

“New Age” Philosophies of Science: Constructivism, Feminism and Post-Modernism (Forthcoming in *British Journal for the Philosophy of Science* **51** (2000), 667-683

Noretta Koertge

---

Abstract

This paper surveys three controversial new directions in research about the nature of science and briefly summarizes both the intellectual and sociological impact of this work. A bibliographic introduction to the major literature is provided and some fruitful directions for future research are proposed. Philosophers of science are also exhorted to perform “community service” by correcting misunderstandings of the methods of science fostered by these new approaches.

- 1 Introduction
- 2 Sociology of Scientific Knowledge
- 3 Feminist Critiques of Science
- 4 Postmodernism
- 5 Conclusion

---

1 Introduction:

As Kuhn has taught us, one quick way to find the center of gravity of a field is to look at its textbooks. When I came to the HPS department at Indiana in 1970, Nagel’s *Structure of Science* [1961] was recommended as summer reading for entering graduate students and provided the background for the set lectures on logical empiricism. To learn about new trends, students read Kuhn’s *Structure of Scientific Revolutions* [1962], followed by a salutary dose of Scheffler’s *Science and Objectivity* [1967]. The next couple of decades saw increasing attention devoted to those philosophers that Stove [1982] labeled “Four Modern Irrationalists”: Popper, Kuhn, Lakatos and Feyerabend.

By the time the huge MIT press anthology edited by Boyd et al. [1991], and the 1992 *Introduction to Philosophy of Science* co-authored by the Salmons and other members of the Pittsburgh HPS department appeared, the topic of Scientific Change was a non-controversial part

of the canon. Nevertheless, most of the subject headings could be mapped remarkably smoothly onto the Table of Contents of Feigl and Brodbeck's 1953 anthology. That is not to imply that nothing radically new was being said on the topics of explanation, confirmation, causality, spacetime, functionalism and the structure of scientific theories. Quite the contrary, as other articles in this series will amply demonstrate! Rather, it seemed to be the case that the canon had simply expanded: Where philosophers had formerly devoted the bulk of their attention to "statics" - issues arising when trying to analyze the logical and cognitive *structure* of mature science, some were now turning their attention to "kinetics" - issues associated with trying to characterize the epistemic and methodological aspects of the *development* of science.

If we now fast-forward to the end of the century, some quite different themes appear: Couvalis' 1997 monograph, subtitled *Science and Objectivity* thus echoing Scheffler's critique of Kuhn, has chapters on Relativism, Sociology of Knowledge and Feminism. Klee's 1999 collection of readings devotes Part II to "Historicism and its Aftermath", where we find sections on Social Constructivism and Feminism. The second edition of Kourany's anthology [1998] not only begins with a section on the "Social Context in which Scientific Knowledge is Produced", but also integrates essays written from feminist and social constructivist perspectives into units on the traditional topics of the "Empirical Basis" and "Validation" of scientific knowledge. The plural in the title of McErlean's *Philosophies of Science* [2000] reminds us of postmodernist rhetoric and literally half of the book is devoted to Cultural Critiques of Science, Narrative and Metaphor, and Feminist Dimensions.

Some of the above books incorporate quite devastating criticisms of these new perspectives. Given the high premium the contemporary textbook market in America places on trendiness, the inclusion of novel topics may be partly explained by market forces, not disciplinary centrality. And not all of these books will become standards in the field - it is perhaps worth noting that neither the 3rd edition of the popular compilation by Klemke et al. [1998] nor the Norton anthology [1998] edited by Curd and Clover give more than a token nod to these new areas . But perhaps the most persuasive indication that we're "not in Kansas anymore" is the decision of the Editors of *This Journal* to

commission an article on these Oz-land topics for their millennial series!

There is no umbrella term which naturally comprises what I am calling “new age” commentaries on science and it is probably best to analyze them separately, a strategy I will follow below. Nevertheless, they all share a certain take on post-positivist developments in philosophy of science. They cite familiar philosophical discussions of the theory-ladenness of observation, the under-determination of theory by data, anti-realist conceptions of scientific theories, and non-foundationalist accounts of observational or experimental data, and then construe them to be arguments that no interesting *epistemic* or *methodological* distinctions can be drawn between scientific “knowledge” and other systems of belief. Any epistemological authority attributed to science is thus to be explained in *sociological* or *political* terms. What philosophers of science see as nuanced and subtle post-positivist accounts of science are now read as an unacknowledged surrender to scepticism. Some of the confusions lurking in this interpretation are discussed in Hacking [1999, Chapter 3], Couvalis [1997, Chapter 6] and Kitcher [1998].

One caveat before I commence a discussion of specific “new age” approaches: the literature on these topics is enormous and my sample of references is inevitably dictated in part by convenience. The primary goal here is to provide a point of entry into on-going debates, not resolve them, although in some cases I will suggest lines of research that look promising.

