Book Reviews/Comptes rendus

WENDA K. BAUCHSPIES, JENNIFER CROISSANT and SAL RESTIVO, Science, Technology, and Society: A Sociological Approach. Malden, MA: Blackwell, 2005, xiii + 149 p., index.

MATTHEW DAVID, Science in Society. Houndmills, UK: Palgrave Macmillan, 2005, xiii + 199 p., index.

MARK ERICKSON, Science, Culture and Society: Understanding Science in the 21st Century. Cambridge, MA: Polity Press, 2005, xi + 241 p., index.

DANIEL LEE KLEINMAN, Science and Technology in Society: From Biotechnology to the Internet. Malden, MA: Blackwell, 2005, x + 141 p., index.

SERGIO SISMONDO, An Introduction to Science and Technology Studies. Malden, MA: Blackwell, 2004, vii + 202 p., index.

STEVEN YEARLEY, Making Sense of Science: Understanding the Social Study of Science. London, UK: Sage, 2005, xv + 205 p., index.

Science and technology studies texts have recently proliferated. The six under review are the latest, but by no means the only ones appearing in the last three years. I have focused on books that are written by sociologists and are particularly, though not exclusively, addressed to social science students. In analyzing science and technology, sociologists have developed three broad and complementary perspectives. The first, which was launched by Robert Merton and his followers, emphasized how science and society interact but at some distance, so to speak. Science is a social institution, with its norms, organizations, rules of conduct, and its own ethos. Modern science has tried hard to distance itself from other social institutions, particularly political and religious. However, its knowledge base has remained alien to sociology; sociology had, according to this approach, not much to say on the cognitive content, which is best left to scientists themselves, philosophers and historians of science. We may call this perspective, which focuses on describing the institutions of science, science and society. This perspective emphasizes the relative autonomy of science from society, and shows that science needs an institutional basis if it is to function properly. In many respects, such as inequality of rewards,

baptized by Merton as the "Matthew effect," it is much like other institutions.

The second perspective may be called society in science and is about the practices of science: how science is done, how facts and theories are agreed upon, how science is also work in particular settings. It was established by innovative sociologists, many of whom came to sociology after training in the natural sciences. They were interested in studying science as a thoroughly social and cultural activity, including the shaping of its content.

The third perspective, which is even more general and has also been the domain of other disciplines such as history, is concerned with the impacts of science on society, which we may call science in society. All three perspectives are represented in the six books under review, but their relative weights are different. Sismondo's and Yearley's books are more concerned with society in science and the analysis of the more narrow field of the sociology of scientific knowledge. Erickson and David focus on broader considerations and relate science to large cultural and social trends. Alone among the authors, Erickson includes a chapter on the science of science fiction and another in which popular science, as presented by scientists themselves, tends to cast an heroic view of science. Kleinman and the trio composed of Bauchspies, Croissant and Restivo have written their texts with the idea of technosciences as their main organizing theme. Let us try to show where the authors depart from one another and where they converge.

The institutions of science are reviewed by all and Merton's sociology is given due credit. All authors acknowledge that Merton and the Mertonians have played a large role in founding an empirical sociology of science, as distinct from the traditional sociology of knowledge. Their emphasis on the ethos of science and norms of science has been correct, but insufficient to understand how science is performed. Empirical research has shown how norms are balanced by counter-norms and by flexibility of interpretation in their applications. The authors who adopt more closely the technoscience idea insist that the context and institutions in which science evolves today are different: commercial, military, political, even ideological pressures are put on scientists to a point where it is no longer possible to defend the complete autonomy of scientific institutions. They are much more deeply immersed in society than was once thought. Kleinman and David, in their case studies of biosciences, show how many researchers and scientists have espoused commercial interests and that their practices have changed; for instance, the norm of universalism can no longer be applied when discoveries bear high-value commercial fruits. All authors pay special attention to the transformation of science into expertise. Scientists are called to solve problems, which are often deeply political. They are more relied on in matters of risk management, environmental problem-solving, science policy, health issues, developing military technologies, not to mention that they are called to testify in the courts of law with some surprising consequences. Yearley has a whole chapter on science and the law, which other writers have eschewed.

