Book Reviews

Escalations

Big Science. The Growth of Large-Scale Research. PETER GALISON and BRUCE HEVLY, Eds. Stanford University Press, Stanford, CA. 1992. xiv, 392 pp. illus. $45. Based on a workshop, Stanford, CA, 1988.

In the 1960s, the quantitative historical studies of Derek de Solla Price and the policy-oriented writings of Alvin Weinberg helped to delineate a new historical phenomenon: big science. During the 1970s and especially the 1980s an increasing number of historians of science turned their attention to this beast, seeking to trace out its origins and characteristics, and in 1988 many of the leading researchers came together in an NSF-supported workshop held at Stanford University. This book is the result. Most of the contributors have already written or are writing books of their own, so *Big Science* often reads like a sampler of accounts and ideas worked out at greater length elsewhere, but this is more a strength than a weakness. *Big Science* will serve for some time, I think, as a broad introduction to this important area of scholarship for students, academics in other fields, policy-makers, and the curious in general.

So, what is this thing called big science? The authors and editors are at pains to insist that no single definition will do, but as a first approximation one can say that a science becomes big science when its funding levels become non-neligible on the scale of advanced national economics. During World War II, physics was the first of the sciences to make this transition, though different branches made the transition in different ways, and is thus at the center of attention here. The two great contributions to the war effort lay in the construction of nuclear weapons—Lillian Hoddeson analyzes the Los Alamos implosion program of 1943 to 1945—and the development of microwave radar techniques. Postwar, the first area of work was translated into high-energy physics, a relatively small field that earned the label of big science by virtue of the great expense of constructing particle accelerators; the radar line evolved into a much wider field devoted to research and development concerned with electronic devices, a big science by virtue of the sheer scale at which it was supported.

Though high-energy physics has claimed the lion's share of historians' attention until recently, several of the essays in *Big Science* dissect parts of the hi-tech electronics line, and their findings are particularly striking. We tend to think of "science" as some more or less autonomous pursuit conducted by academics for its own sake, but these essays make clear just how inappropriate this image can be. The authors tend to speak of the "constraints" imposed upon research by its "context," meaning the industrial and military sources that alone can guarantee massive funding. But such formulations are too weak. That a remarkable lack of autonomy has always characterized the physics of electronic devices is clear from Peter Galison, Bruce Hevly, and Rebecca Lowen's account of the growth of physics research at Stanford University in the period from 1935 to 1962. Even before World War II, the Stanford physicists did all they could to integrate themselves into the circuits of industry via an emphasis on the construction and development of useful devices. When a 1935 plan to construct a 100-foot x-ray tube failed to attract sponsors, the group switched their attention to William Hansen's "rhumbatron" as an alternative x-ray source and then, in 1937, to the Varian brothers' microwave klystron. Here, at last, they succeeded in enrolling industry, with Sperry offering royalties to the inventors and the university, as well as research funds for further development, in return for a manufacturing license.

With the coming of World War II, the military displaced industry as the principal sponsor of physical research, at greatly increased funding levels. At the cessation of hostilities, funding began to fall but, as Daniel Kevles discusses in his paper "Korea, science, and the state," quickly returned to wartime levels in the Cold War. This is the context for the continuing analysis of science at Stanford in an essay on "Science regions and the organization of research and development" by Robert Kargon, Stuart Leslie, and Erica Schoenberger. These authors' interest is in the emergence of Silicon Valley, and their documentation of the integration of Stanford into the military-industrial complex is impressive. Beyond a continuing emphasis on useful technique—exemplified in the Stanford Electronics Laboratories' focus on the traveling-wave tube—this essay explores many other axes of penetration of the university by industry, especially the aerospace industry. Faculty were encouraged to act as consultants for industry, corporate engineers were brought to campus to keep the academics up to date on their interests, industrial engineers were recruited as Stanford faculty (sometimes with industry paying the bills), various programs were set up wherein industrial subscribers were offered a kind of panoptic access to scientific and engineering research at Stanford, and so on. (Models for most of these moves originated in the Massachusetts Institute of Technology, discussed in less detail by S. S. Schweber in his very instructive comparative discussion of "Big science in context: Cornell and MIT.") The basic image that emerges from these studies is, then, that of academic big science as an integral part of wider industrial and military endeavors in the postwar United States.

At this point, two questions come up. First, does it make sense to try to treat big science as the kind of largely self-contained topic appropriate to historical study? I think the answer is no. In the end, the way forward in our understanding must come from studying "content" and "context" together as a unitary academic-military-industrial whole. Exemplary works in this respect, by two authors not represented in this collection, are David Noble's *Forces of Production: A Social History of Industrial Automation* (Oxford University Press, 1986) and Paul Forman's "Behind quantum electronics: national security as basis for physical research in the United States, 1940–1960" (*Historical Studies in the Physical and Biological Sciences* **18**, 149–229 [1987]). The second question is whether there is any difference at all between some branches of academic science and in-house industrial research. David Hounshell's essay "Du Pont and the management of large-scale research and development" suggests that there is but that it is a difference in degree of hierarchical control rather than anything more fundamental. Du Pont hired Wallace Carothers in 1928 to do chemical research with "no strings" other than the specification of an area: polymerization. Nevertheless, when Carothers's group arrived at a synthetic rubber and a first synthetic fiber in 1930, Du Pont was in a position to immediately organize development work on the former and to "push Carothers to do more" with the latter. In 1934, Carothers was "strongly encouraged" to return to the exploration of synthetic fibers and, when this encouragement quickly paid off with the forerunner of nylon, his group was directed toward development work, "perhaps much against Carothers's own intentions." The difference between academic and industrial research per se, then, appears to be that within industry research can be limited to short loops away from immediate commercial returns, whereas academics, subject to less direct and detailed control from their sponsors, can allow themselves longer loops.

