polio, and other terrible infectious diseases.[12] Physicians no longer bleed people to death or apply hot irons to the temples of the dying Charles II; instead they perform heart or kidney transplants and greatly prolong the life of cancer patients even when the underlying condition may not be curable. In fact, a personal medical situation, requiring some very advanced (and expensive) modern

[12]Of course, much of this progress is being called into question with the spread of antibiotic resistant bacteria. The situation is especially serious for some forms of TB. Yet, ever more potent drugs are being developed.

medical technology to resolve, reinforced my hero worshipping attitude towards medical giants like Pasteur, Lister, Koch, or Cushing. I was very much in the same mental world as Paul de Kruif's *Microbe Hunters* (1926) or Arturo Castigioni's *History of Medicine* (1947). Such judgements as these seemed to me too obvious to be contested—since the Enlightenment they merely represented, so I thought, the common sense of reasonably well educated people at least in the developed world.

I also believed that human moral categories do not often apply to science. The existence of neutrons, for instance, has nothing to do with moral values. Science certainly can be used for good or evil purposes, but it has no inherent moral qualities of its own except a commitment to truth and honesty in the research process. Whether we use nuclear physics to produce bombs or CAT scans is the responsibility of political leaders or society generally, not that of scientists. It is especially not the responsibility of those whose fundamental or "pure" research lies a great distance from technological applications; thus Rutherford or Chadwick should not be blamed for nuclear weapons. Moreover, even if some applied research is morally problematic, this has nothing to do with the validity of the results obtained; these stand or fall on their own scientific merits. For example, we can disapprove of Pentagon funded research seeking to improve the stickiness of napalm, but the conclusions of such research are true or false independently of moral factors. In spite of the fact that science can be and too often has been exploited for evil ends, I continued to believe that it remains one of the intellectual glories of the modern *Western* world. No other civilization in history has come close to producing the scientific miracles that Europe has since the Scientific Revolution of the seventeenth century. This view of science was also tied into a wider set of cultural values. I valued knowledge for its own sake, and thought that in general more of it was a good thing. I believed that truth was an objective property which in many cases we could discover by comparing an assertion about something to what was actually the case; even when this is difficult or impossible to do careful and *objective* study can at least clarify the issues. In short, I was still (in the 1990s!) a child of the Enlightenment and believed in the power of reason to uncover the nature of a common physical and social world. Reason, despite the historical excesses in its application, was far better for humanity I thought than myth and superstition.

I knew that the 1960s had discovered the pleasures of debunking politicians, generals, writers, and philosophers, particularly if they were white and male. In a mild way I—especially when younger—had enjoyed partic-

ipating in the sport. But such treatment could not apply to medical heros such as Pasteur, or to great scientists like Newton or Einstein. Certainly, not all of them were "nice" people or of sterling moral virtue; I probably would not have enjoyed a social evening with Newton or a political conversation with Pasteur; nor would I have trusted Einstein with any young and attractive female relative.[13] But this situation is not surprising, since these scientists were merely human beings. But no criticisms of this kind can possibly discredit their monumental achievements.

Such were the clichés which described my state of mind around 1995. Alas, like Monsieur Jourdain who discovered that he had been thinking and speaking "prose" all his life, I soon learned that I had been thinking and speaking "modernism" all of mine. But while "prose" was still presumably a viable option for Jourdan, modernism was now definitely passé for anyone with any pretence to sophistication. In my ignorance I had not realized that most, perhaps all, of my attitudes concerning science were viewed with contempt by advanced intellectuals. Locked in my narrow technical world composed of splines, classical inequalities, weighted Sobolev embeddings, or the spectral properties of differential operators, I was unaware that since the 1970s there had been a "postmodern" shift concerning science, mathematics, and reason almost as profound as the shift beginning in the seventeenth century and completed in the Enlightenment, but in the opposite direction.