One changing role of scientists is the frequency of interactions with lay publics. All books have something to say about experts meeting the public. Famous cases by many authors serve as demonstrations; namely, Wynne's study of government experts on nuclear radiation deceptively meeting sheep farmers after the Chernobyl accident, and Epstein's analysis of clinical testing in an AIDS-infected community. The "mad-cow" disease and its public management disaster are, of course, discussed. They make good pedagogical studies in class, either for sociology students or for science, engineering and medical students. These cases may open their minds to new realities and, especially, to the fact that lay persons are not dumb and can go to great lengths to learn about scientific subjects, through questioning authorities and mobilizing resources, that bear on scientific decisions, such as the protocols of clinical testing.

Public understanding of science is extensively dealt with mostly by Yearley and Sismondo. Instead of asking if the public understands this or that piece of science ("quiz science" in the apt words of Yearley), researchers have tried to understand how people receive and integrate science and expertise in their own cultural, social, and personal context. They judge people who present themselves as experts, trusting or not trusting them and the institutions they speak for. The so-called deficit model led to more science being almost forcefully fed to people. It was thought that people need to be exposed more to scientific culture through the press, popular journals, and schools. The lens of inquiry has changed. It still is science in society, but the relationship is no longer thought to be linear, top-down. On the contrary, public understanding of science is about how lay culture and people interpret and use science. This kind of public understanding of science casts a new light on the social appropriation of science and seems to me one of the most important aspects contributed by sociology on the changing role of science in society.

The relationship of science with political and economic institutions is often presented as a clash of authority and power, not as a cultural understanding and appropriation of science, though that may also be the case when, for example, policy-makers and scientists in epistemic communities discuss how to understand and solve global environmental problems. This is, however, under-researched by sociologists, as Yearley and Kleinman in particular show, except for such researchers as Jasanoff on the "fifth branch," and Shackley and Wynne on climate change. On the other hand, the field is widely investigated by political scientists, such as Peter Haas, Clark Miller, William Clark and The Social Learning Group at Harvard University, to name but a few, though they don't much rely on the sociology of science.

The authors are aware of the various processes of stratification in scientific communities. It is for none of them a main line of inquiry, best left to the Mertonian type of the sociology of science. But they delve into what is called gendered science. In the "old" sociology of science, discrimination against women, for whatever reasons – and there were plenty – is part and parcel of the workings of the institutions of science. Rationality and

disinterestedness seem to have stopped at the door of gender. However, after the cognitive turn, women's place, or too few places, in science is replaced by more epistemological concerns. What is a feminist theory of science? Is science's cognitive core gendered? Are the conceptions and methods of science shaped by men, for men and with men's interests in mind? If the authors share the idea that science is socially constructed, they broadly agree with feminists' diagnosis, men's interests are a special kind of social interests. Clearly, this is the view taken by Bauchspies, Croissant and Restivo. David, Sismondo, Yearley have all included a chapter on gendered science. Kleinman stays closer to the institutionalist approach in his interesting investigation of "gender and the ideology of merit" in science.

What most distinguishes the six books is their analysis of and the emphasis they put on the sociology of scientific knowledge, the hard core of the "new" sociology of science. All are of course influenced by it, and there is no way of turning back the clock. Sismondo and Yearley are insiders, but also outsiders, for they show the limits of the sociology of scientific knowledge. They understand what it means to follow scientists closely: Yearley because he has been trained in this tradition, and Sismondo because, as a philosopher (a sociologist as well), the sociology of scientific knowledge is a stimulating subject of debate. Kleinman, though he has himself done some observations of lab scientists, has moved to larger problems and considerations. Restivo has himself been a contributor to, even an American pioneer of, the sociology of scientific knowledge. But the text he has written in collaboration has taken a very broad view of science, technology and society, although the three authors expressively claim that it is deeply informed by social constructivism. These three authors are more concerned with science in society than with society in science, even though each has training in science and technology (two were first trained in engineering; Bauchspies, in physics). Erickson and David give to the sociology of scientific knowledge its due credit, but one feels that they prefer more external analyses of science, less society in science than science in society. However, David develops a critical and reflexive epistemology of science, open to many perspectives, because of science's increasing complexity and impact. For instance, following in the steps of Marx, Bernal, Hilary, and Steven Rose, he emphasizes the connection between science and capitalism and the uses capitalism makes of science: science for profits which may clash with science for the people, as Bernal would have liked science to be. Kleinman, too, examines the connection between science and economic interests, in particular when he asks who is using these new technologies, such as the Internet, the new biotechnologies and the technosciences in general. This line of investigation leads him, in a unique example, to a whole chapter on science and technology in the Third World.

