

The Critique of Science Becomes Academic

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The author uses personal experiences to introduce the view that the critique of science, on entering the academy in the form of the sociology of scientific knowledge, has become increasingly remote from crucial social issues and social movements confronting it. By linking their analyses more with such issues and movements, science studies scholars can serve a more useful social purpose and also reinvigorate their theory.

In the early 1970s, I first became involved with the radical science “movement.” I was a Ph.D. student in physics at the University of Sydney and made contact with Hugh Saddler, recently returned from London, where he had been secretary of the British Society for Social Responsibility in Science (BSSRS). There was a small Science for People group in Sydney, named after the magazine of BSSRS. The group included a few established scientists, such as Peter Mason, professor of physics at Macquarie University; some such as myself among the junior orders of scientists; and a number of nonscientists.

In those days, a critique of science was seen as part of a critique of society. The emphasis was on political economy, especially an analysis of capitalism. There was a feeling of change in the air. Associated with the social movements spawned in the 1960s was the founding of BSSRS—which soon became radicalized—and, in the United States, Science for the People. The roles of science in imperialism, exploitation of workers, war, racism, and oppression were of central concern.

About this time, there were a number of feminist critiques of science, such as Shulamith Firestone’s (1971). But it was not until the 1980s that there were significant groups that adopted a feminist critique of science. A partial

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exception was the women's health movement and associated feminist critiques of medicine, which developed along with the 1960s' second-wave feminist movement. But the women's health movement mostly ran parallel with the early radical science movement: Each supported but did not embrace the other.

We read the magazines *Science for People* and *Science for the People*, books and articles by Bob Young and by Steven and Hilary Rose (1969, for example), and subscribed to the new *Radical Science Journal*. There was a party when David Dickson came to Sydney, shortly after writing his book *Alternative Technology and the Politics of Technical Change* and an incisive article in the first issue of *Radical Science Journal* (Dickson 1974a, 1974b). As I became more familiar with writing about science, I began to read more of the sociology of science. The now-classic books by Thomas Kuhn (*The Structure of Scientific Revolutions* [1962] 1970) and Jerry Ravetz (*Scientific Knowledge and Its Social Problems* 1971) had primed me for a comprehensive analysis of knowledge and power in science. My reading included works in the "sociology of scientific knowledge," epitomized by Barry Barnes's *Scientific Knowledge and Sociological Theory* (1974), David Bloor's *Knowledge and Social Imagery* (1976) and Michael Mulkey's *Science and the Sociology of Knowledge* (1979). The "Edinburgh school," through the journal *Social Studies of Science* and associated writings, seemed to promise a more sophisticated, penetrating critique of science. Yet, as I pondered Barry Barnes's books, it seemed that the critique was losing touch with the flesh-and-blood struggles in and over science.

As the years rolled on, sociological treatments of scientific knowledge seemed to me to become more insular, more disconnected from those early concerns about the human impact of science. As theory about the practice of science has become more sophisticated, it has become less accessible to scientists and activists. Still, it seems radical enough in principle. It seems that it should be possible to develop some radical applications from the critical theory.

My own career has, to some extent, replicated this process of moving from a critique of science for scientists to a critique of science for sociologists. After a decade of postdoctoral scientific research, my position was terminated, and I ended up in a department of "science and technology studies," meaning the history, philosophy, sociology, and politics of science and technology, with environment and medicine thrown in. A more congenial milieu could not be imagined. Yet, although the field offers great intellectual stimulation, the process of academization of the critique of science continues apace.

My aim in this article is to offer some observations on the evolution of the critique of science and where it is going. This provides a basis for suggesting resolutions to some of the theoretical dilemmas in the field and, more important, points to some opportunities for the future. It should be obvious that this critique of the critique of science is also a self-critique, a way of reflecting on my own past and future.

The Critique of Science Enters the Academy

What can be called the *social studies of science* is a diffuse field. In the English-speaking world, at the center are the social activities whose intellectual face is represented by the journals *Social Studies of Science* and *Science, Technology, & Human Values*, books by key practitioners, and spillovers into a range of journals in history, philosophy, sociology, political science, and general interest science.

Bluntly speaking, my view is that much of this professional work on the critique of science can be interpreted as a process of taking over the insights of the radical critics, recasting them in an academic and sanitized mold, and pursuing the dilemmas internal to the resulting intellectual terrain. Needless to say, this should not necessarily be considered a conscious process.