An awakening from my dogmatic slumbers came in the spring of 1996 when I discovered Alan Sokal's marvelous hoax. Sokal, a physicist at New York University, had placed a completely nonsensical article in the cultural studies journal *Social Text*.[14] The article was written in a ridiculous jargon and was full of mistakes, yet it had been refereed and accepted by a journal that included such intellectual heavyweights as Andrew Ross, Frederic Jameson, and Stanley Aronowitz among its editors. The ensuing howls of rage by victims of the hoax and Sokal's replies made the national press.[15]

[13] But this aspect of Einstein although real is often exaggerated. The most reliable account of Einstein's love life may be found in Isaacson (2007).

[14] Deliciously entitled "Transgressing the boundaries: Toward a transformative hermeneutics of quantum gravity." The original article was published in *Social Text*, 46/47 (1996), pp. 217–252. A reprint may be found in Appendix A of Sokal and Bricmont (1998) or in Sokal (2008). One should be a physical scientist or mathematician to fully appreciate the unsavory bouquet of this paper. Perhaps its most remarkable feature is that it was not noticeably sillier than some of the other papers in the same issue of the journal.

[15] Several of the responses to the hoax have been republished by the editors of *Lingua Franca*; see The Sokal Hoax (2000).

There were many letters to the editor and op-ed pieces in major newspapers. One would have had to been a permanent resident of an organic chemistry lab (as some of my colleagues in the Chemistry Department were), a desert island, or even a petri dish to have been unaware of the controversy. Then I saw a splenetic review (I forget by whom) of Gross and Levitt's 1995 book *The Higher Superstition* which piqued my interest, and I decided to read it. The book was a devastating portrait of what the authors conceived to be the ideological zoo of contemporary "science studies." In this way I learned of the existence of the "Science Wars" which were apparently raging—without any awareness on my part—all around me, possibly even in certain departments of my own university. A little later I was led to Sokal and Bricmont's *Intellectual Impostures* (1998), a demolition of postmodernist French intellectuals like Lacan, Irigaray, Baudrillard, et al., and also to the critical essays in *A House Built on Sand*, edited by Noretta Koertge (1998). As a result of this initial experience, I started an accelerated course of reading several of the sources considered by these authors. My first reaction was to compare myself to Rip van Winkle. After a nearly thirty year nap I had awakened to a new and strange intellectual world. I was amazed by it just as Bernal Diaz, a soldier of Cortez's army, was when he first gazed at Tenochtitlan, the fantastic capital of the Aztecs and learned of the sacrificial feeding of the gods with living human hearts.

The writers I examined had attitudes that were polar opposites to mine. Most shared a common stance that science is just another form of socially constructed "discourse" divorced from an independently existing natural world (which itself is a social construction). At best it was one "way of knowing," peculiar to the West, not to be privileged over others, and whose theoretical modes of explanation tell us more about the political agendas or social situation of scientists than about an essentially unknowable "reality".

More precisely, the following is a representative sample of the doctrines I encountered. They are given in a list of fifteen propositions. I have arranged them in a quasi-logical order, going from the philosophical to the stridently political. A few contain striking quotations from prominent writers, but most are abstractions from many sources.

(i) Science claims to "discover" the underlying regularities of nature by observation, but this is an illusion. We can establish such regularities only by inductive argument. However, the validity of induction has been destroyed by the objections of Hume. The regularity of nature is part of the belief system of scientists; it is not a property of the world.

(ii) Furthermore, "observation" itself is a problematic concept. What a scientist observes is always determined wholly or partially by the theoretical and/or ideological apparatus he or she brings to the task. Observation is "theory-laden." We see only what our theories permit us to see. We are not a *tabula rasa* which the world inscribes; rather it is our concepts and theories which "construct" the world.

(iii) More generally, scientists have no access to the world as it is. All we can know are our representations of it—which from (ii) are in great part supplied by us. Hence the claim that nature and its properties exist independently of our representations of it is meaningless, for how could we ever verify this? We can only compare one representation with another, not a representation with the reality it is supposed to represent. A consequence is that the correspondence theory of truth is false.

(iv) Whatever the status of observation, scientific theory is always underdetermined by the evidence. This evidence is always finite since it necessarily consists of finitely many experiments or observations. But a sufficiently complicated theory—and there are potentially infinitely many of these—can explain any finite amount of evidence. Duhem and Quine have also shown that it is impossible for a single crucial experiment to falsify a theory since it is possible to explain away failure by changing one or more of the auxiliary assumptions involved in the experiment.

