BOOKS: HISTORY AND PHILOSOPHY OF SCIENCE

Counter-Revolutionary Kuhn?

Joseph Rouse

"History...could produce a decisive transformation in the image of science by which we are now possessed." This opening sentence of Thomas Kuhn's *The Structure of Scientific Revolutions* is now often seen as prophetic. The most widely accepted image of science dramatically changed in the decade after Kuhn's book appeared in 1962. An ahistorical conception of scientific knowledge, as a formal logical structure confirmed by direct observation of its empirical consequences, increasingly seemed remote from scientific practice. It was replaced by an image of the sciences as less unified, with multiple subdisciplinary communities pursuing research programs grounded in specific theoretical and methodological commitments. Although many writers contributed to this reconception, Kuhn's book has become, in retrospect, an icon for its own revolution.

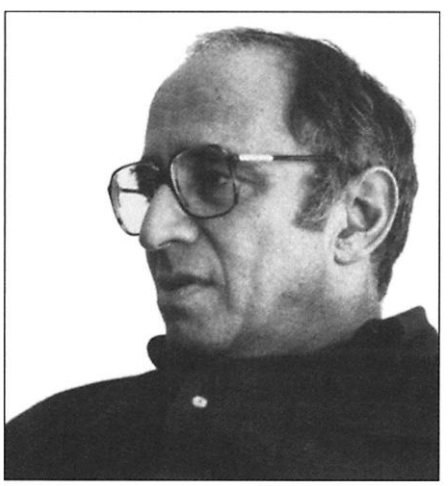
Thomas Kuhn
A Philosophical History for Our Times
by Steve Fuller
University of Chicago Press, Chicago, 2000. 490 pp. $35, £24.50. ISBN 0-226-26894-2.

Proponents and critics alike have generally agreed that Kuhn's account leaves particular scientific communities open to critical assessment by outsiders, who could challenge either the scientists' substantive presuppositions or the practices of socialization and exclusion that shape their community. Steve Fuller's recontextualization of Kuhn is thus provocative. Fuller argues that Kuhn himself aimed instead to secure the autonomy of research communities from "external" criticism and from ongoing engagement with their own disciplinary history. Moreover, he claims, the effects of Kuhn's work upon philosophy and the social sciences have been profoundly conservative. On Fuller's view, earlier philosophical programs (notably logical positivism, falsificationism, and pragmatism) insisted that sciences be accountable to goals and standards not of their own choosing. Post-Kuhnian scholars have largely abandoned such commitments. The outcome, Fuller concludes, is the "ritualized political impotence" of contemporary intellectual reflection upon science.

The author is in the Department of Philosophy, Wesleyan University, Middletown, CT 06459, USA. E-mail: jrouse@wesleyan.edu

Fuller develops these claims in three ways: an extensive historical study of the genesis of Kuhn's book within the intellectual and political milieu of Harvard in the 1950s; a more general "philosophical history" of debates over scientific research and its appropriate place in modern societies; and a biting critique of recent work in philosophy, science studies, and the social sciences.

Fuller situates Kuhn's book within J. B. Conant's Harvard General Education in Science program (where Kuhn taught while writing *Structure*), against the background of the cold war and the dramatic expansions of the scale and costs of scientific research propelled by two world wars. The Harvard case studies in the history of science immersed students, the future managerial and political elite, in exemplary "tabletop" experiments from pre-1925 science and encouraged them (mistakenly, for Fuller) to see contemporary "Big Science" as continuous with earlier eras. Both the Harvard program and Kuhn's book disconnected successful research from attention to the sociopolitical context in which scientific problems arise and are resolved. Fuller thus reads Kuhn as an analogue in the natural sciences to contemporaneous proclamations of an "end of ideology," in which political conflict would be replaced by the technocratic management of specialized knowledge.



CREDIT: MIT MUSEUM

Paradigm shifter. Despite the fame and influence of *The Structure of Scientific Revolutions*, Kuhn avoided the role of intellectual celebrity.