## 2 The Sociology of Scientific Knowledge:

Hacking’s 1999 book *The Social Construction of What?* provides an amusing survey of enterprises that self-identify as constructionist, ranging from studies of the social construction of serial homicide to histories of the inventing of Japan. The same term has also been adopted by pedagogic theorists. Matthews’ *Constructivism in Science Education* [1998] shows how theories of concept formation in children stemming from the work of Piaget have become entwined with new developments in the sociology of scientific knowledge (SSK) associated with the Edinburgh school.

We will restrict our discussion here to the social constructivist

programme in history and philosophy of science. Nelson's "How *Could* Scientific Facts be Socially Constructed?" [1994], which provides examples of constructivists' own characterizations of their approach, serves as a useful short introduction.

For example, Woolgar and Ashmore characterise constructivists as holding that "scientific and technical knowledge is not the rational/logical extrapolation from existing knowledge, but the contingent product of various social, cultural and historical processes" [1988, p. 1]. In the introduction to *Confronting Nature*, Pinch describes social constructivists as

"now try[ing] to provide explanations of the very content of scientific knowledge - an endeavor which has been accomplished by detailed empirical studies of scientific developments (both contemporaneous and historical) and by anthropological-style studies of 'laboratory life'." [1986, p. 3]

Citing the sorts of invitations to scepticism listed above, Pinch then concludes:

"Ultimately the sociologist must broach sociological factors to explain why some beliefs in science are accepted by the actors and others rejected... In other words, it must be shown how interpretative flexibility vanishes from scientific findings... Given that epistemological/methodological canons do not exclusively settle this matter, the sociologist must offer some alternative to explain how agreement arises in science [1986, p. 21]

Pinch presents the central features of what we might call the constructivist historiographic research programme. Whereas Lakatos [1978] claimed that most major episodes in the history of science could be understood in terms of rational assessments of the theoretical and empirical merits of proposed explanations, social constructivists set out to demonstrate the contingency of scientific results. In case studies such as Pickering's *Constructing Quarks* [1984] or MacKenzie's "Statistical Theory and Social Interests" [1978], they argue that what are taken to be well-established scientific results, even in the long-run, are the results of *choices* made by the scientific community and that quite different outcomes of equal scientific merit were possible. So, for

example, Yule's coefficient of colligation might have triumphed over Pearson's correlation coefficient had the scientific community's interest in eugenics not entered in, a claim vehemently disputed in Sullivan's 1998 critique of the MacKenzie case study. And Pickering envisages what Hacking calls "an equally successful physics that did not proceed in a quarky way" [1999, p. 70]. The alternate route would have privileged different sets of experiments conducted with different kinds of apparatus. For one detailed critique of Pickering's history see Franklin [1998].

For physicists, such attempts to construct a counterfactual history or to demonstrate residual traces of ideology are ridiculous on their face. Hacking [1999] gives examples of their scornful responses:

"Any intelligent alien anywhere would have come upon the same logical system as we have to explain the structure of protons and the nature of supernovae" [Glashow, 1992, p. 28].

"Whatever cultural influences went into the discovery of Maxwell's Equations and other laws of nature have been refined away, like slag from ore" [Weinberg, 1996, p. 56].

There have been sustained historiographic criticisms of constructivist case studies in which it is argued that, as a matter of historical fact, there were at the time strong, internal, scientific and mathematical reasons for the superiority of Pearson's tetrachoric  $r$  (cf. Sullivan [1998]) or the preference of the physics community for experiment E122 at SLAC over the Washington-Oxford experiments (cf. Franklin [1998]). These debates follow a general pattern: constructivists argue that since traditional scientific desiderata are not strong enough to explain the choices scientists make, sociological factors must be brought into play. Their opponents then re-do the history, arguing that as a matter of fact the winning theory, concept or experiment was clearly superior according to non-controversial methodological or epistemological criteria. The opponents go on to conclude that the professional or ideological interests of scientists play no essential role in these historical episodes. Although we need not omit all mention of interests, a suggestion one might infer from Weinberg's comment about refining slag from ore (one is also reminded of Lakatos' recommendation that lapses from his normative historiographic schema

be relegated to footnotes!), at best the sociologists' account adds a bit of human interest to the rational narrative and at worst it detracts us from what Popper would call the logic of the scientists' problem situation.

Sociologists of science respond to their opponents by criticizing the deployment of what they call "the zero-sum assumption", a term introduced by Bloor and Edge [1998] in a letter to the editor of *Physics World* in which they fault Jean Bricmont and other scientists for assuming that cognitive and social factors are two, mutually exclusive categories of causes for beliefs. MacKenzie also focuses on the supposition "that cognitive and social explanations are mutually exclusive, and "trade off" against each other" [1999, p. 230]. MacKenzie believes this assumption is often false, but that it is perhaps "our culture's most entrenched prejudice about the processes shaping knowledge" [1999, p. 231].