(v) Since observation is theory-laden and experiments settle nothing (by (ii) and (iv)), scientific claims or theories are not "true" in the sense of corresponding to or describing the underlying facts of nature, but only reflect the consensus of scientists. What we call "science" is a network of "interpretations" hammered out by quasi-political negotiations among scientists, just as Party dogma is hammered out by Party ideologists or theology by the Holy Office. "Nature" may play little or no role in the formulation of these interpretations; at the very least she does not "compel" them. In the words of Bruno Latour's "Third Rule of Method":

> Since the settlement of a controversy is the *cause* of nature's representation, not its consequence, we can never use this consequence to explain how and why a controversy has been settled. [italics in original][16]

Thus, since nature does not exist independently of human representations and our so-called knowledge of it is a purely human product and not a reflection of a mind-independent reality, it follows that scientific theories are "social constructions" rather than "discoveries" about the world. Like other human cultural activities they are "situated" relative to a particular social and historical context.

(vi) The negotiations determining scientific theories may not even be "rational" processes. Different theories are embedded in much larger intellectual structures called "paradigms." Paradigms dictate what problems scientists should

[16]Latour (1987), p. 258.

investigate, what questions theories should answer, and and what constitutes valid scientific explanation. Paradigms are mutually "incommensurable", i.e., there are no standards accepted by both sides which can decide between them. As in political disputes scientists talk past each other. Consequently, the decision must rely on rhetoric, persuasion, networking, social pressure, etc.—in a word, "politics."

(vii) What in particular are "scientific facts" or "truths"? In view of (v) and (vi) they are just those interpretations agreed on by the dominant scientific group or victorious paradigm. They are an expression of social power and consensus, not a "mirror" of reality. Consequently, "facts" are not stable. They develop and change with time. One era's facts or truth are a latter era's mistakes. Like other human cultural expressions, they and the theories behind them are relative to their social and historical context.

(viii) Scientific progress considered as an cumulatively more precise understanding of nature is an illusion. Accounts of such "progress" are merely "Whig history"—in other words edifying propaganda produced by those whose conceptions momentarily dominate science.

(ix) External political, social, and other contextual factors also play a critical role in the formation of science. Race, class, gender, political ideology, or the demands of the state or the military determine (often through funding decisions) what problems the scientist will investigate and what methods he will use. More than this, they also influence the *content* of scientific theories. As is implied by (ii)–(vi), the scientist's interpretation of data and the hypotheses he will entertain are functions—perhaps unconsciously—of his ideological baggage, and the ambient ideological climate can determine the social acceptability of his conclusions. The belief, therefore, that science is or should be value free, objective, or that its development depends on "internal" factors only is a myth. Science is as affected by external political and social factors as much as any other cultural "discourse." The same is true even of mathematics. The mathematician's belief in his personal autonomy is a delusion. "The point is that neither logic or mathematics escapes the 'contamination' of the social."[17] To express the mutual entanglement of science with politics more abstractly: "Solutions to the problem of [scientific] knowledge are solutions to the problem of social order."[18]

(x) The nature of these external factors as well as who deserves the credit for "unmasking" them has been well expressed by Alan Sokal:

> ... most recently feminist and poststructuralist critiques have demystified the substantive content of mainstream Western scientific practice, revealing the ideology of domination concealed behind the facade of 'objectivity'. 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

[17] Aronowitz (1988), p. 346.
[18] Shapin and Schaffer (1985), p. 332.

> from being objective, reflects and encodes the dominant ideologies and power relations of the culture that produced it . . . [19]

(xi) Not only does science encode ideologies of domination *internal* to the West, but even at its purest it is basically a Western ethnoscience "molded on the twin templates of capitalist greed and imperialist expansion" and thus is a tool for the oppression of the Third World.[20]

(xii) We conclude from (i)–(xi) that science does not give "objective" (whatever this can possibly mean) knowledge about the world. At best such a belief is a product of wishful thinking, and in many cases it is a scam practiced by scientists to enhance the power of the ruling elites or of the West generally, as well as their own social prestige. Thus science should not be "privileged" over "other ways of knowing" to be found in one's own society or in other cultures. For example, it is not to be ranked above intellectual systems such as Chinese traditional medicine, American Indian cosmology, Vedic science, astrology, Zuni rain dances, or African magic. Because of its ties to Western patriarchy, capitalism, and colonialism it may indeed be inferior to these alternative systems.

