A Case Study in Patronage

A Fragile Power. Scientists and the State. CHANDRA MUKERJI. Princeton University Press, Princeton, NJ, 1990. xiv, 253 pp. $24.95.

Chandra Mukerji, a sociologist at the University of California at San Diego, set out to study how technological innovation shaped the work worlds of her colleagues at the Scripps Institution of Oceanography. She found that this issue was of little interest to the ocean scientists she was interviewing. Despite their dependence on instruments and techniques, they were much more concerned with their scientific identities, with the character and reception of their research, and with raising the funds needed to sustain their investigations and reputations. She ended up focusing on the relationship between contemporary American scientists and their federal patrons.

Although this shift in orientation broadened her purview, Mukerji decided that her ongoing study of the oceanographers could shed much light on the scientists' relationship to the state. Oceanography is a particularly promising research site because the federal government provides much of its funding—for example, in 1986, 1900 university and college oceanographers averaged $145,000 per capita while 15,900 physicists and astronomers averaged $45,000. In fact, many academic ocean scientists depend upon federal funding for large fractions of their salaries. Hence, Mukerji argues, oceanography is an extreme case that highlights the principal features of the soft-money system that nurtures so much contemporary science. Following out this logic, she concentrated in her interviewing on scientists working in two expensive deep-ocean specialties—research related to seabed disposal of nuclear wastes, a field that was killed in 1986 when the Department of Energy finally abandoned it, and research on the geology and ecology of hydrothermal vents. She also analyzed several of the tapes made during exploration of the vents with the submersible *Alvin*.

Relying heavily on her interviews and the tapes for examples, Mukerji advances a provocative, and for the most part persuasive, picture of the symbiotic relationship between scientists and the state in contemporary America. In her view, no more than a minuscule fraction of federal patronage for science is motivated by curiosity about nature. Rather, the federal bureaucracies—in particular, NSF, NIH, and mission-oriented agencies and departments (such as NASA, DOE, and especially DOD)—fund academic scientists as a means of sustaining a cooperative reserve labor force of elite specialists who collectively possess the expertise and status needed to serve and legitimate the state's practical policies. In exchange for keeping their side of the bargain, funded scientists are provided with the wherewithal for their research. They do not, however, share the state's utilitarian orientation toward nature. Hence they are constantly seeking to maximize their control of the intellectual life of science. So long as they continue acquiring and refining what the bureaucrats regard as useful instruments and techniques, they are likely to enjoy substantial success. Indeed, their quest after autonomy is in itself useful, for it lends credibility and authority to their expert counsel. Mukerji's conclusion, therefore, is that federally funded academic scientists have paid dearly for their support and the considerable intellectual freedom in thinking about nature that has accompanied it—they have traded away control of the politically powerful voice of science.

In the course of her main argument, Mukerji develops many interesting auxiliary concepts and themes. My favorite is the idea that successful principal investigators and their laboratories have distinctive identities or "signatures" constituted by the unique, steadily evolving sets of tools that they use for expanding the domain of science. It is a straightforward extrapolation of this idea to suggest that specialties and disciplines also have signatures. I find the extended idea particularly congenial in the present context.

The typical historian would have gone about investigating oceanography as a case study in the relationship of scientists and the state in a very different way from that taken by Mukerji the sociologist. While the historian would have relied primarily on documents and publications for evidence, Mukerji relies primarily on interviews and the *Alvin* tapes. While the historian would have used the recent work on postwar science of Paul Forman, Daniel Kevles, Robert Seidel, Joan Bromberg, David DeVorkin, Robert Smith, and others to depict the historical context, Mukerji draws upon an older literature. While the historian would have recounted the case histories in some detail, Mukerji leaves the reader largely in the dark about the scientists involved, their institutional affiliations and patrons, the evolution of their instruments and techniques, and turning points in the research. But, while the historian would have provided a more textured and satisfying account of oceanography in the last two decades, Mukerji has been more systematic and daring in her exploration of the workings of soft-money science in contemporary America.

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Transformations in Physics

The Restructuring of Physical Science in Europe and the United States, 1945–1960. MICHELANGELO DE MARIA, MARIO GRILLI, and FABIO SEBASTIANI, Eds. World Scientific, Teaneck, NJ, 1989. xiv, 813 pp., illus. $86. From a conference, Rome, Italy, Sept. 1988.

As events in Europe signal a new turning point, historians are grappling with the impact of World War II upon culture and society. In few areas of human activity is this impact more controversial than in science. Physicists who have lived through the period and their younger colleagues who seek redefinition of their enterprise have become active participants in this struggle to understand what happened to their discipline as a result of its involvement in the war. In September 1988, the physics department at the University of Rome organized a conference on the restructuring of the physical sciences after World War II. Historians and physicists were invited from the United States, Japan, the Soviet Union, and Western Europe to comment upon the evolution of physics and astronomy after the war. Their papers are reproduced here much as they were given. One must sort out the dross without much assistance from the editors, who neither reproduce the discussions occurring at the conference nor provide guidance in their introduction.