Many of the radical science activists have been driven by their commitments to a more democratic, egalitarian, and peaceful society and have worked through various social movements, including the labor movement, the environmental movement, the peace movement, and the feminist movement. Many of the academic analysts of science share these goals. But they differ in method: They prepare their critiques for classrooms, professional conferences, and journals.

In the process of academization, those critics with a more open political agenda are given little acknowledgment. For example, the magazine *Science for the People* published many incisive critiques of science. Yet it is a frustrating quest to attempt to find a single reference to *Science for the People* in a scholarly analysis of science. The problem is twofold: *Science for the People* was openly political and, in part as a consequence of this, it was not recognized as a scholarly publication itself, in spite of its many top-flight contributors and detailed referencing.

In the 1970s, the left-wing critics were not entirely ignored. For example, Barry Barnes in *Scientific Knowledge and Sociological Theory* (1974) discussed work by Bob Young, among others. But sometimes the radical critics were kept invisible. John Ziman, a prominent scientist, joined the social

science fraternity with a series of defenses of science. In his book *The Force of Knowledge* (1976), he stated that “it was all very well for an enthusiastic bunch of leftists to conceive a Society for Social Responsibility in Science. After a few years, they converted this society to other ideological ends, and most of the reputable scientists resigned” (p. vii). Ziman assumed that doing science and having an overt, nonorthodox political commitment are incompatible. But he did not want to leave the analysis of the links between science and society to the “leftists,” hence his book.

For Ziman, the critics were a shadowy source of misconception and misdirection. He never did them the courtesy of giving them names. For example, Ziman rejected any analysis that suggested that Darwin’s theory was shaped by the ideas of Malthus: “This is the sort of pitfall that is often encountered in the game of searching deeply for social and economic influences in the history of science” (p. 131). Who were the players of this “game”? Bob Young? Ziman didn’t say. Of T. H. Huxley, Ziman said, “His sincere but rather self-conscious religious radicalism mirrors the naive political radicalism of some of our own distinguished contemporaries” (p. 138). Hilary and Steven Rose? Ziman didn’t say.

Ziman at least indicated that there were some radicals in the field. Even such oblique references became less frequent in the 1980s. Today, those involved in the politically articulated critique of science seldom receive even a Ziman-style acknowledgment. For example, Harry Collins, in his 1990 book *Artificial Experts: Social Knowledge and Intelligent Machines*, has a long footnote summarizing case studies in the sociology of scientific knowledge. After acknowledging the early work of Ludwik Fleck, he gets to the brass tacks of citing contemporaries:

- Collins on gravity waves;
- Latour and Woolgar on a brain peptide;
- Pickering on magnetic monopoles;
- Harvey on quantum theory;
- Pinch on solar neutrinos;
- Travis on learned behavior in worms and rats;
- Knorr-Cetina on a food research laboratory;
- Rudwick on geology; and
- Shapin and Schaffer on Robert Boyle and vacuum pumps.

Of contemporaries, Collins has pride of place: first to be published, in 1975. It is his story, after all. Somewhere along the line, earlier works by Young (1969, 1971, 1973), Forman (1971), various authors in the *Radical Science Journal* and in Rose and Rose (1976a, 1976b), or even the vulgar Hessen (1931) seem to have disappeared—even when published in mainstream

journals. No doubt they have been defined as not really being sociology of scientific knowledge.

Collins also lists some of the inspirations behind post-Kuhnian studies, naming Kuhn himself, Feyerabend, Wittgenstein, ethnomethodology, and phenomenology. All these are properly anchored in the world of scholarship. The tradition of radical political critique of science in the late 1960s and early 1970s is omitted from Collins's account. In essence, Collins presents the sociology of scientific knowledge as a history of ideas, a history of scholastic traditions, with no suggestion that it is also a history of social movements and class, gender, and racial struggle.

Note also the topics treated by the authors cited by Collins. Nearly every one is "internal" to the scientific community, in the sense that public debates and "external" social groups played little overt role. Kuhn ([1962] 1970) and others, attacking the "internalist" history of scientific ideas, opened the possibility of a comprehensive analysis of science in society or, rather, of "science-society." (Kuhn himself retreated from the radical possibilities.) Ironically, the contemporary contextualist sociologists of scientific knowledge—the successors of the Kuhnian challenge to internalism—look primarily at issues whose social dynamics are mainly internal to the scientific community (Rowse 1986).

