

prejudices and traditions that had been shaped by local economic and political factors. *An American Dilemma* sought to resolve these many contradictions by delineating the irrational bases of racial prejudices, documenting the scientific evidence that refuted claims about inherent racial differences, and explicating findings of the deleterious effects of prejudices, discrimination, and racial oppression on the African-American community. Myrdal argued that a "vicious circle" of racial prejudice, stereotypes, poverty, and prejudice generated a cumulative process. And he warned consistently of the dangers of neglect, of the growth of an alienated underclass. But he concluded that the power of the American creed in popular culture and political life was sufficient to halt this process.

Jackson chronicles the popular and scholarly reception of *An American Dilemma*, its utility in providing a popular education on the scope and complexity of racial discrimination, its influence on the direction of scholarly research, and its role as the key text in the new liberal orthodoxy about civil rights. In penetrating analysis, he also discusses the important silences in this work: the absence of a focus on African-American culture, institutions, or communities; the assumption that assimilation rather than cultural pluralism was the democratic solution to racial oppression; the failure to analyze how to address persistent poverty, de facto segregation, or racial polarization. In all of these senses, Myrdal was no Tocqueville—who had been deeply pessimistic about the intractability of racism. In his subsequent work and writing, Myrdal heightened his emphasis on the structural sources of racial oppression and began to appreciate the tremendous obstacles to the development of a social democratic movement across racial lines in America. His service in the Swedish cabinet and his important work heading the United Nations Economic Commission for Europe and supporting east-west trade frequently pitted him against the Cold War superpower that had provided his second home. And his writings on Third World development and opposition to the Vietnam War further strained relations with U.S. government elites. Myrdal died in 1987, his sequel to *An American Dilemma* unfinished. He had hoped to restore to Americans a moral concern with racial inequality. However pessimistic we may be about the current racial crisis, this important biography draws us back to the power of ideas and to the crucial role of an optimism of the will in movements for social change.

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Physics in Geneva

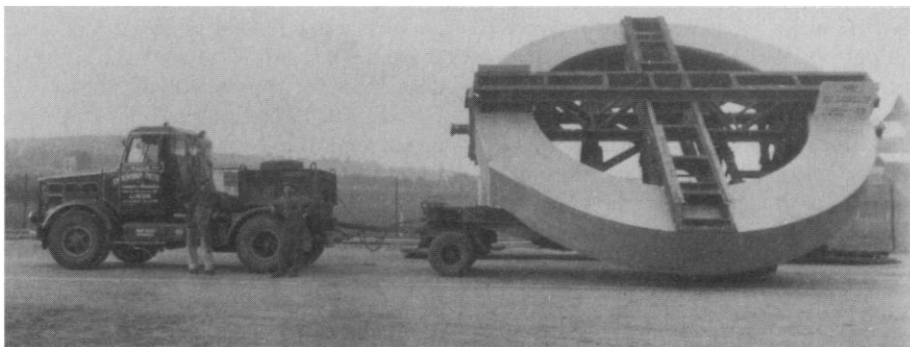
History of CERN. Vol. 2, Building and Running the Laboratory, 1954-1965. ARMIN HERMANN, JOHN KRIGE, ULRIKE MERSITS, and DOMINIQUE PESTRE, with a contribution by Laura Weiss. North-Holland (Elsevier), New York, 1990. xxii, 880 pp., illus., + plates. \$138.50.

This second volume by the "Study Team for CERN History"—an international group of European historians and physicists that formed to write the history of the Geneva-based international high energy laboratory—makes a substantial contribution to the scholarly literature on big science since World War II. The first volume focused on the politics of founding the laboratory. This one touches all major facets of CERN's development in the 1950s and '60s, uncovering a multitude of cultural, scientific, technological, and sociological themes relating to the building of the accelerators and detectors, the organization of the large-scale collaborative research effort, the administrative and financial program, and the negotiation of contracts with industry. Special attention is given to the relationship of CERN to the states that were members of the enterprise and to outside countries, particularly the United States. Each chapter stands alone, reflecting the interests, experience, and style of its author. Thus, some accounts are presented in the more analytic and often provocative tone of the historian (Krige and Pestre) and others in the more matter-of-fact voice of the physicist (Mersits and Weiss). Broader in focus than the other chapters is Hermann's social and institutional essay connecting CERN to the larger field of high energy physics in the '50s and '60s.