A similar comment applies to the other line of big-science development in physics, the accelerator and particle-physics line. As Robert Seidel makes clear in his essay "The origins of the Lawrence Berkeley Laboratory," E. O. Lawrence's prewar development of a sequence of ever-larger cyclotrons was strongly coupled to the medical interests of his sponsors, and particle-physics research at Berkeley was parasitic upon accelerator development and medical uses. After the war, though, the particle physicists succeeded in extending their research loops seemingly indefinitely. The clearest instance of this is in the founding of CERN, the European organization for particle physics, in the early 1950s. As the CERN historians Dominique Pestre and John Krige argue, the attraction of CERN for its sponsoring governments was precisely that the research to be conducted there would, by design, escape the circuits of industry and the military. This was what made CERN a potential site for and symbol of postwar European unity and cooperation, and this essentially political consideration made possible the birth of a "pure," relatively decoupled, big science. In the United States, accelerator-based physics was in general much slower to attain similar autonomy, in a process that remains to be clearly elucidated, though Schweber's comparison of MIT and Cornell is illuminating.

The coming of this kind of autonomous big science, symbolized by particle physics, has taken place within a new and distinctively postwar regime of science politics and science administration. In this area, *Big Science* includes contributions by Sharon Traweek on "Big science and colonialist discourse: building high-energy physics in Japan," Robert Smith on the academic coalition-building behind the Hubble Space Telescope, and W. K. H. Panofsky on "SLAC and big science" (back to Stanford again). Allan Needell offers some fascinating insights into the biography of Lloyd Berkner, one of the first great postwar science administrators, and his movement between the project of further integrating science into the military-industrial complex and that of fostering the new pure big science. And C. W. F. Everitt's insightful account of the Gravity Probe B test of general relativity—37 years from first proposal to planned launch in 1997—includes some wonderful first-hand reports on the mysteries and brute contingencies of federal science politics. "I, from a distance of 30 feet, have witnessed an exchange between two Congressional staffpersons lasting twenty seconds that added $1 million to our budget authorization," he recalls. "Likewise, from a wall seat at a Space Science Board meeting I have heard two sentences from different speakers, one calm, one impassioned, so transform a debate that a straw vote of 8 to 6 against a report . . . was followed an hour later by an almost unanimous vote of approval."

Though this last group of essays is interesting and of immediate relevance to policy-makers, I think that *Big Science* loses its wider audience here. To put it bluntly, to get to grips with the "new world order" of the late 20th century it is the performativity of Silicon Valley that we need to worry about, not the politics of the Superconducting Super Collider.

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Vignette: An Argument about Determinism

*In 1574 Tycho Brahe delivered the first of a series of invited lectures at the University of Copenhagen. "The result," writes Victor Thoren in a recent biography of Tycho (*The Lord of Uraniborg, *Cambridge University Press), "was about what one expects to get when a scientist waxes philosophical or historical":*

Having established the positive aspects of his intellectual position [the utility of astronomy as an empirical science] Tycho moved to refute the various criticisms of astrology. No one could deny that plagues and wars killed off large numbers of people who had different horoscopes, but any responsible astrologer would leave room in his predictions for the possibility of general calamities that had nothing to do with the specific fate of the individual. Nor did the fact that people could be born at the same instant but meet different ends discredit astrology, for the stars did not determine the basic circumstances of life but, rather, produced the variations that distinguished the fates of people who lived in the same basic circumstances. Twins, who shared both horoscope and circumstances, were actually born at slightly different times, and one was always weaker than the other. Most important was that astral influences were influences, not determinants. . . . Thus the ancient objection that prognostications were not even desirable, as they merely diluted the joy of happy events and added worry to the grief of sad events, was forestalled by the possibility of resisting the influences working to produce undesired situations.

Conservation Realities

Neotropical Wildlife Use and Conservation. JOHN G. ROBINSON and KENT H. REDFORD, Eds. University of Chicago Press, Chicago, 1991. xviii, 520 pp., illus. $62; paper, $28.

"It is perhaps brazen to link the words conservation and use, as we have done in the title of this book," write Robinson and Redford, "but it is our opinion that wildlife has been, is, and will always be used by people, and those of us who advocate the conservation of wild species and biological communities must incorporate that use into our conservation strategies." On this premise they and over 40 other authors consider whether and how the large-bodied animal species of the neotropics can be managed to yield economic, scientific, aesthetic, and other benefits.

Patterns of use by native peoples and subsistence hunters are examined. Basically, people eat anything large enough to hunt and will hunt any species having utility or value, whether for leather, wool, feathers, venom, or adornment. From 1976 to 1979, 21.5 million mammals were legally exported from Argentina. In Amazonas, Brazil, rural hunters kill about 3.5 million vertebrates annually. It is doubtful that species with strong economic, ecological, or societal importance can, or should, be relegated to zoolike preserves. In any case, absolute protection in small reserves may not suffice to ensure continuity for many populations.

James Shaw notes that in 1900 the