Lack of acknowledgment of radical or activist origins¹ is symptomatic of the process by which academics use the critique of science for professional purposes, distancing it from working scientists and activists dealing with the impact of science. The process of academization has seen a move from a critique of science in society to a critique of scientific knowledge and finally to a critique of the knower. This has led to antiseptic concerns about the way scientists persuade each other about what is a fact, the discourse of sociologists (who are studying scientists), and the epistemological basis for the knowledge of sociologists (who are studying scientists). (The details of theoretical twists and turns are omitted here to spare the reader.)

Who Benefits from Science Studies?

The answer to this question is pretty easy when it comes to that prosperous branch of the field called science policy. *Policy*, by consensus and virtually by definition, refers to decision making by government and perhaps industry. Policy researchers are on to a good thing: large contracts and consultancies from government and industry, and lots of status as being the people dealing with the "real world."

It is a common observation that science policy workers are seldom critical of the fundamental assumptions underlying their field. They may question particular policies, of course. What is unquestioned is that policy is something decided at the level of government and industry, with perhaps some mechanism for responding to pressure groups. It is no surprise that policy researchers and advisers adapt to this environment, because that is where their money and status come from. Those who are theoretically inclined will note that this assessment also follows from the sociology of knowledge.

If policy is conceived as something carried out by government and industry, then it is perfectly obvious that working in a women's group, a trade union, or an environmental group cannot be considered "science policy" (Bammer, Green, and Martin 1986). There is little status and even less money to be gained by consorting with such groups. Indeed, getting involved with a community action group can be rather suspect: It smacks of making a social commitment, which is hard to reconcile with either positivism or relativism. Similar concerns are seldom articulated about those who get involved with government or industry policy, although that, too, may involve some sort of a "commitment." Apparently, commitment to one's career or serving powerful groups is honored, but commitment where there is no money is suspicious.

The question, "Who benefits from science studies?" is harder to answer when it comes to the more academic treatments in scholarly journals. Most obviously, this work is useful to the academics themselves, because they get publications, status, visits to conferences, and, in some cases, jobs and promotions. But is there some wider use to this research? In particular, is there anything useful to members of social movements? If you talk to activists in social movements—including those who are well read—you will be lucky to find anyone who has even heard of professional work on the critique of science. Perhaps some of the work could be useful, but most of it is couched in an inaccessible academic style and deals with topics of peripheral interest. Why would a social activist be interested in the social construction of gravity waves or fluorescent lights, anyway?²

Consider some of the crucial problems of the world today.

- *War.* Considering the usual statistic that one-quarter or one-half of the world's scientists and engineers are engaged on military research and development, there has been remarkably little attention to military science and technology. To find some pungent critiques (as contrasted with "policy studies"), it is necessary to go back decades (Bernal 1939; Clarke 1971). A partial exception is Donald MacKenzie's (1990) careful analysis of the social construction of nuclear missile accuracy. This work, inspired by peace movement concerns, is among the most insightful in science studies today. But for all that, it really has little relevance to the peace movement. After all, questioning of nuclear

missile accuracy is not new. In any case, what great difference does it make to peace activists, even if they had time to read MacKenzie's scholarly treatments?⁵

- *Repression.* In many countries around the world, governments are routinely involved in surveillance, harassment, imprisonment, torture, and killing of dissidents. Science and technology are heavily involved. The science studies community seems to have ignored this area, unless the work of a BSSRS group is counted (Ackroyd et al. 1977; British Society for Social Responsibility in Science 1985).
- *Poverty and inequality.* Science and technology are very much involved in struggles over wealth and its distribution, via technology transfer, the green revolution, ownership of information, workplace technologies, ideologies of the market, and many others (Third World Network 1988). What was the last article in a science studies journal that centrally addressed the role of science and technology in creating or challenging poverty and inequality?
- *Patriarchy.* Feminist critique is thriving but has made little impact in the central science studies journals (Delamont 1987). Is it only coincidence that almost all the leading figures in the sociology of scientific knowledge are men?
- *Environment and health.* This is one area where critiques of science abound: analyses of nuclear power, risk assessment, chemical hazards, and so on. Why is this? Is it, perhaps, because social movements have made this into a respectable area, taken seriously even by white middle-class men? I do not know the answer, and there is certainly something worthwhile to be learned about why the critique of science has been applied in a more sustained and penetrating fashion to environmental and health issues than to the topics mentioned above. Unfortunately, only a fraction of the scholarly treatments are of the slightest use to the frontline participants.

So far, I have talked about direct research on issues of concern to social activists. But there is another influence here, more indirect but more pervasive: teaching. Many scholars have their greatest influence through their classrooms and students. They present critical perspectives on science that are uncommon in orthodox science texts and classes.