I might disagree with MacKenzie's claim about the popularity of the zero-sum assumption in our culture at large - it is, after all, a cliché to say that suicides are "overdetermined" or that even paranoids have enemies. But Lakatos' proposed division of labor between "external" and "internal" factors in doing history of science is very much in the spirit of the zero-sum assumption and, as I argued in Koertge [1999], there are very good reasons why scientists should adopt it. Although it is fashionable in philosophy of science today to deny the legitimacy of talking about *the* scientific method, nevertheless, a central part of science education comprises methods of avoiding error: everything from rinsing test tubes with distilled water to the "gold standard" of randomized experimental design. A large number of these operational norms have the explicit function of insulating scientific results from the interests and ideologies of scientists. Cognizant of the folk wisdom embedded in the aphorism "the wish is father to the thought", scientists systematically use instruments to replace subjective judgments and double-blind experiments to insulate their findings from their own expectations.

In fact almost every methodological maxim has the intended function of preventing a causal link between interests and results. As Popper often emphasized, the objectivity of scientific knowledge does not depend on the purity of motives of individual scientists. It is the *sociology* of

scientific institutions, not the psychology of individual scientists, which is primarily responsible for the special character of scientific knowledge. It may be that sociologists of scientific knowledge are conflating two quite separate claims: it is non-controversial to claim that sociological factors such as peer review play a causal role in the production of scientific knowledge. To claim, however, that factors such as an interest in eugenics directly influence one's appraisal of statistical measures implies that the social institutions of science are not working properly.

Ironically, it is the social norms of science that dictate the adoption of the zero-sum approach. If the sorts of interests invoked by sociologists of science are influencing results, it *is* a sign of bad science. The attempt to establish "both/and" explanations as a paradigm for history of science goes against our best understanding of the historical development of the institution of science. Of course, that institution does not always work properly and when it fails sociologists and others should quickly point it out, but this should be viewed as an occasion for sorrow, not as a triumph of the explanatory power of the sociology of scientific knowledge! The strength of scientists' resistance to SSK is a good measure of their commitment to the traditional scientific norms described by earlier sociologists of science such as Merton and Parsons.

The SSK approach has generated some lively discussions about episodes in both past and contemporary science. Its effect on philosophy of science has been more attenuated, but I will mention three areas where it has exerted influence. First, work by sociologists of scientific knowledge has undoubtedly provided some of the stimulus for recent analyses of the impact of the scientific reward system and standards of professional expertise on the production of knowledge, such as Hull's *Science as a Process* [1988] and Kitcher's *The Advancement of Science* [1993]. Ideas for a fruitful interaction between descriptive and normative approaches to the sociology of science are laid out in Kitcher's forthcoming paper on Reviving the Sociology of Science [2000].

Secondly, extravagant and confused claims about the constructed character of everything from anthrax (cf., Latour [1988]) to Zulu

nationalism (Hacking's example) have been countered by Searle's lucid ontological theory of the structure of social reality [1995]. Searle's analysis gives social institutions the combination of contingency and solidity that social constructionists talk about, but does so by showing how social facts are constructed on with what he calls "brute facts", an account that is unabashedly realist and non-relativist. It would be very interesting to supplement Searle's ideas about the "logic" of social consensus with a theory about the *development* of consensus over time and how it is maintained, particularly as applied to science.

A third area that cries out for exploration is related to the "symmetry requirement" that plays a central role in the Strong Programme. As articulated by Bloor [1976], sociologists should provide an account of scientific knowledge that would be causal, impartial with respect to truth or falsity, symmetrical in its explanation of true and false beliefs, and reflexive. Couvalis [1997] provides a good exposition of and commentary on all four tenets - here I will focus on the demand that the style of explanation be symmetric, i.e. that "the same types of cause would explain, say, true and false beliefs" [Bloor, 1976, p.5]. As with the other three elements in this manifesto, the demand for symmetry has spawned all sorts of interpretations, some trivially true or trivially false, others more worthy of investigation. It seems obviously true to say that the explanations of veridical perception reports and optical illusions will each have to talk about both the eye and the brain. Popper's discussion in the *Logic of Scientific Discovery* of what he calls Fries' Trilemma, soundly criticizes the idea that there can be a psychological causal link that guarantees the truth of "basic statements". All of this seems to resonate with the spirit of Bloor's Symmetry Principle.

On the other hand, how helpful is it to say that we should invoke the "same types of cause" to explain the protocols of subjects in a perception experiment when one is a hallucinating paranoid schizophrenic, another is hysterically blind, and a third is a normal observer? Of course, in a sense the report of the normal observer depends on the *absence* of the factors involved in hysteria or paranoia, but then it is easy to make explanations symmetric simply by listing in each account not only items that are present but also those that are absent! This particular problem can probably be solved using something like Mackie's [1974] INUS theory of causality. What is most needed,

perhaps, is some account from those advocating the Strong Programme of what counts as same or different *types* of causal factor.

