preneurial 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.

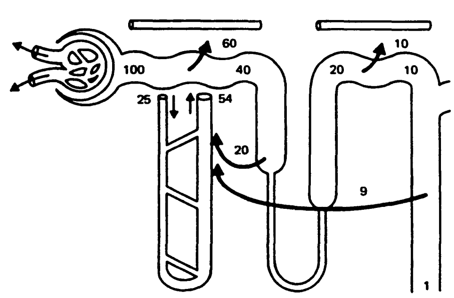

Illustration from a scientific paper."The explanatory diagram or schema is [one] means of restricting the number of possible interpretations of an image." In contrast to the elements of a graph, "it is a direct means, one that reveals the interpretation of the author." This diagram represents the concentration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

The empiricism is made striking by two speculative papers translated from a French collection of 1985. (Everything else in the book was published as an issue of *Human Studies* in 1988.) The theses expounded by Bruno Latour are paradoxical but welcome because they are theses. The success and power of science and technology are to be understood on political lines. A new idea, technique, machine, knowledge, theory—any enterprising part of "technoscience"—has to overcome competitors. It does so by forming allies. The most powerful way of building a controlling network is by overwhelming opponents with materials. You don't do this by hitting rivals with a diesel engine or an electron or a distant island or a floppy disk. You do it with paper and the like. The technoscientists think they convince by argument, by demonstrations, by prototypes. But what they exchange is inscriptions. Whether these are words or spreadsheets, photographs, tables, graphs, or maps, they have two essential properties: They are easily transported, sorted, and retrieved, and they are endlessly reproducible without change. They are "immutable mobiles."

Latour brings two tired subjects to life in a trice. Why did what we call science emerge in the Renaissance and never look back? What did the invention of printing do for the West? Answer: the two events are identical. Science is the manufacture of transportable reproducible inscriptions. The forms of representation are unimportant; all that matters are movable retrievable "documents" that can activate the largest network of users, allies. The editors of the volume say that the two translated papers "exemplify a distinctive approach . . . that creatively synthesizes semiotic, post-structuralist, and social-constructivist initiatives." Phooey; they exemplify imagination, daring, finely drawn argument, and far-reaching speculation.

The second of these essays, by the late Françoise Bastide, is a striking analysis of the diagrams and photographs in a contested paper in *Nature*. Originally stating an important discovery about the crystallization of a transfer RNA, it was later accused of error or worse. Bastide uses this story to exemplify Latour's theses and to examine what must be done to undermine this power of a coherent set of inscriptions and representations. She argues that it is unimportant whether the "immutable mobiles" are text or tables or figures or whatnot. They must compactly convey unassailable information. The sharp definition of Bastide's essay shows how much it helps to have a background theory (in this case, about the purpose of inscriptions), no matter how at odds that theory might be with the better judgment of readers of *Science*.

Edward R. Tufte's *The Visual Display of Quantitative Information* has already been praised to the skies. You'll see why on looking into *Envisioning Information*. Latour noticed how important it is that inscriptions are flat; it makes them so easy to transport and to file. Tufte illustrates "escaping flatland"—deploying the page to represent innumerable dimensions and facets. From Latour's philosophical perspective that's making the world flat. The two authors admire the same objects, the compact and immediately accessible display of complexity. (There are wonderful Japanese examples in Tufte's book, by the way, not just of "science" but of train timetables, which of course have to be flat enough to go in a purse or up on a placard.) Tufte also reminds us of the virtue of theory and slogans over mere empirical observation. He tells us why some visual things work and others are disasters, inimitably illustrating Josef Albers's doctrine about space, "1 + 1 makes 3."

There's a happy tension between the two completely different books under review. Many of the graphics taken from scientific texts and reproduced in Lynch and Woolgar are plain awful. They didn't help anyone envision information. So they must have had another purpose. *Exercise*: go through the present issue of *Science* first with Tufte in hand, to see how the information in the charts and pictures could be better presented (include the ads). Next go through it to ask whether the point of the representations is to convey information at all, or rather to convince us that this is solid stuff, not to be challenged, not challengeable.

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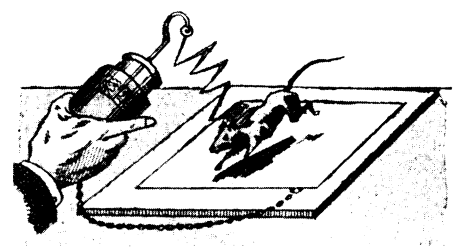


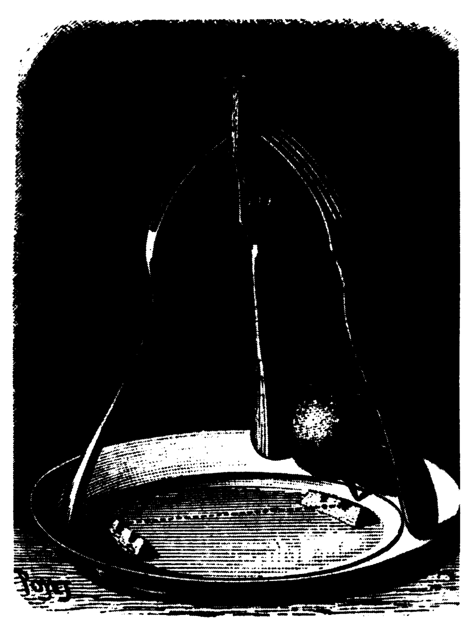
"Le rat condamné à mort," a *récréation électrique* proposed by H. Graffigny, according to whom "le condamné est parti *ad patres* sans douleur." [Reproduced in *La Science pour tous* from Graffigny's *100 expériences électriques* (Paris, 1896)]

Ventures in Popularization

La Science pour tous. Sur la Vulgarisation Scientifique en France de 1850 à 1914. BRUNO BÉGUET, Ed. Bibliothèque du Conservatoire National des Arts et Métiers, Paris, 1990. 168 pp., illus. Paper, F230.

This beautifully illustrated collection of essays by scholars and staff members associated with the Conservatoire National des Arts et Métiers in Paris focuses on traditions of *vulgarisation* or popularization of science and technology in France before the First World War. Appendixes, annotated lists, and notes (including brief biographical paragraphs on 33 science writers) provide a wealth of information about science journalists, popular science books and periodicals, publishing houses, images and spectacles, lectures, and exhibitions that have been important vehicles for the diffusion of science and technology in France.

The authors argue that a new phase of science popularization, different from the Enlightenment tradition, began around 1850, at the time of the popular success of the first Universal Exposition in London in 1851. Increased efforts were put into diffusing science to a broader public. The aims were to advance scientific progress, to increase national strength and prosperity, and to further social harmony through common goals and common understanding. In France, these aims were pressed in the 1860s and 1870s by republican scientists, educators, and administrators (like Paul Bert) who were concerned to counter Catholic



The Foucault pendulum as represented in Tom Tit, *La Science amusante* (second series, 1891). [From *La Science pour tous*]