It is possible to postulate a "trickle-down" effect in spreading the critique of science. After leading theorists publish esoteric articles in erudite journals, their ideas are presented by their peers in a more accessible form in classrooms. Because some students are involved in activist groups, in this way the high-level critique gains practical relevance. The metaphor of trickle-down has certain attractions to theorists. It says, go ahead with remote theorizing; the intellectual system will ensure that worthy groups benefit. Unfortunately, there is little evidence for such a process. The only obvious case of trickle-down is the popularity of Kuhn's ([1962] 1970) concepts of paradigm and revolution in numerous arenas outside the traditional scientific disciplines that Kuhn studied. This sort of "vulgar Kuhn" is castigated by many of today's experts who propound a more sophisticated—and difficult to apply—

post-Kuhnian sociology of scientific knowledge. But the impact of teaching is not dependent on the trickle-down effect. Teachers in science studies simply need to deal with issues that have meaning for students. Quite a number of these students are refugees from science, looking for some relevance and application for fields that hold much promise but seem to serve only vested interests. Science studies gives hope of something different.

But is there some insight for students that they could not gain by reading such early critiques Young (1969, 1971, 1973), Dickson (1974a, 1974b), and Rose and Rose (1969, 1976a, 1976b)? The danger for advanced students is getting caught in the bog of current theory.

Theoretical Dead Ends

Some socially concerned theorists have the dream of developing an analysis that, because of its penetrating insights, is a decisive contribution to progressive social change. More academically minded theorists have the parallel dream of developing an analysis that is a decisive contribution to social understanding. In either case, the goal is incredibly elusive, because it misconceives the relation between theory and practice.

One aspect of this quest is disputes about epistemology and the role of the researcher. Steven Rose, a radical in terms of politics, takes a traditional epistemological line. For example, Rose (1976) condemns research supporting the heritability of IQ as bad science as well as bad politics. Like orthodox scientists, he draws on the authority of science to attack those whose politics he opposes. The role of the social researcher, from this point of view, is to align oneself with those who do good science. It is usually assumed, or hoped, that this is also the side of "good" politics.

The sociologists of scientific knowledge have rejected this form of analysis. They call for a social analysis of all science, whether it was "bad" or "good." David Bloor's (1976) "strong programme in the sociology of scientific knowledge" laid out the principles of analysis: causality, symmetry, and so on. It also set the pattern for the role of the analyst, who was to be a professional social scientist writing for social science journals in social science jargon. Adherents to the strong program were not supposed to become passionate partisans in the debates they studied. The strong program was built on the assumption that the analyst was above the debate, rather than involved in it. The weak program,⁴ presented by Daryl Chubin and Sal Restivo (1983), attempted to go beyond this. It recognized that analysts were always involved in the issues they studied. But the weak program was indeed weak when it came to articulating a role for a socially concerned theorist.

Whatever its practical weaknesses, the weak program was an aberration. In recognizing and recommending involvement by analysts in the practicalities of science “policy,” it was out of step with the main line of development in the sociology of scientific knowledge. The dominant theoretical thrusts were toward analysis of the role of the analyst. “Discourse analysis” tried to expose the author’s role in writing. Bruno Latour in *Science in Action* (1987) presented an “action” framework greatly disconnected from political critique and political action.⁵ Steve Woolgar (1983) has demanded attention to “the problem” of the relation between the knower and the known, an issue in epistemology rather than power.

This narrowing of theoretical focus is congruent with a shift in commitment by the analysts. Earlier writers with an overt political commitment, such as the Roses, analyzed large-scale political-economic interests linked to science. Sociology-of-scientific-knowledge theorists are more likely to conceive of “interests” as the microlevel interests associated with games of individual persuasion and advancement, such as a scientist’s interest in getting a paper published. Both approaches are “political.” The first deals with politics in the familiar public sense. The second deals with the politics of individual promotion. The shift is compatible with the career science studies academics’ greater preoccupation with personal interests than social interests.

For students looking for a critique that can provide help for social action, recent theoretical developments can be incredibly frustrating. The frustration is inevitable, because creating social change by extending the analysis is impossible. The flaw in the theoretical search is the assumption that a grounding for analysis can be founded on ideas alone. Analysis ultimately depends on practice. The analysis by academics for the most part reflects a practice of professional advancement and scholarly theorizing. An analysis relevant to social problems must be linked to a relevant practice.