If given a common sense interpretation, the symmetry principle seems very implausible. Our ability to survive in social situations seems to depend on our realization that certain epistemic situations have a high propensity to produce true beliefs while others often result in false. Folk epistemology tells us that judgments made in a hurry under emotional duress are unusually fallible and as mentioned above, the whole point of scientific methodology is to structure situations so that they are likely to be truth-promoting. One goal of cognitive psychology is to describe the “heuristics”, as Tversky and Kahneman [1982] call them, that influence our judgments about probabilities and guide our decision making in ways that can result in systematic errors. In general, it may well turn out to be very fruitful to apply causal accounts of belief formation from cognitive science to the history of science as some philosophers of science such as Darden [1991] and Mangnani, Nersessian, and Thagard [1999] are now doing, but this is a far cry from the way sociologists have pursued the Strong Programme.

### 3 Feminist Critiques of Science

There are substantial overlaps between the views of feminist commentators and sociologists of scientific knowledge. Many feminist epistemologists are also dubious of trying to draw even rough distinctions between facts/values, discovery/justification, epistemology/ontology. They, too, tend to see the content of even mature science as reflecting the interests and ideology of those who constructed it. But the disciplinary origins of the two approaches are quite different and so are some of their goals.

Feminist commentaries on science, especially in the United States, have generally been closely associated with Women Studies Programs. From the beginning these units had an explicit political as well as academic agenda - they were to be the “academic arm of the feminist movement” (see Patai and Koertge [1994]). In the case of science, one recorded not just the heroic deeds of hitherto obscure or under appreciated female worthies but also detailed the wide variety of formal and informal social mechanisms, past and present, that either

prevent or deter women from becoming scientists. These studies of *women and science*, such as those of Rossiter [1985] and Zuckerman, [1991], presupposed that science was a good thing and that it was a shame that more women did not get to engage in it. These assumptions were *not* shared by the feminists who studied *gender and science*, although the differences were sometimes obscured.

Followers of the more radical programme looked for ways in which gender stereotypes and values seemed to be inextricably linked to both the content and methods of science. "Gender feminists", as Sommers [1995] called them, produced dozens of case studies. Especially well-known are the saga of "bashful eggs and macho sperm" (see Gross' [1998] critique for references) and the attempt by Irigaray and Hales to relate the slow development of hydrodynamics to an alleged masculinist antipathy for soft, yielding fluids coupled with a gender-based privileging of linear mathematics and models employing rigid bodies. (See the critiques of Sokal and Bricmont [1998] and Sullivan [1998] for documentation.) Other attempts to show gender influence on the content of science are more plausible, especially when the scientific subject matter is obviously related to social gender roles as in anthropology or primatology, although even here one needs to look for alternative explanations. For a useful catalogue of the most famous feminist case studies, see Schiebinger, *Has Feminism Changed Science?*

Like social constructivists, feminist historians set out to reveal episodes where the path science takes is most plausibly explained by non-scientific factors, such as the availability of gendered models and metaphors. So, to take a stock example, Aristotle's theory of reproduction in which the male provides the active form and the female the passive matter would surely seem to reflect gender ideology. Yet even in this favorable case, opponents of the feminist analysis have some come-backs. Might not the model for Aristotle's theory have been chickens? Here is a case where the inertness of the unfertilized egg is easy to observe. As any observant farm girl knows, eggs laid by hens when there is no rooster around do not hatch - neither do they rot as quickly. And as any curious farm boy can testify, despite its name, there is no obvious transmission of anything other than sound and fury from the cock! One also wonders how the feminist would account for later speculative theories such as the preformationist accounts of reproduction that gave the lead role to the egg! Are we to suppose that

the inventors of the emboitement theory were less sexist than Aristotle?

Feminist histories of science have certainly enriched the range of topics covered - we now know that the Linnaeus who named the class of *mammalia* was also a staunch opponent of wet nursing (see Schiebinger [1995]) - but the attempts to show long term influence of gender on the content of science have been less successful. Might there not have been less exotic reasons for Linnaeus' decision to use mammary glands as a taxonomic category instead of other attributes proposed at the time, such as hirsuteness or the structure of the inner ear?.

Feminists have also alleged that both the theory and practice of scientific methodology incorporates gender bias. So, for example, Fausto-Sterling [1985] plausibly suggests that an experiment reporting sex differences in visual-spatial ability is poorly designed because it does not take into account the possibility that a female subject sitting in a completely dark room with a male experimenter being asked to make judgments about when the lighted rod is exactly vertical may be rather uncomfortable and hence not perform up to her real ability.

Other critiques, such as the allegations by Harding, Merchant, and Keller that Bacon's experimental method was inspired by rape metaphors and that science became more sexist when the mechanical philosophy displaced the alchemical world view, are more radical and have been vehemently repudiated. See Soble [1998] for an alternative analysis of Bacon's metaphor and Newman [1998] for a quite different perspective on alchemical imagery; both provide extensive references to the feminist literature.

Feminists have also called for the replacement of each of the traditional Mertonian norms for scientific inquiry (see Koertge [1996] for a brief introduction to this literature). One core element is the descriptive claim that women's "ways of knowing" are different from those privileged in science and hence science should be changed to accommodate women's cognitive styles. But there is also the normative claim, associated especially with Harding, that an epistemology which takes into account the "stand points" of participants would result in a "stronger" kind of objectivity. Here again there is an enormous literature. Campbell [1998], who is sympathetic to the project of

producing a naturalized feminist epistemology, has an extensive bibliography. Criticisms of the very idea of a feminist epistemology can be found in Radcliffe Richards [1996], Haack [1993] and Pinnick [1994].