Early 20th-century disagreements between Ernst Mach and Max Planck over the nature and future direction of the sciences are the centerpiece of Fuller's larger philosophical history. Their debate partly paralleled more recent arguments (with Philip Anderson in Mach's role and Steven Weinberg in Planck's) over whether high-energy physics is "fundamental" science. But more was at stake for Mach and Planck than funding priorities. At issue were the role of science in public culture and the kinds of science and science pedagogy needed to serve that role. Mach thought science should be pursued and taught only to the extent that it served the society's preexisting goals and self-understanding. Planck insisted instead that science education should enlist the whole society in the preexisting intellectual mission of science, initiated by Newton's *Principia*. For Mach (as for Wilhelm Ostwald), the most "fundamental" science was chemistry, which fulfilled the Promethean promise of reconstructing nature. For Planck, only physics could discover the most basic constituents and principles of nature. Fuller acknowledges, regretfully, that Planck decisively won this struggle for the soul of modern society. He sees Kuhn's book as consolidating Planck's victory, thereby foreclosing public and philosophical debate about the directions that science and science education should take.

How should we assess Fuller's revisionist interpretation of Kuhn and his impact? Fuller is illuminating and mostly correct about the intellectual and political genesis of *Structure*, but in his revisionist zeal he underestimates Kuhn's originality and insight. In particular, Kuhn's shift of focus from the retrospective assessment of knowledge to the prospective assessment of research is not given its due. Fuller's broader historical reconstruction is also informative. Current thinking about science has a mostly forgotten history, and Fuller instructively shows how others once saw issues differently. Many of Fuller's criticisms of recent philosophy, science studies, and social science are telling.

Nevertheless, I think his main thesis is fundamentally mistaken. Fuller seeks a comprehensive, democratic science policy, one that would shape the intellectual, pedagogical, and practical development of science by social norms independent of scientists' professional goals. He endorses Mach's vision of "scientific careers as self-sacrificing rather than self-perpetuating": scientific success should be measured by the ability to make society less dependent on further research. Yet this vision presumes a sharper separation between science and society than is now tenable. We live in a scientific culture. Materials, concepts, practices, and aspirations drawn from the sciences permeate our world and our self-understanding, while human society is not separable from its "natural" environment. "Society," as Fuller con-

ceives it, is an artificial, thin abstraction that overlooks the interconnectedness of a scientific-technological-material culture.

Another way to see Fuller's mistake is to recognize that both Planck and Mach were wrong. They agreed that the fundamental issue for the politics of science concerned how the unified guild interests of scientists relate to other social aspirations. But the sciences are neither unified nor separable from the wider society in responding to contemporary issues. Fuller's critique of the "political impotence" of recent science studies is tellingly silent about energy use and global warming, genetic intervention, the cultural meaning of evolution, the resurgence of infectious disease, the social transformations wrought by information technologies, or the dangers of nuclear and biological weaponry. Not surprisingly, he identifies "science studies" with the sociologists who accept Mach's and Planck's terms, rather than with the cultural historians, anthropologists, and feminist scholars who take us beyond them. Readers interested in the philosophy and politics of science will gain much from the detailed interpretations and arguments in this provocative book, even though Fuller is led astray in his own conclusions by a fundamental misconception of how to think about the intertwining of science and society.

BOOKS: MEDICINE

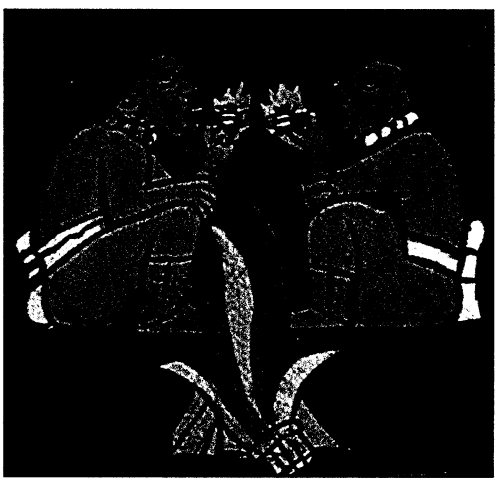
Cancer Research by Way of Metaphor

John Cairns

As a recognized authority on leukemia, Mel Greaves is comfortable in many disciplines, including molecular biology, immunology, pathology, therapeutics, and epidemiology. So he is well placed to review the whole field of cancer research. Additionally, he has a naturally fluent way of writing that carries the reader along with its uninhibited haste. Unfortunately, these skills cannot overcome the undisciplined presentation of thoughts in his book *Cancer*.

