honor and reputation, as described by Daston, was a factor in the decline of sectors of French science in the 19th century.

In their commentaries on the essays in this well-written volume, Giuliano Pancaldi and J. L. Heilbron both point to the need, when studying the various kinds of institutions associated with science, to examine more closely the science itself. Those wishing to study its practice might begin with the essays by Eda Kranakis and Svante Lindqvist and the commentary by Hans-Werner Schütt in the fourth section of this volume. Kranakis's excellent schematic overview of how technology and science interface and Lindqvist's discussion of the relation between industry and the scientific laboratory are superb examples of the historical craft. It is Schütt's belief that for some historians pure science no longer possesses what he calls a "metaphysical justification" as a "distinct phenomenon." His viewpoint is supported by the papers in this volume, which compel us to consider science as a social activity bound to cultural and social mores and values. The historical challenge, presented by Heilbron and Pancaldi, is to demonstrate precisely how scientific knowledge itself is a part of the contexts discussed in this volume.

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Sociological Explanations

Theories of Science in Society. SUSAN E. COZZENS and THOMAS F. GIERYN, Eds. Indiana University Press, Bloomington, 1990. viii, 264 pp., illus. $25. Science, Technology, and Society.

This book takes up a perennial debate over whether science is a special form of knowledge. The issue arose in its contemporary form just over 60 years ago when Karl Mannheim in *Ideology and Utopia* excluded science from the domain of the sociology of knowledge on the grounds that it was based on rational principles and thus was exempt from the dictum that knowledge is socially shaped.

The conclusion that science could be detached from the historical-social perspective of scientists paved the way for the development of a sociology of science independent of the sociology of knowledge. Indeed, the first phase of the sociology of science, originated by Robert K. Merton, was largely devoted to explaining how science, with its distinctive structure and norms, was bounded from the rest of society.

During the past 15 years, the sociology of science has taken a "constructivist" turn in which the production of scientific knowledge is viewed as the outcome of a negotiation among scientists in the laboratory. Jonas Salk in his introduction to Bruno Latour and Steve Woolgar's *Laboratory Life*, the exemplar work for the second phase of the sociology of science, commented that scientists would find some of what the authors said familiar but much else incomprehensible. It will be the same with this volume. The historical and ethnographic depictions of science will be familiar, but the theories used to explain them will appear abstruse unless the disciplinary context within which they arose is understood.

Readers of the news columns of *Science* are aware that scientists lobby for congressional appropriations, initiate contacts with the press, and reformulate the boundaries between basic and applied research. Such actions have led sociologists to raise the issue of whether science is indeed an activity apart from the rest of society. Thus, a projected third phase of the sociology of science would return to the sociology of knowledge as formulated by Mannheim, but without his exemption clause for the quantitative natural sciences. By implication the editors of *Theories of Science in Society* ask whether there is sufficient justification for a special sociology of science and, if so, how it should be formulated.

The basic theoretical issue of this book is relational. Is science best viewed as "science in society" or as "science and society"? This cryptic difference in connectives denotes a dispute over whether science is a distinctive institution in which truth claims are adjudicated independently of the exercise of power or whether science and society form a seamless web in which the principles for settling disputes within science are the same as in the larger society.

Three authors provide a counterpoint of thesis, antithesis, and synthesis on this issue. Thomas Gieryn argues that there are no essential boundaries between science and society, only ones imposed by those scientists or non-scientists who get their definition of the situation accepted at a given time. He uses the interaction between the physicist Richard Feynman and the press during the Challenger investigation to argue that science is a form of rhetoric through which scientific knowledge is created. Gieryn, following Latour, argues that Feynman succeeded not because his version of what had gone wrong corresponded more closely to a reality of nature but because he was able to lower the entrance requirements for participation in reality construction by making his version more accessible. It may be asked, however, whether Feynman's exposition of the properties of rubber was not merely a display of knowledge rather than a discovery of it or whether, if science, it is not best characterized as police or forensic science. Differences in media interpretation of the details of the display do not change the brittleness of a rubber ring under low-temperature conditions—that is unambiguous. The Challenger explosion can also be explained as a "corporate induced disaster" where the existence of a problem was known in advance by an organization but suppressed to protect bureaucratic interests.