The first set of papers deals with where the action was: with the transformation of physics in the United States after the war. Historian Daniel Kevles reflects in his paper that the postwar diversification of science was fed by and integrated with the technological demands of national security. This was especially true after the Korean War "generated a pervasive psychology of permanent mobilization" in American society. Lillian Hoddeson, who has recently written an as-yet-unpublished history of wartime Los Alamos, contends in her paper that the development of the implosion process for the atomic bomb there became the model for the research style of big science after the war. The first claim is unexceptionable; the second is not supported by an analysis of the research style in peacetime laboratories.

Other social scientists see the postwar

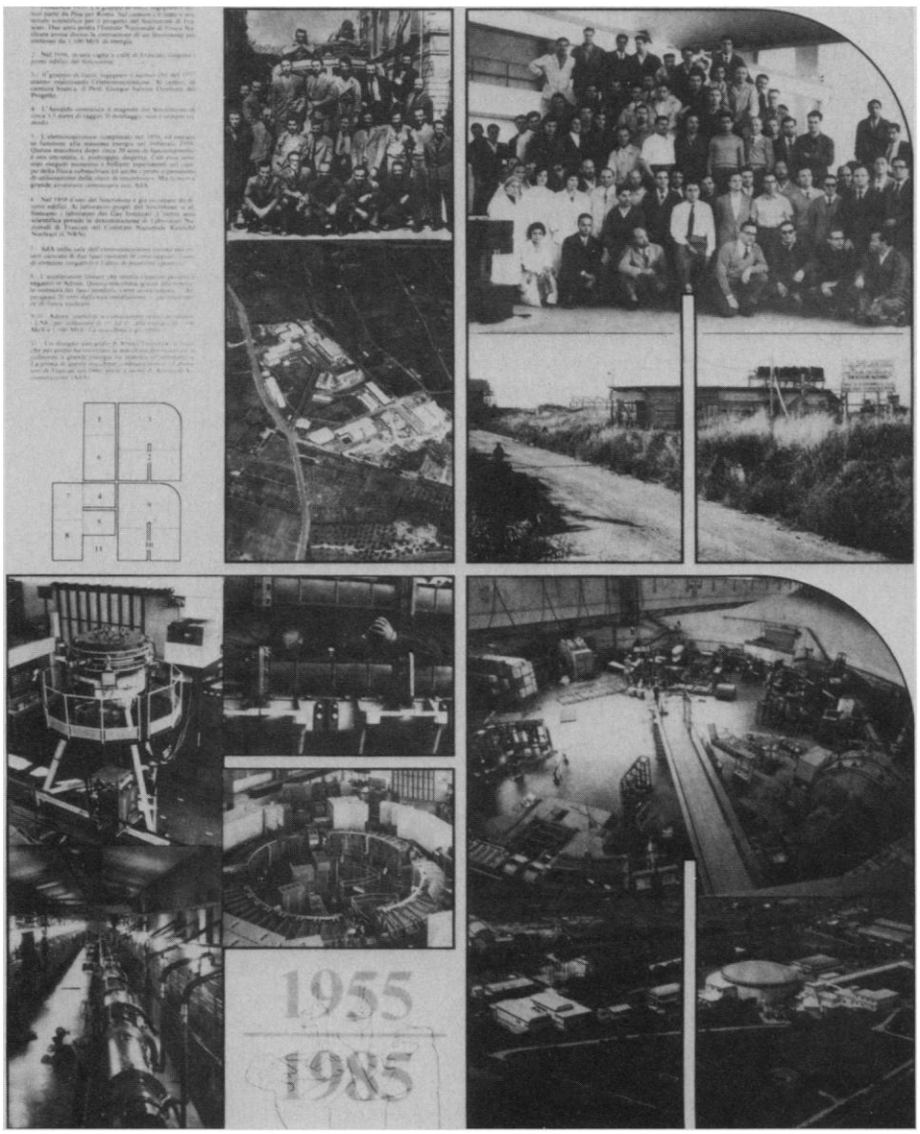
changes as a fall from grace for the physicists: Andrew Pickering has big science emerging from the wartime alliance between science and the military as a "form of life" incompatible with little science, giving rise to "a progressive alignment of postwar physics with the military [which] is a bad thing." Paul Forman argues that physicists have abandoned the claim to special moral standing exemplified by such prewar paragons as Robert Millikan and Arthur Compton for a view of physics as "fun" because, having lost control of the ends of their research, they seek an ersatz autonomy in research for its own sake.