Greaves introduces readers to all the major topics in contemporary cancer research. They are told what is known about the causes and distinguishing features of the major forms of human cancer and of the experimental counterparts in other animals. If readers are left a little confused, that is partly the fault of the field, which at present is rather fragmented and incoherent. But it is also the fault of Greave's organization of his book. If there is a logic to the order of the chapters, I do not see it. The main sections are entitled (i) "Cancer: ancient legacies and modern myths"; (ii) "Evolving cancer"; (iii) "Paradox of progress: indecent exposures"; and (iv) "Finessing the clone." The book's general thesis is the widely held belief that cancers arise as the result of mutation and selection—in other words, through conventional Darwinian evolution operating at the level of somatic cells, rather than the germ line. But even knowing that, any scientist would be hard pressed to guess what the four sections are about. Scientists are trained to be direct and plainspoken; when we come up against unnecessarily flowery language and a profusion of metaphors, our first thought is that we are being conned. I can well believe, however, that nonscientists find things easier to understand when a certain amount of space-filling rumination is placed between major concepts, as a kind of referees' time out during which everyone can get another beer. So perhaps Greaves has judged his intended audience correctly.

Cancer
The Evolutionary Legacy
by Mel Greaves
Oxford University Press, Oxford, 2000. 288 pp. $27.50, £19.99. ISBN 0-19-262835-6.



Early smokers. This detail from Diego Rivera's *Las Mansiones de Xibalba* (1931) reflects temple reliefs from the Yucatan peninsula that show Mayans blowing smoke from a tube of rolled up palm leaves, reeds, or bamboo.

Nonetheless, I have to say that I found it hard to keep reading. Although I was told many things I did not know, most of them could not then be coupled with any of the references at the end of each section, nor could they be revisited later via the index. But what I found hardest to tolerate was the endless stream of metaphor. For example, the author's photograph of a leukemia cell with three copies of chromosome 8, the one color illustration in the book, is labeled "molecular mug shot of cancer cell," which is the sensational language of the popular press. And surely Greaves was being carelessly anthropomorphic when he says that expanding clones may find that "Machiavellian ploys may be advantageous"; this is the kind of lateral thinking that would have been excised by a knowledgeable copy editor.

Often the metaphors either obscure the meaning or actually introduce confusion. Even the title, *Cancer: The Evolutionary Legacy*, does not really convey what the author thinks. Cancer is hardly a "legacy" from the past, but simply the result of an unavoidable level of somatic mutation combined with the opportunity for natural selection of fitter, potentially cancerous variants. The only sense in which I can see cancer as a legacy is that natural selection decreed that lower rates of mutations were not cost-effective as long as life expectancy was only 40 years.

Like many clinicians and molecular biologists, Greaves tends to underplay the contribution of epidemiology. I had expected to find an account of Kinlen's brilliant demonstration that childhood leukemia is often the result of some kind of infection because it becomes much more common when children in isolated communities mingle with city children—as happened, for example, when London's children were evacuated to country towns during the war. Greaves never refers to Kinlen. And all he says, mysteriously on the penultimate page, about the link between infection and childhood leukemia is that he himself has a "vested interest in this idea" and craves the indulgence of his readers for mentioning it.

Reviewers are expected to show their conscientiousness by pointing out a few errors in the text. Here are some. Does any epidemiologist believe that the age-standardized incidence of cancer was 10-fold lower at the end of the 19th century? Would anyone these days say that *Mycobacterium tuberculosis* and streptococci were viruses? Since when was Bob Weinberg at Harvard, and when did the word "bacteria" count as a singular noun? I know that this is nit-picking, but I think these errors arose because Greaves's admirable ebullience is not coupled with much of an inclination to check what he has written (or to get others to check it). In the acknowledgments, he thanks someone for "deciphering [his] midnight scrawls." Perhaps that explains a lot.

The author is in the Clinical Trial Service Unit, Radcliffe Infirmary, Oxford OX2 6HE, UK. E-mail: j.cairns@ctsu.ox.ac.uk

Science's weekly Books Received list is now available online (see Books *et al.* at www.sciencemag.org).