At the other end of the spectrum Rob Hagendijk argues that science is a culturally distinct practice in which knowledge claims stand on their own without needing moral or political support. Allocation of resources can affect choice of research problems but not the answers. Drawing on his study of Dutch freshwater ecologists, he proposes a theory of crosscutting institutional spheres, with different combinations of rules and resources to explain overlap and distinction among them. In the case at hand, funding patterns were stable and so were the intellectual profiles of research groups. Hagendijk's interpretation would not likely hold under conditions of financial stringency such as currently obtain in the United States, where pressure to raise funds to maintain a group can lead to investigation of the applied aspects of a basic topic or to taking up of a new topic altogether.

Finally, Susan Cozzens argues that it is the ability of scientists and science to accumulate power along several dimensions, including the contribution to meeting the needs of sponsors, the ability to set individual research goals, and societal legitimation as an honored activity, that gives science its partly autonomous character. Cozzens discusses the Latour-Callon "actor network" approach in which scientists are viewed as building power bases for the sake of science, consisting of heterogeneous networks of researchers, sponsors, machines, and nature, all cooperating. Scientists need not be economic persons under this approach. Some can devote their time exclusively to extending knowledge while others work to enrol patrons. Cozzens recognizes that the patron-science-nature relationship captures only part of the interaction between scientists and society: how scientists serve others' goals. This model recognizes scientists' accumulation of resources from patrons and transfer of knowledge to meet the needs of sponsors, but it does not include scientists' setting their own goals in the larger society and using their knowledge and organizational skills to achieve them.

An alternative model of scientists as entrepreneurial risk-takers and champions of new knowledge-producing activities is required to capture scientists' political ability to draw on the public treasury to mount big science projects that have limited direct application. Scientists also capitalize their knowledge in the economy by forming new firms, rather than by attaching themselves solely to existing establishments. Donald Fisher discusses the founding of the Social Science Research Council and shows how social scientists along with capitalists and foundation officers set the boundaries around the social sciences. This essay establishes the relationship between power and knowledge as one in which each can be used to control the other.

Merton in his study of the sources of 17th-century science and technology argued that the direction of science and technology is significantly influenced by social factors. This conclusion is reinforced by several excellent contributions to this volume. I would like to call especial attention to Adele Clarke's analysis of the interaction between social groups and elements of various scientific disciplines that brought into existence the field of reproductive science. A model for its delineation of scientists and their social alignments, this analysis is complemented by Stephen Turner's theory of the forms of scientific patronage and Daryl Chubin's depiction of a continuum from normative science to malpractice and "pork barrel" politics. Although "technology" is not in the title of the volume, Wesley Shrum and Joan Morris's classification of technical systems by degree of certainty and range of alternatives and Ron Westrum's discussion of technical establishments as barriers to inventive activity attest to its importance in constructing a theory of science in society.

The editors incorporate a social constructivist principle, that all forms of knowledge are equivalent, into their program for viewing science in society. Proponents of the so-called strong program, such as Barry Barnes, assert that scientific knowledge is essentially social, that it develops and changes in response to practical contingencies. Thus Darwin's observation that agricultural practices were the basis for ideas about natural selection led to his theorizing about natural selection; but did not these processes exist in nature prior to their utilization for agricultural production? As in the view from Plato's cave, strong-programmers hold that the representations or constructs through which knowledge claims are made are artifacts of their culture rather than depictions of an independent reality. This position is represented in the volume by Sal Restivo's analysis that the systems of notation of pure mathematics represent social relations of competition and consensus-formation among mathematicians.