(xiii) It follows from (v)–(xii) that science is relative to the tradition or frame of reference in which it is embedded. While science or another set of beliefs may be a valid consequence of a particular frame of reference or perspective, the frame itself, is at the ultimate level, not given by nature, but rather is a culturally determined interpretation of nature. Secular humanists, for example, believe in Darwin because they still live in a culture of bourgeois capitalism which supplied the metaphors Darwin exploited and encoded in the *Origin of Species* and think from the perspective of metaphysical naturalism, associated since the Enlightenment with that culture. On the other hand, those embedded in the perspectives of the Bible incline to "Creation Science." There is no "neutral" evidence that can decide between the two, because the evidence is interpreted differently by the two perspectives. Fossils, for example, are either the relics of long extinct species or the relics of animals living before the Flood, a few thousand years ago, or perhaps even a trick introduced by Satan to cause men to lose their souls.[21] Each is a perfectly rational "discourse" relative to the frame of reference supporting it. From this and other examples a profound epistemological relativism follows, with the result that:

> It is no more easy to defend non-context dependent, non-culture dependent beliefs of things or objective scientific truths than be-

[19] This is a direct quotation from Sokal's hoax article, quoted in Sokal and Bricmont (1998), p. 200; but since it expresses one of the characteristic attitudes of the postmodern interpretation of science—and, after all, the article was accepted—we felt at liberty to use it.

[20] Nanda (2000), p. 208. Nanda claims to be presenting the views of Sandra Harding.

[21] The writer heard this theory fervently endorsed over lunch by a prominent NSF funded mathematician at another southern university.

liefs in gods and demons.[22]

(xiv) Given the dependence of Western science on contextual—often "oppressive"—factors (see (ix)–(xiii)), it can be changed if we change these factors. A good start will be to purge scientists with the wrong social, political, gender, or ethnic characteristics and to replace them by underrepresented groups having the characteristics we prefer, and also to change the sources of financial support behind science—such as by eliminating military or corporate funding. We can then produce a Jewish science, a Feminist science, an Aryan science, a Hispanic science, an African Mathematics, or a socially responsible "postmodern and liberatory science," etc.[23] Some of these products will be "better" or more "Strongly Objective" than the conventional science existing today.

(xv) On the other hand, science may be so deeply flawed that it would be better to simply junk it or replace it by literary criticism, the utopian politics of youth, or poetry as the preeminent cultural activity of mankind. As Richard Rorty has asserted:

> My utopia, as I have often said, is one in which poets rather than scientists . . . are thought of as the cutting edge of civilization, and are heros and heroines of culture.[24]

Let us denote the collection of sentiments (or any substantial part thereof) revealed by the above list as a "postmodern interpretation of science" (or as we have abbreviated it "PIS"). PIS has at least four main components. There is first of all a borrowed philosophic anti-realism encoded in statements like (i)–(iii) which independently of any application to science has a long and honorable tradition. Then there are constructivist theses like ((ii), (v)—(vii)) combined with relativism, e.g., (vii), (ix), and, finally and perhaps of most significance, a political attack on the "objectivity" and neutrality of science ((v)–(vii), (ix)–(xiv)). Many of the latter two categories have been the contributions of largely UK sociologists who have since the early 1970s created a subdiscipline which they call the "sociology of scientific knowledge" (or SSK) which argues that not only the institutional structure and culture of science is socially determined, but its very *content* is as well. SSK is a continuation, applied to science, of the

[22] Mary Douglas, embedded in a quotation of Yehuda Elkana defining relativism in terms similar to ours, as recorded in Cole (1991), p. 11.