Other conference participants introduce many fun-loving heroes: Lloyd Berkner, Luis Alvarez, and the builders of postwar institutions of physics and astronomy research. Do they have feet of clay, as Forman suggests? Certainly Alvarez had fun with physics, but he shared with Berkner a serious commitment to national security, as did Ernest Lawrence and many other members of the postwar generation of physicists. In other words, they freely adopted the ends of national security. The German hero, or antihero, was Heisenberg, whose contributions are variously appraised here. The consensus seems to be that, although he carried over anti-democratic mandarin practices from the Weimar and Nazi periods, he was unsuccessful, despite his considerable political skills, in achieving dominance of big science in Germany in the postwar era. Historians disagree, however, about the extent to which he concealed or misrepresented his role in the German atomic bomb project in order to retain his leadership of the discipline. These biographical studies illuminate the situations of the leaders of the profession but cannot adequately demonstrate Forman's hypothesis. Nor can they define the essential product of the postwar restructuring, big physics.

In a paper that most closely articulates the title of the conference but departs most strikingly from the hypothesis that the war made a great difference, Spencer Weart argues from demographic and institutional indices that solid-state physics enjoyed an exponential growth that was scarcely perturbed by the war. Though nuclear physics and electronics were more likely beneficiaries of wartime effort, Weart's analysis is a warning to those who take the war too seriously as a watershed in the history of science: science, especially physics, was on the way to becoming big science before the war.

Accounts of the postwar diffusion of big science to underdeveloped countries like Spain and Yugoslavia show that the effects there were less than their initiators hoped, given the American example. Physicist M. Mladjenovic's paper on physics in Yugoslavia concludes that that nation's big science establishments, having lost their original purposes in nuclear science, adopted diversification strategies. These are similar to those that some multipurpose national laboratories in the United States have adopted as their postwar missions have atrophied.

Physicist Wolfgang Panofsky's account of the Stanford Linear Accelerator Center (SLAC), a national laboratory specializing in high-energy physics, is grounded in a conception of technological and scientific causation. Although he does not admit that social and political factors have driven the growth of high-energy physics, Panofsky worries that they may stop the next stage, the construction of the Superconducting Super Collider. Other physicists' anecdotal and technical accounts of the laboratories and their research traditions in this volume tend to slight the political and social circumstances surrounding the restructuring of physics. This is not true, however, of Pestre's account of the emergence of the first international high-energy physics laboratory, the European Organization for Nuclear Research (CERN). Historian Lanfranco Belloni and physicist Constance Dilworth briefly describe its precursor, the multigroup collaboration in the G-stack balloon cosmic ray experiment, a last-ditch effort by cosmic ray observers to beat the accelerator physicists to an effective description of strange particles. Physicists who were participants in this and other cosmic-ray expeditions testify in other papers to their impor-



Poster for the 30th anniversary of the Frascati National Laboratories. The creation of the Laboratories "was an important moment in postwar Italian physics: first the electrosynchrotron (1955–59) was built, then the first electron-positron storage ring AdA (1960–61) and finally the storage ring Adone (1961–69)." [From *The Restructuring of the Physical Sciences*; courtesy F. Sebastiani]

tance in shaping a labor-intensive big physics.

Besides big accelerators, laboratories, and collaborations, big physics required the development of detectors that went beyond the emulsions, counters, and cloud chambers available at the beginning of the period. Seymour Lindenbaum's contribution relates the rivalry between the counter and chamber traditions from the counter perspective; Marcello Cresti's paper provides a very clear account of the development of cloud and bubble chambers; D. H. Perkins's contribution on pion physics traces the evolution of emulsions; and G. Fidecaro's account articulates the counter tradition at Rome and CERN.

Insofar as the physicists' accounts illuminate the issues posed for and by historians in the conference, they enrich our understanding of the restructuring of their enterprise through revealing the fine structure of big science. Participants' accounts cannot, however, resolve the spectrum of change implied in the title, and its resolution requires more detail about more of the institutions and actors in the process than historians' studies have yet provided. John Heilbron's discussion of the discovery of the anti-proton indicates one approach to historical resolution. The anti-proton was discovered through the tools supplied by the restructuring of physics, and Heilbron both tells why those tools were supplied and dissects the ethical, legal, and political issues implicated in the discovery. Many case studies like this will be required to characterize properly the endeavor physics has become. This volume reflects work in progress rather than a refined understanding of its subject.