Feminist philosophers of science have also produced positive suggestions for changing the way science is taught and practiced, such as Rosser's call for "female-friendly science" (see Koertge [1998] for a response). One of the most interesting proposals calls for the deliberate injection of feminist political values into the content of scientific research (see Longino [1995] and other authors cited by Campbell). In its simplest form the argument goes like this: theory testing relies in part on background knowledge and the availability of plausible alternative theories. (For example, what variables do we especially need to control for?) But gender bias affects both common sense and the family of models and hypotheses readily available at a given time. The process of scientific testing is an extended and fallible one and theories are often applied before their erroneous aspects are corrected. For both reasons, science would be improved if we were to go out of our way to introduce feminist and other politically progressive ideas into science: feminist perspectives would introduce a greater diversity of perspectives into science and even if they were factually wrong they would be less likely to do social harm.

Koertge [2000] provides a partial rebuttal to this argument, pointing out that it is often not easy to tell which scientific conjectures are politically progressive, especially since that depends on predicting the varying social contexts in which the theory will be applied. (Genetic theories of homosexuality would have been a disaster in Nazi Germany, but many gay activists in America welcome them.) Nevertheless, there is no doubt that feminist philosophy of science is challenging the field to provide more complete and more sophisticated accounts of the proper role of social and ethical values in science.

#### 4 Postmodernism and the "Science Wars"

Many of the works and issues listed in the previous sections have enough intellectual gravitas to make them worthy of philosophical attention, regardless of the outcome of that scrutiny. But amongst the "new age" commentaries on science, we also find writings that seem to

have little scholarly merit - or so it appears to many scientists and philosophers of science. The result has been a polemical public debate, popularly known as the "Science Wars". Key writings include Gross and Levitt's *Higher Superstition*, the "Science Wars" issue of the journal *Social Text* (part of which is reprinted in Ross 1996), and Sokal and Bricmond's *Fashionable Nonsense* (which includes the famous Sokal hoax paper omitted in the Ross 1996 reprint of the original issue of the journal).

Some of the social constructivist and feminist works discussed above have played a role in the "Science Wars", but in this section I will comment briefly on additional themes found in these controversial writings about science. There are many historical roots to the conflict - one thinks immediately of the romantic reaction to the Enlightenment and C. P. Snow's account of the "two cultures". Holton's *Science and Anti-Science* analyzes the wider historical context of the "anti-science" phenomenon. But perhaps the best way to focus our overview here is to recall Clark Glymour's provocative introduction to his *Theories of Evidence* in which he contrasted two kinds of people in the world, logical positivists and goddamn English professors! This crude categorization has proved to be remarkably prescient. Although there are probably no self-identified logical positivists today, there *are* lots of English professors and a significant number of them are now writing about science, an enterprise which they find to be vitiated by the logical values and empiricist delusions of positivism!

There are many reasons why English professors might turn their attention to science. Internal developments in the field of literary criticism, such as deconstruction and reader-response theory, stress the plasticity and relativity of interpretations of *all* texts so why not apply these techniques to the last bastion of literalness, scientific treatises? Furthermore, in beginning college composition classes in America, students are traditionally asked to analyze the rhetorical strategems used in current public controversies. Students are very interested in issues of environmental ethics, but here the basic science of the situation is often difficult. How tempting in a postmodernist climate to teach them to analyze the scientific elements that figure in, say, debates about the greenhouse or second-hand smoke as appeals to authority, not as clashes between inconclusive data sets. Deconstruction became a sophisticated alternative to the traditional

conception of critical thinking.

Whatever the etiology, the result was a corpus of essays and books critiquing science written by people well trained in literary criticism but often with little background or previous interest in science. This work is anchored in centers or institutes for the study of science led by English professors, such as the Center for Interdisciplinary Studies in Science and Critical Theory at Duke University and the Rutgers Center for the Critical Analysis of Contemporary Culture. There then developed loose alliances between literary deconstructionists, feminists, and people who worked in science, technology, and society studies, itself a highly multidisciplinary initiative. A good example of this sort of cooperative effort is provided by the Ross collection, where we find essays by a veteran of the science-for-the-people movement, Richard Lewontin, Langdon Winner, a political scientist who has written interesting histories of technology, Sandra Harding, a leading feminist philosopher of science, and Katherine Hayles, who has two books on chaos in science and literature. All of the contributors were asked to comment on the Science Wars phenomenon, especially Gross and Levitt's *Higher Superstition: The Academic Left and its Quarrels with Science*. There are also frequent references to the proceedings of a conference held at the New York Academy of Sciences, entitled *The Flight from Science and Reason*.