All analyses are de facto—if not overt—interventions. The question is not whether to intervene but what sort of intervention, what audience, who benefits, and who loses.

Conclusion

The taming of science studies by its academic context is nothing new. Similar processes are well documented in the evolution of more traditional social science disciplines (R. Collins 1979; Furner 1975; Schwendinger and Schwendinger 1974; Silva and Slaughter 1984). The radical activists are cast adrift from the discipline in the mainstream quest for job security and status,

which are achieved through professionalization and specialization. Just as politics has become the study of government and economics has become the study of capitalism, so science studies has become the study of science as it is serving society as it is: The radical critique and the development of alternatives have been pushed to the wayside.

It is necessary to repeat a familiar question: In the words of Robert S. Lynd (1939), “Knowledge for what?” The early radical critics of science had a vision of a better society and argued that changing fundamental aspects of science-society was an essential part of getting there. Today, science policy seeks to reform society by refining operations within existing social structures. Science studies has abjured even that modest social aim. Its search for a better understanding of science is intellectual, channeled toward academic ends.⁶

Yet, just like the disciplines of politics and economics, science studies remains a contested field, with social movements as well as dominant power structure having an impact on theory and practice. For those who favor a more activist critique of science, I offer the following suggestions.

- Talk to people to find out whether there is any social analysis of science and technology that they think would be useful. That means talking to people who are directly involved with the issues. Rather than studying things that are “intellectually interesting,” the aim should be to find what is intellectually stimulating about things that people consider important.
- Practice writing in a more accessible fashion. Learn from journalists and from authors such as Theodore Roszak and Langdon Winner.
- Try to publish in journals for practitioners, not just in journals for social science peers. Try *Science*, *Nature*, *New Scientist*, and *Technology Review*.
- Do research and write about an area that is becoming hot, or investigate an area to make it hot. Political analysis can be embedded in a discussion that is read because of the subject matter.
- Aim to write practical materials that can be taken up by practitioners or activists and applied in day-to-day situations.
- Undertake “action research.” This means integrating social and intellectual goals and methods.
- Become a member of a social action group and work with others to apply the analysis of science and technology in ongoing campaigns or the development of new campaigns.

Note that every one of these suggestions could apply just as well to social science research that serves dominant interests as to research that serves relatively powerless groups. The choice of whom to serve—not theoretical sophistication—is a key issue.

But wait—a final reservation. Do we really want to go back to the overtly partisan analyses and exposés of the 1960s, with their simplistic treatment of

epistemology? Surely it is not good enough just to be a crass advocate, thereby losing both critical finesse and the status of being an ostensibly objective analyst? Well, yes, I agree that it would be desirable to develop a critique that is both epistemologically sophisticated and socially relevant, and also self-critical about its method and social location. I look forward to analyses that fulfill all these specifications. But for those of us who are not superhuman, I suspect it is more appropriate to set less exalted goals.

Notes

1. Since writing this, I have come across an interview (Darnovsky 1991, 75-77) in which Donna Haraway makes the same point most forcefully.

2. This is not to say that activists require spoon-feeding with material that is immediately applicable. But they, like any of us, need to be able to see relevance at some level to their key concerns. The academic literature seldom spells out the links between critique and application. Do the links exist?

3. At the end of his book, MacKenzie has some nice comments about thinking about a world with no nuclear weapons, but his in-depth analysis of missile guidance is not a prerequisite for these observations. Undoubtedly, there are some insights in MacKenzie's book and related articles that would be welcomed by activists. The trouble is that these insights are not highlighted by MacKenzie, whose hefty tome and formal language make the book rather unlikely reading for anyone except a scholar with plenty of free time.

4. The name "weak program" was a cute challenge to the "strong program," because Chubin and Restivo believed that the so-called weak force in physics was stronger than the "strong force." Actually, the relative strength of the strong and weak forces depends on location, a relation that is a better metaphor for the relative strengths of the strong and weak programs anyway.

5. Steve Epstein (personal communication, 22 January 1992) commented to me that Latour, like other micro theorists, "starts with discourses, texts, laboratories, and the local construction of interests, but (unlike those theorists) works his way outward to develop what by the final chapter is a sketch of Western imperialism as historical cause and consequence of the power of the laboratory." In this way, he uses the micro to deal with wider issues. I agree that there is radical potential in Latour's approach—as, indeed, there is in many approaches, even seemingly conservative ones. But so far I have seen little evidence that Latourian analysis has any attraction or use outside academic circles.

6. Since writing this, Daryl Chubin (1992) has published similar concerns.

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