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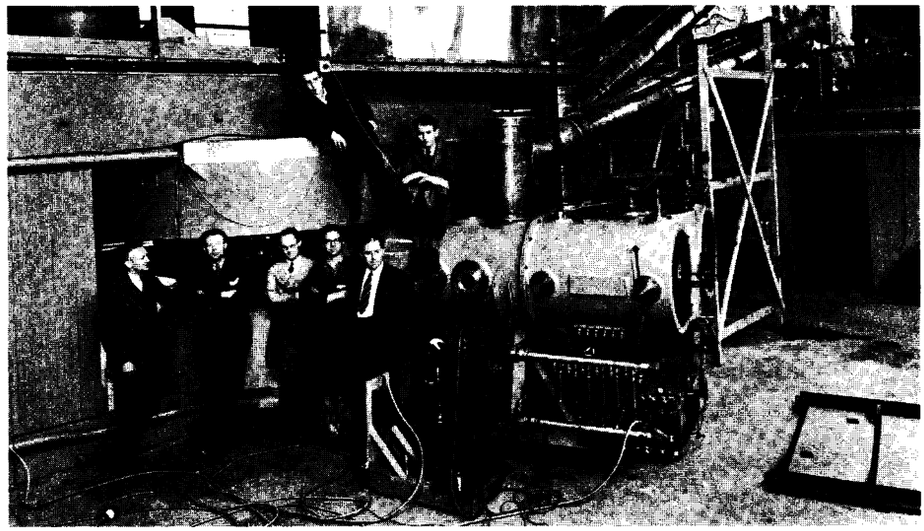
The Cyclotroneers

Lawrence and His Laboratory. A History of the Lawrence Berkeley Laboratory, vol. 1. JOHN L. HEILBRON and ROBERT W. SEIDEL. University of California Press, Berkeley, 1990. xxviii, 586 pp., illus., + plates. $29.95. California Studies in the History of Science.

The Radiation Laboratory set up by the young Ernest Orlando Lawrence at Berkeley in the decade preceding Pearl Harbor was the "Mecca" of cyclotrons before the war, that of the accelerator expertise after it. This thick book, well written and well documented, tells the reader all he or she would like to know about the Lab's early years, whether it be the nature of the California milieu that enabled it to become the land of



Lawrence Laboratory staff "lolling around the poles and dee supports of the 60-inch cyclotron." Left to right above, Luis Alvarez, Edwin M. McMillan; left to right below, Donald Cooksey, Lawrence, Robert Thornton, John Backus, Winfield Salisbury. [From *Lawrence and His Laboratory*; Lawrence Berkeley Laboratory]

cyclotrons or the exact amount of money the determined Lawrence got annually for his machines and his "boys."

As the title indicates, the book is above all a history of the laboratory. It sets the intellectual and material scene for "the invention of the laboratory," it describes Berkeley's technological achievements and research programs, and it shows the entry into war work. But the authors offer much more. They provide, *inter alia*, a new history of nuclear experimental physics, beginning with the work on nuclear disintegrations by Cockcroft and Walton in 1932 and carried through Chadwick's hypothesis of the neutron, Joliot's discovery of artificial radioactivity, and Fermi's demonstrations of the importance of slow neutrons up to fission studies and the discovery of plutonium by Seaborg.

The book similarly offers a worldwide "techno-social" history of x-ray–producing devices, of high-tension machines, and of course of cyclotrons. Chapter 6, for example, is a thorough description of "American cyclotronics" and chapter 7 a presentation of developments in Europe and Japan. Here the reader will find the best study available of what attitudes in Europe were toward accelerators (why did Europeans remain faithful to high-tension machines up to very late in the '30s?); on the differences between British and Continental ways of handling things (even if the authors seem too hard on British industry); on the help provided by American foundations in the spread of the cyclotron art (combined with the generosity of Lawrence himself, which contributed to his being awarded the Nobel); on the fact, too, that the only way to get such a complex device as a cyclotron to work is to participate in building it with someone who has already succeeded (a point previously stressed by Collins with regard to lasers).

Finally, the book is a study of power games played by scientists—among themselves and in their dealings with politicians, industrialists, and the press. It presents case studies of what the words "science policy" concretely mean, and it offers analyses that could be categorized as microsociology of scientific practice. In a chapter entitled "Cast of characters" the reader will even find something more anthropological in nature: a description of the daily social relations in the Rad Lab, for example (they look definitely "American" to someone having worked on European physics); of the racial prejudices of Berkeley people (rather marked, notably vis-à-vis Jews); of the political behavior and cultural claims of Lawrence and his "boys" (when those claims are made at all); and of the culture shocks experienced by the Europeans arriving in Berkeley (the least able to cope with such a strange crowd of frantic machine freaks was Maurice Nahmias, the emissary sent by Joliot to learn the Berkeley art, whose recollections are extensively quoted).

The strength of the book lies in its success in interweaving all these stories and in the quality of the sources used (roughly two-thirds of the items cited in the notes are private letters). History is revealed as made by human beings, all different, often unpredictable in their reactions. There is no ideal Comtean science in this book, no ideas floating in the air, but men, men who simultaneously think, tinker, and fight for ideas and power over one another and over nature, men who try to convince others that they are right, men with habits, idiosyncra-