The *Flight* collection, edited by Gross, Levitt and Lewis [1996], also reflects a loose coalition. In this case the primary voices are those of scientists and physicians; they are joined by historians and philosophers of science and others - including some English professors! Their articles document attempts to invalidate scientific reasoning and minimum standards of rationality, spanning the gamut of alternative medicine, academic charlatanism, voodoo sociology, "scientific" creationism, postcolonial feminism, afrocentric pseudoscience, pathological social science and ecosentimentalism.

The antagonisms exhibited by the opposing groups in the "Science Wars" were evidently widespread, not only in America but also in Europe, as was evidenced by the tremendous interest the Sokal affair generated in both the popular and academic media. Sokal's parody paper, "Transgressing the Boundaries: Towards a Transformative Hermeneutics of Quantum Gravity", was published in the very issue of

*Social Text* that was supposed to put paid to what Ross called the “churlish tone” of scientists such as Gross and Levitt. The successful hoax was undoubtedly cathartic for some and sobering to others and it raised interesting issues about how to maintain quality control in new interdisciplinary fields. It left, however, the fundamental controversy unresolved: what is the proper goal of science in a democratic society, explanation or emancipation, and if conflicts between these desiderata arise, how are they to be resolved?

Although postmodernist theorists refer to writings and issues within philosophy of science, their intellectual impact on the field thus far is limited. Perhaps they will have the salubrious effect of encouraging philosophers to express positions more clearly and circumspectly in order to minimize the chance of being misunderstood! Their *sociological* impact, however, is far more significant. Philosophers of science have traditionally placed a premium on having close contacts with working scientists - the subject matter for general philosophical accounts comes from present as well as past science and there are often very fruitful collaborations between philosophers of physics or biology and theoretical physicists or biologists.

At first, it looked as if the “Science Wars” might drive a wedge between scientists and philosophers of science. Scientists pride themselves on correcting the errors of their colleagues. Now they were being bombarded with what appeared to them to be bad philosophical analyses, so why weren’t philosophers of science speaking out? Why did philosophers wait until a biologist (Gross), a mathematician (Levitt) and a physicist (Sokal) stepped forward to comment on the clothing of the new anti-empiricists/anti-imperialists? And if philosophers thought scientists’ characterization of the epistemology of science was too naive, why weren’t they wading into the public debate and sorting things out? Philosophers are now taking more of an interest, as evidenced by recent sessions at professional meetings, and they may well be pressured by external factors to do more. As units for the study of science and culture or science, technology and society become more popular within universities, the present institutional arrangements for doing history and philosophy of science will undoubtedly be affected.

## Conclusion:

During the last fifty years the field of philosophy of science has seen a steady expansion of the aspects of scientific inquiry deemed to be of philosophical interest. Questions about the logical structure of theories, confirmation, and meaning were supplemented with problems about concept formation, the nature of scientific discovery, and the roles of metaphysics and heuristics. Discussions of classical issues such as explanation and induction broadened to include pragmatic factors, such as the actual historical availability of alternative theories. The historical turn in philosophy of science was quickly followed by the cognitive turn and an increased interest in naturalized epistemology. Although these new directions were often inspired by work in adjacent fields, most of the impetus for growth came from within. There were quarrels about which developments were most important or most promising but these disagreements rarely led to the disbanding of departments or the secession of disgruntled philosophers to form new professional organizations.

The “new age” initiatives I have surveyed offer a different sort of challenge. Here we have a large influx of works that present themselves as philosophical perspectives on science. People from other academic disciplines are talking about “our” issues and footnoting “our” colleagues. I have suggested that our profession needs to make two quite different sorts of response to this growing corpus.

First, some of the issues being highlighted by these disparate approaches are ones that philosophers of science have already found to be of at least peripheral interest: analysis of the ways in which the social structures of science either reinforce or undermine its epistemic goals; ditto for science policy; the nature of the interactions between science and technology; the interplay between ethical, pragmatic and epistemic considerations in the design of experiments; studies of the relationship between the logical structure of mature science and the cognitive capacities of knowers; the roles of pictures, models, metaphors, and computer simulations in both developing science and mature science. Topics such as these should be of intrinsic interest to philosophers of science and if folks from other disciplinary backgrounds are bringing them to the attention of a wider audience we have no

reason to complain. If they do it badly, then we should simply provide better accounts.

But it is also happening that “new age” philosophies of science are being widely used as the basis for policy decisions (see Levitt[1999]). Science education at the elementary and high school level and general education requirements in American colleges are being directly effected (Stotsky [2000]. “New age” critiques of science are figuring in decisions about making agricultural technology and medicine available to developing countries (Nanda [1992]) and regulations affecting genetic research and the archeologist’s access to fossils. If we have reason to believe these philosophies are unsound and are leading to bad social results, then it is our moral responsibility to speak out. As Popper phrased it in an essay on the moral responsibility of the scientist first published in the sixties, *sagesse oblige*: “Everyone has a special responsibility in the field in which he has either special power or special knowledge” [1994, p. 128]. Philosophers of science need to shoulder part of that responsibility.