Of the three or four broad theoretical approaches to the sociology of scientific knowledge, namely the strong programme; the empirical programme of relativism (for some critics too quickly assimilated to the preceding); actor-network theory; and the varieties of ethnomethodological, discourse and conversation analysis, the authors do

not hide their intellectual preferences either in treatment or in affinity. Yearley is highly critical of actor-network theory, but many others are as well. The principle of supersymmetry between social actors and natural actors cuts no ice for the authors. They certainly prefer the idea of a co-construction of science, technology and social order without attributing any form of agency to things. Actor-network theory (or the Paris School as it is sometime called) has many friends in cultural studies, but fewer, if not many foes, in sociology. The strong programme as developed by the sociologists of Edinburgh University seems to be waning: it has spearheaded a change in perspective, but its conception of social interests, as notably criticized by Woolgar, with whom many of these authors agree, has not matured well. The wine has turned sour. Latour, Callon and Law's statement that all is social and that sociality cannot be externalized from the process of scientific cognition may have been the fatal blow to S.P. Bloor's methodological principles. While causality based on external social interests given by class and social position is rejected, or at least toned down, and no longer considered the determining cause in explaining positions scientists take in a cognitive controversy, as exemplified by MacKenzie's study of statistics in Britain, social causes are not altogether abandoned.

In the empirical programme of relativism, as viewed by Collins, controversies and the closure of controversies may be sociologically explained by decisions people make in the course of studying nature, building instruments, interpreting facts, relating them to theories. But the main explanation at work is limited to the participants, localized and contextualized, and depends on who's participating. The core set, with its limited number of participants, to use Collins' expression, is decisive in stabilizing and closing a controversy. The microsociology of Collins, or Knorr-Cetina and others in ethnomethodology, is in this respect not far from the microsociology of actor-network theory. Agency is certainly not extended to objects in Collins' research, but scientific representations and explanations that last are arrived at and stabilized through relations and interactions among scientists. This process may resemble politics by another name, as actor-network theorists are fond of saying with their use of enrollment, mobilization and so on. A better explanation is simply that scientists faced with complexity and nature's silence, as Weber would have said, need to agree on what they have observed, established as data, on how to connect them to existing theory, and, when it is not possible to do so, develop a new explanation. Sismondo and Yearley insist on people making sense of what confronts them. Sismondo relies more on philosophy, namely on the Duhem-Quine thesis of underdetermination and theory-dependent observation, whereas Yearley remains sociological in stating that it is scientists who choose, not nature.

Two others differences are worth noting. The first is how often each book refers to papers in what have become the two leading journals of the new sociology of science: Social Studies of Science and Science, Technology & Human Values. Sismondo leads the pack with 60 citations to the former and 12 to the latter. Yearley follows: 25 and 5, respectively. The other books do much less referencing to these two journals. This helps to

distinguish between books that are more interested in society in science from books chiefly focused on science in society. It gives university teachers an idea of what book to choose. Let me state my own preferences. If my students were coming from the sciences and engineering, I would make them read either Kleinman's book or the text written by Bauchspies, Croissant and Restivo. On the other hand, if I were to teach to sociology students only, Sismondo's and Yearley's books would be more rewarding. If there are students in philosophy in the class, Sismondo's will win. But if the class is composed of people from the humanities and cultural studies, Erickson's is the one to choose. David's may be very appropriate in a sociology class, perhaps less suitable in a science and technology class. But students from the health sciences may like what they read, especially since the author goes into some depth to assess the new genetics and the impact of the biosciences on society.