[23] Of course, no one today speaks of an Aryan or Jewish science, although such notions were fashionable in certain countries in c. 1935. I introduce them here only to emphasize that in 1935 they followed from arguments *structurally* similar to those given here. I am indebted once again to Sokal's hoax article for the adjective "liberatory" although it pervades the literature he is satirizing. See Sokal and Bricmont (1998), p. 200.

[24] Rorty (1995), p. 32.

so-called "sociology of knowledge" (henceforth "SK") mainly developed by Karl Mannheim (1892–1947). Whether or not it is a really a variety of PIS in the sense that it shares the postmodern characteristics of the latter is debatable. Certainly, the two are "postmodern" in the trivial chronological sense. Yet, as we shall see below, SSK thinks of itself as a full fledged "science of science" which obtains objective knowledge about science (just as science purports to obtain objective knowledge about nature); this is a thoroughly "modern" rather than a "postmodern" attitude. There are certainly also varieties of PIS which do not explicitly involve sociology such as various strands of French poststructualism. On the other hand, we have chosen to regard SSK as a member of PIS family both because it supplies much of the intellectual muscle (even if unacknowledged) behind other forms of PIS and because it generally views any science (other than itself) as something to be deconstructed as does PIS.

Perhaps none of those of a PIS temper who are not incarcerated in a mental institution believe in *all* the statements in our list, but they are all individually close to what I have found in the literature. Some who are closest to the postmodern stance in the humanities, that is, those who see "truth," "reason," and "objectivity" as code words for various kinds of domination and who are dissatisfied with the political or ethnic status quo in the sciences emphasize (ix)–(xv). Others are simply philosophic anti-realists, buying into the remaining propositions only mildly. Additionally, some social constructivists allow nature or even "evidence" a role (how much is a matter of ongoing dispute in the PIS community) in the construction of scientific theories and certainly would not regard them as delusions. They and the anti-realists may also value science and would admit that science progresses in the sense that, although it does not converge on "truth," its theories become evermore refined instruments of prediction and control.[25] There is, moreover, evidence that the more extreme ideas on our list are now a bit passé, having had their strongest appeal in the 1970s and 1980s.[26] But common to many residents of the PIS camp is at least a certain degree of relativism together with the belief that critical influences on the development of science include almost any factor other than rational argument or evidence; hence scientific facts are (in some sense) constructed or invented rather than discovered and may have no better warrant than other forms of cultural discourse. There are also endless variations (both weaker and stronger) on all the statements on our list. We have also omit-

[25] This, indeed, is a characteristic positivist view.
[26] See Hess (1997a).

ted doctrines such as Bruno Latour's "actor-actant network theory" as we find it impossible to express his ideas coherently, although they are surely part of PIS.[27] We recognize that this analysis has been rough and inexact. The reader may think of other characteristics of PIS which we have omitted or disagree with some we have given. But we are trying to describe an intellectual miasma or fog, not a precisely defined doctrine. Our intention has only been to indicate the "flavor" of one side of the Science Wars. Also, what we have been describing is just one small pond within the much larger sea of postmodernism, a movement which has affected almost every area of culture since the 1970s. There are, for example, doctrines parallel to what we call PIS in art, literature, philosophy, architecture, theater, and in many other fields. When we describe the social changes leading to PIS (Chapter 13) we will briefly describe some of these; but we point out here that one characteristic common to all aspects of postmodernism is a rejection of the Enlightenment belief in the power of reason to accurately represent (and improve) the world. For many postmodernists this is not because of a failure of mental power, but because there *is* no world or reality that exists independently of human culturally determined concepts that purport to describe it. Instead, there are many worlds, each as actual as any other but often mutually incommensurable, which these concepts *construct*. What we call "reason" or "truth" is never absolute, but is relative to the set of concepts we use; these in turn depend on our culture, historical period, etc. No God's eye view is available to us.

What is perhaps the most remarkable feature of the PIS position on science is that it is not just the product of a minor academic sect; rather it has become in some form the "mother's milk"[28] or set of common postulates of a new and substantial discipline, usually called "Science and Technology Studies" or "Science, Technology, and Society" (STS). STS hardly existed in the 1960s, but now (as a casual internet search shows) it is a rapidly growing field represented in some form in more than fifty US universities, many offering Ph.D.s, as well as several abroad, especially in the UK.[29]

[27] But see Chapter 11 for an attempt.