Department of History and Philosophy of Science  
Indiana University  
Bloomington, IN 47405  
USA

## References

Bloor, D. [1976]: *Knowledge and Social Imagery*, Chicago: University of Chicago Press.

Bloor, D. And Edge, D. [1998]: 'Letter: Knowing Reality Through Society', *Physics World* March, p. 23.

Boyd, R., Gasper, P. and Trout, J. [eds.] [1991]: *The Philosophy of Science*, Cambridge, MA: MIT Press.

Campbell, R. [1998]: *Illusions of Paradox: A Feminist Epistemology Naturalized*, Oxford: Rowman & Littlefield Publishers, Inc.

Couvalis, G. [1997]: *The Philosophy of Science: Science and Objectivity*, London: Sage.

Curd, M. And Cover, J. [1998]: *Philosophy of Science: The Central Issues*, London: W.W. Norton & Co.

Darden, L. [1991]: *Theory Change in Science: Strategies from Mendelian Genetics*, Oxford: Oxford University Press.

Fausto-Sterling, Anne [1985]: *Myths of Gender: Biological Theories about Women and Men*, New York: Basic Books.

Feigl, H. And Brodbeck, M. [1953]: *Readings in the Philosophy of Science*, New York: Appleton-Century-Crofts, Inc.

Franklin, A. [1998]: 'Do Mutants Die of Natural Causes? The Case of Atomic Parity Violation', in N. Koertge [ed.], *A House Built on Sand: Exposing Postmodernist Myths about Science*, Oxford: Oxford University Press.

Glashow, S. [1992]: 'The Death of Science!?', in R. Elvee [ed.], *The End of Science: Attack and Defense*, Lanham, MD: University Press of America.

Glymour, C. [1980]: *Theory and Evidence*, Princeton: Princeton

University Press.

Gross, P. [1988]: 'Bashful Eggs, Macho Sperm, and Tonypandy', in N. Koertge [ed.], *A House Built on Sand: Exposing Postmodernist Myths about Science*, Oxford: Oxford University Press.

Gross, P. and Levitt, N. [1994]: *Higher Superstition: The Academic Left and its Quarrels with Science*, Baltimore: The Johns Hopkins Press.

Gross, P., Levitt, N. and Lewis, M. [1996]: *The Flight from Science and Reason*, Baltimore: The Johns Hopkins Press.

Haack, S. [1993]: 'Epistemological Reflections of an Old Feminist', *Reason Papers* **18**, pp. 31-43.

Hacking, I. [1999]: *The Social Construction of What?* Cambridge, MA: Harvard University Press.

Holton, G. [1993]: *Science and Anti-Science*, Cambridge, MA: Harvard University Press.

Hull, D. [1988]: *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science*, Chicago: University of Chicago Press.

Kitcher, P. [1993]: *The Advancement of Science: Science without Legend, Objectivity without Illusions*, Oxford: Oxford University Press.

Kitcher, P. [1998]: 'A Pea for Science Studies', in N. Koertge [ed.], *A House Built on Sand: Exposing Postmodernist Myths about Science*, Oxford: Oxford University Press.

Kitcher, P. [2000]: 'Reviving the Sociology of Science', *Philosophy of Science*, forthcoming in Supplemental Proceedings.

Klee, R. [1999]: *Scientific Inquiry: Readings in the Philosophy of Science*, Oxford: Oxford University Press.

Klemke, E. et al. (eds.) [1998]: *Introductory Readings in the Philosophy of Science*, Amherst, NY: Prometheus Books.

Koertge, N. [1996]: 'Wrestling with the Social Constructor', in P. Gross et al. (eds.), *The Flight from Science and Reason*, Baltimore: The John Hopkins Press.

Koertge, N. [1998]: "Postmodernisms and the Problem of Scientific Literacy", in N. Koertge (ed.), *A House Built on Sand: Exposing Postmodernist Myths about Science*, Oxford: Oxford University Press.

Koertge, N. [1999]: 'The Zero-Sum Assumption and the Symmetry Thesis', *Social Studies of Science* **29**, pp. 561-68.

Koertge, N. [2000]: 'Science, Values and the Value of Science', *Philosophy of Science*, forthcoming in Supplemental Proceedings.

Kourany, J. [1998]: *Scientific Knowledge: Basic Issues in the Philosophy of Science*, 2nd ed., Belmont, CA: Wadsworth Publishing Co.

Kuhn, T. [1962]: *The Structure of Scientific Revolutions*, Chicago: University of Chicago Press.

Lakatos, I. [1978]: *The Methodology of Scientific Research Programmes: Philosophical Papers, Vol. 1*, Cambridge: Cambridge University Press.

Latour, B. [1988]: *Pasteurization of France*, Cambridge, MA: Harvard University Press.

Levitt, N. [1999]: *Prometheus Bedeviled: Science and the Contradictions of Contemporary Culture*, New Brunswick, NJ: Rutgers University Press.

Longino, H. [1995]: 'Gender, Politics, and the Theoretical Virtues', *Synthese* **104**, pp. 383-97.