The thesis that the internal content of scientific knowledge is socially shaped goes beyond the claim that a given set of social conditions is conducive to the production of a corresponding form of scientific knowledge, as is made in Paul Forman's argument that the social instability of interwar Germany encouraged physicists to formulate probabilistic models of the physical universe. The strong program is based on the assumption that the natural world emanates from the social. This is a return to a pre-Copernican view in which human beings are presumed to be at the center of the universe. However, as human beings gain control of the direction of nature through genetic technology and of human nature through what Herbert Simon has called the "sciences of the artificial," the prospect for the social control of the internal content of science becomes more real.

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Matters of Graphics

Representation in Scientific Practice. MICHAEL LYNCH and STEVE WOOLGAR, Eds. MIT Press, Cambridge, MA, 1991. x, 365 pp., illus. Paper, $16.95.

Envisioning Information. EDWARD R. TUFTE. Graphics Press, Cheshire, CT, 1990. 127 pp., illus. $48.

Photographs, figures, graphs, diagrams, and tables seem to show how things really are, bare facts. Aptly chosen, they lend an air of authenticity and objectivity. Lynch, Woolgar, and the other authors in this collection of essays iconoclastically examine how we use pictures and displays. The examples for their case studies are happily varied. They include:

1) A famous book. E. O. Wilson's *Sociobiology* has plenty of illustrations. What for? The relations among text, captions, and pictures are not straightforward. The pictures don't save a thousand words; when they tell a story, it is not at the same level as the words. They legitimate the doctrines without bothering to argue for them. The piece on *Sociobiology*, by Greg Myers, had the helpful participation of Wilson's illustrator.

2) A classification manual. Field guides become authoritative for dedicated bird-watchers but are very difficult for novices to use. Beginners think the picture of the bird shows "how it is." In fact it tries to point out distinctive features to regulate the very odd "language-game" (for once Wittgenstein's famous phrase fits) of sighting and naming numerous birds seen outdoors. There's some good practical advice for three competing mass-circulation field guides to American birds in Law and Lynch's analysis of them.

3) Some day-to-day experimentation. Two papers use tape recordings of laboratory conversations, one, by Woolgar, showing how previous accepted graphs of experimental results help a couple of solid-state post-docs to agree on what they are seeing, and the other, by K. Amann and K. Knorr Cetina, tracing the way visible marks are turned into data, which are recorded and saved and then redescribed for a final report on the work done. This latter essay may be the best available moment-by-moment account of how a mundane laboratory fact is ascertained.

Several of the authors have a background in Harold Garfinkel's ethnomethodology. They attempt to describe the unnoticed behaviors that express tacit assumptions. The editors say that "it may, of course, be presumptuous to propose an empirical study of anything, and especially to propose an empirical study of scientific activity." My objection is not to the lack of nerve and skepticism about science that such a remark betrays. These papers are all too empirical. They remind one of the usual canards against "Baconian" science. It is no good just looking and recording; you need a structured guiding theory. Jargon only makes empiricism worse: "The juxtaposed document contributes to the achievement of interactional closure on descriptive adequacy"—that's when one of the solid-state post-docs fetches a graph.

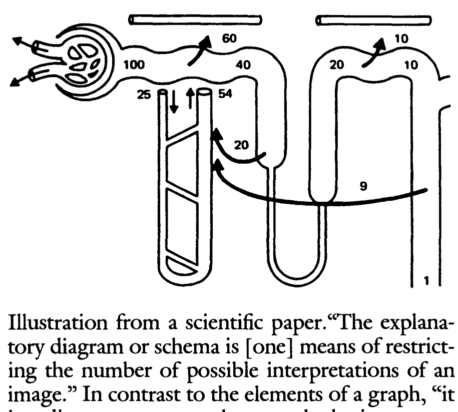

Illustration from a scientific paper."The explanatory diagram or schema is [one] means of restricting the number of possible interpretations of an image." In contrast to the elements of a graph, "it is a direct means, one that reveals the interpretation of the author." This diagram represents the concentration of urine by a nephron. [From F. Bastide's paper in *Representation in Scientific Practice*]