Is anything crucial lacking in these books? Yes: a sociological analysis of the new sociology of science should have been undertaken, or at least started. Can we apply to this sociology our own methods of inquiry? Can we be reflexive, as Bloor and Collins had hoped for, but never seem to have the time or taste to do, too busy building the case for society in science? Sismondo, Yearley and, to some extent, David have written a history of the new sociology of science. They have also tried to assess it critically, if not epistemologically. There are at least three sociological questions that should be asked: how can we explain sociologically the "paradigm change" between the old and the new sociology of science? What factors (causes?) explain this shift: internal anomalies; social context; new generations of sociologists; researchers moving into sociology from natural scientific disciplines; deep institutional changes, such as a larger social base in university enrollments; and so on? This may lead to in-depth interviews with influential actors. The second question has to do with accounting for "styles" or "frames" in the sociology of science: why have the strong programme and the empirical programme of relativism flourished in Britain; actor-network theory in Paris; ethnomethodology and interactionism, as practiced by fine researchers such as Leigh Star, Fujimura, Clarke (which the six books are too silent about) in the USA? What social (that is, personal, institutional, political, cultural, economic) factors can explain these differences in the dominant approach to understanding science? And finally, how far is this new sociology of science institutionally and cognitively stabilized in research and in teaching and, if so or if not, why?

From a methodological viewpoint, we should also ask whether the conclusions of the sociology of scientific knowledge show strong differences between the sciences and scientific practices. The sociology of scientific knowledge has, on the whole, successfully applied similar principles of investigation, irrespective of the area of science. Physical and biomedical sciences have been cherished. Are there important variations in the way these types of science operate? Does the closure of controversies, for example, follow a similar process irrespective of the sciences investigated. There are good reasons to believe that epistemic cultures, as Knorr-Cetina has recently shown, differ among areas of science. What are

the consequences of this for theory-building in the sociology of scientific knowledge?

One slight deception, at least for this reviewer, is the secondary part played by technology. The relationship between technology and society is, wrongly, more or less subsumed under technoscience. Sismondo wrote the only chapter on technology. Despite writing under the banner of technosciences, Kleinman and the "trio" tend to emphasize science contributions which they see in terms of impacts. In a seminal paper on the social construction of technology, Pinch and Bijker (1984) have shown how methods and principles of the new sociology of science can be successfully applied to technology. Donald Mackenzie has gone far along this path and it is very unfortunate that his research programme is, apart from his work on statistics, so little discussed. Although the same methods can be used in understanding science and technology, are there important, irreducible, differences between them? If science is about "discovery" and technology about "innovation," aren't we faced with two logics? Are technological practices under the same constraints as science? I doubt it.

Sociology should be proud, without boasting about it, of what this new sociology of science has contributed to knowledge, despite the fact that it remains contested from outside, as shown in the Science Wars. It has produced a plethora of empirical research and has challenged, perhaps at times too aggressively, the received view of science. Sociology hasn't, however, been the only, even the main, tectonic force in shaking science's positivist foundations. It has given us a better understanding of scientists in the public space, where they meet very different publics. It has taken a nuanced and complex view of all the relations between science and society. It has described in great detail how science is carried out. But this relative success is no reason for complacency. What are the next steps?

The new sociology of science can widen its scope, notably to interdisciplinary and environmental sciences, and can encompass new objects, new areas of science, although it has, in some 30 years, covered many areas, disciplines, and research frontiers. It can sharpen its analytical tools; it can explore new avenues. It also has to find some ways to better convince reluctant disciplines, by arguments, evidence and case studies, of sociology's worth in "making sense" of science. Even if these books go a long way to show that sociology has been particularly innovative and has no axe to grind with science, none seem as yet ready to open new territories of sociological inquiry of science. This comment should not diminish their merits, but one would have liked a few suggestions. To those who think, wrongly, that this new sociology of science is devilish and undermines scientific authority, there is only one way to reply: a science that cannot accept to be critically examined from outside and opened in daylight is perhaps not worth the name of science.

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