Mackenzie, D. [1978]: "Statistical Theory and Social Interests: A Case Study", *Social Studies of Science* **8**, pp. 35-83.

Mackenzie, D. [1999]: 'The Zero-Sum Evaluation: Reply to Sullivan',

*Social Studies of Science* **29**, pp. 223-34.

Mackie, J. [1974]: *The Cement of the Universe*, Oxford: Clarendon Press.

McErlean, J. [2000]: *Philosophies of Science: From Foundations to Contemporary Issues*, Belmont, CA: Wadsworth Publishing Co.

Magnani, L., Nersessian, N. And Thagard, P. (eds.) [1999]: *Model-Based Reasoning in Scientific Discovery*, New York: Plenum Publishing Co.

Matthews, M. [1998]: *Constructivism in Science Education: A Philosophical Examination*, Dordrecht: Kluwer Academic Publishers.

Nagel, E. [1961]: *The Structure of Science: Problems in the Logic of Scientific Explanation*, London: Routledge & Kegan Paul.

Nanda, M. [1992]: "History is What Hurts": A Materialist Feminist Perspective on the Green Revolution and its Ecofeminist Critics', in R. Hennessy and C. Ingraham (eds.), *Materialist Feminism: A Reader in Class, Difference, and Women's Lives*, London: Routledge & Kegan Paul.

Nelson, A. [1994]: 'How *Could* Scientific Facts be Socially Constructed?', *Stud. Hist. Phil. Sci.* **25**, pp. 535-47.

Newman, W. [1998]: 'Alchemy, Domination, and Gender', in N. Koertge (ed.), *A House Built on Sand: Exposing Postmodernist Myths about Science*, Oxford: Oxford University Press.

Patai, D. and Koertge, N. [1994]: *Professing Feminism: Cautionary Tales from the Strange World of Women's Studies*, New York: Basic Books.

Pickering, A. [1984]: *Constructing Quarks: A Sociological History of Particle Physics*, Chicago: University of Chicago Press.

Pinch, T. [1986]: *Confronting Nature*, Dordrecht: Reidel.

Pinnick, C. [1994]: 'Feminist Epistemology: Implications for Philosophy of Science' *Philosophy of Science* **61**, pp. 646-57.

Radcliffe Richards, J. [1996]: 'Why Feminist Epistemology Isn't', in P. Gross et al. (eds.), *The Flight from Science and Reason*, Baltimore: The John Hopkins Press.

Ross, A., (ed). [1996]: 'Special Issue on the Science Wars', *Social Text* **46/47**.

Ross, A., (ed). [1996]: *Science Wars*, Durham, NC: Duke University Press.

Rossiter, M. [1985]: *Women Scientists in America: Struggles and Strategies to 1940*, Baltimore: The Johns Hopkins Press.

Salmon, M. et al. [1992]: *Introduction to the Philosophy of Science*, Englewood Cliffs: Prentice Hall.

Scheffler, I. [1967]: *Science and Subjectivity*, Indianapolis: Bobbs Merrill.

Schiebinger, L. [1995]: *Nature's Body: Gender in the Making of Modern Science*, Boston: Beacon Press.

Schiebinger, L. [1999]: *Has Feminism Changed Science?*, Cambridge, MA: Harvard University Press.

Searle, J. [1995]: *The Construction of Social Reality*, New York: The Free Press.

Soble, A [1998]: 'In Defense of Bacon', in N. Koertge (ed.), *A House Built on Sand: Exposing Postmodernist Myths about Science*, Oxford: Oxford University Press.

Sokal, A. and Bricmont, J. [1998]: *Fashionable Nonsense: Postmodern Intellectuals' Abuse of Science*, ?????: Profile Books.

Sommers, C. [1995]: *Who Stole Feminism? How Women Have Betrayed Women*, ?????: Simon and Schuster.

Stotsky, S. (ed.) [2000]: *What's at Stake in the K-12 Standards Wars?* New York: Peter Lang.

Stove, D. [1982]: *Popper and After: Four Modern Irrationalists*, Oxford: Pergamon Press.

Sullivan, P. [1988]: 'An Engineer Dissects Two Case Studies: Hayles on Fluid Mechanics, and MacKenzie on Statistics', in N. Koertge (ed.), *A House Built on Sand: Exposing Postmodernist Myths about Science*, Oxford: Oxford University Press.

Tversky, A. and Kahneman, D. [1982]: 'Judgment under Uncertainty: Heuristics and Biases', in Kahneman, et al. (eds.), *Judgment Under Uncertainty: Heuristics and Biases*, Cambridge: Cambridge University Press.

Weinberg, S. [1996]: 'Reply', *New York Review of Books*, August 3, pp. 55-56.

Woolgar, S. And Ashmore, M. [1988]: 'The Next Step: An Introduction to the Reflexive Project', in S. Woolgar (ed.), *Knowledge and Reflexivity*, London: Sage Publications.

Zuckerman, H. et al. (eds.) [1991]: *The Outer Circle: Women in the Scientific Community*, New York: W.W. Norton & Co.