[28] Levine (1996), p. 126.

[29] These include Cornell, MIT, Rensselaer, and Virginia Tech for interdisciplinary STS Ph.D. programs and Northwestern, UC Davis, UC San Diego, University of Wisconsin, and the University of Illinois at Urbana Champaign for disciplinary programs. Additionally, several other universities offer STS flavored Ph.D.s in departments such as history, philosophy of science, or sociology. At present STS faculty seems a mixture of historians and philosophers of science, anthropologists, sociologists, and cultural study types. Perhaps as the field continues to mature they will be replaced, as in the earlier case of computer science, by people having degrees in the field.

Moreover, constructivism, attacks on the objectivity, status, or epistemological privileging of science, and a general radicalism[30] have spread beyond departments associated with STS and are now found throughout the humanities; they particularly infest English, Cultural Studies, Comparative Literature, and even as we pointed out above the History of Science.

The literature of PIS is also huge. Thousands of articles and books sympathetic to it have been written since 1970. This is not to say that there are no dissenting voices. A substantial number of scholars working within STS oppose epistemological relativism, and, for example, study the sociological factors that affect the growth or organization of science without critiquing its content on ideological grounds.[31] The most vociferous dissent, however, comes from outside STS in, for example, the books mentioned above. But the most surprising aspect of all this activity is, as we have indicated, its near invisibility to most practicing scientists. There are no nefarious reasons for this; it is just an effect of the specialization and isolation within the modern university.

Had I never studied the history of science, I would have dismissed PIS as just another of the twentieth century's, if not *traison*, at least *folie des clercs*. After all, intellectual history is replete with bizarre doctrines, and for me it was more fun and more rewarding to try to show that the best constant K in the Opial-type inequality

$$\int_0^1 |yy'| \le K \int_0^1 (y')^2,$$
$$\int_0^1 y = 0.$$

is $1/4$ or to study Sobolev-like embeddings on a domain $\Omega \subset \mathbb{R}^n$ satisfying weak conditions on its boundary (such as having Minkowski dimension $< n$) than to wade through material, much of which seemed on my initial encounter with it as appetizing as the contents of an overfull and long disused septic tank installed by the Civilian Conservation Corps in the 1930s which—while afflicted with flies and an excruciating case of poison oak—I had to clean out during a college summer job in 1957.

Many of the writers considered by Gross and Levitt, Sokal and Bricmont, or Koertge supported their conclusions by historical "case studies"

[30]The political flavor that STS can have is proudly announced in the Mission Statement of Rensselaer's program: "faculty and students pursue studies of power, gender, race, colonialism, and the interactions between research and activism." See the internet website: *http://www.sts.rpi.edu/index.php*.

[31]See Hess (1997a) for an informative survey of the many interests of STS.

and claimed inspiration from Thomas Kuhn's ideas, even as they had gone far beyond him. Could this really be true of the kind, scholarly, and slightly shy person I knew? But if it was, the fact that I had been to quote Dean Acheson "present at the creation," and yet was totally ignorant of developments since the early 1960s was embarrassing. I had left the field towards the end of an age in many respects dominated by the values of George Sarton. How and why did the older conventional views of science vanish among humanistic intellectuals, especially those in universities, to be replaced by PIS, encoded in the rapidly metastasizing discipline of STS? By what weird historical transformation did Steve Shapin, Bruno Latour, or Harry Collins supplant Alexandre Koyré, C. Coulton Gillespie, or I. B. Cohen in the academic study of the nature and history of science? How did we get from *From the Closed World to the Infinite Universe* to *Leviathan and the Air Pump* or from *Franklin and Newton* to *Pandora's Hope*? This radical change had happened, moreover, in little more than a generation—during a period when science was developing at an exponential rate and had achieved many of its most spectacular successes—and in a modern increasingly secular society which had been more committed to the development and exploitation of science than any other in history. This book is an attempt to find some answers to these questions.