Volume 2 begins at the end of 1954, with the replacement of the original Conseil Européen de la Recherche Nucléaire by the English-named European Organization for Nuclear Research. It recounts the building of CERN's first two accelerators—the 600-MeV Synchro-cyclotron (SC, first beam in 1957) and 28-GeV Proton Synchrotron

(PS, first orbit in 1959)—and the early years of research with these machines and continues through the launching in late 1965 of the Intersecting Storage Rings (ISR) and 300-GeV Proton Synchrotron (SPS) projects. Two themes are interwoven: how the inexperienced CERN physicists learned to conduct big science and how they struggled to catch up with the Americans, having to experiment in the '50s and '60s, as Pestre explains, "with very heavy equipment on the scale of that in the best American laboratories, but without having the benefit of a tradition, without having transited through medium-sized devices or intermediary energies." Although American laboratories were a "source of expertise," CERN could not simply borrow from them, because of its need to be, above all, "reliable and accessible to all countries." This need, Mersits proposes, bound the laboratory by "a kind of conservatism hindering spontaneous developments. . . . The need of balancing national interests combined with a lack of experience on this terrain, resulted in a tendency to a rather heavy committee structure and meant being overcautious in commitments to very new ideas." The committees slowed down decision-making; CERN tried "to proceed by the most precise and secure way, which was often far from being the most efficient and quick one." Initially, CERN physicists did not worry about their detectors being "late." But by the time the PS was under construction, Krige points out, the importance of being "ready in time" was all too obvious. Victor Weisskopf, CERN's fourth director-general, a veteran of wartime Los Alamos, where time was perhaps the most pressing constraint, reflected, "It is no good in this field to be excellent and always late."

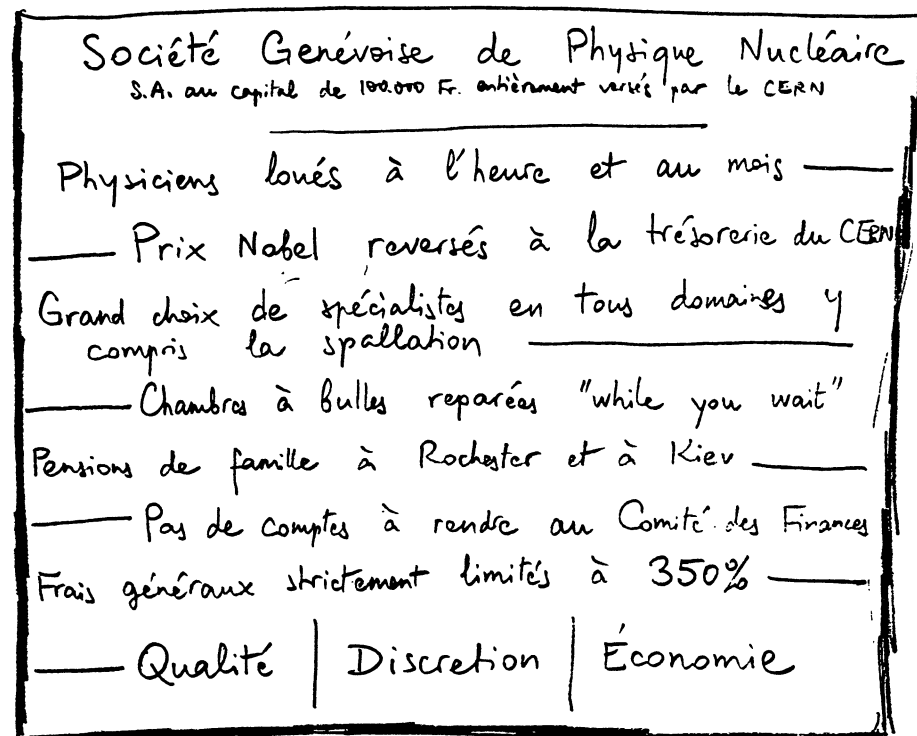
The authors zero in on many specific issues of collaborative research in the international high energy physics laboratory. One was "whether or not the builders of the detectors should have particular advantages when it came to using them." Pestre explains



"The Synchro-cyclotron magnet coil on its way to Geneva, December 1955." [From *History of CERN*, vol. 2; CERN photo]

that "this key question of 1959 was 'resolved' in practice: *de facto*, some priority was always given to those who had constructed big equipment. This was never formalized or defined as a right but no one ever openly disputed it." CERN was one of the first laboratories to confront the "visitor problem," the conflict between inside laboratory and outside "user" groups. By the early '60s, Pestre asserts, senior physicists trying to preserve their earlier "certain way of working" in which they had "functioned rather like an aristocracy," were shocked to find themselves "under attack" from bubble chamber specialists, counter groups, visiting researchers, and even the directorate. Eventually the tensions were absorbed by "the idea of CERN as a 'central' laboratory available to all." At this point the balance between inside and outside groups began to shift. By the mid-'60s, "the house-physicists in fact represented little more than 20–25% of the experimenters working at Geneva." By the '80s, "CERN physicists were reduced to 4–5% of the total, and were simply an auxiliary force."

High energy physicists tend to measure the success of their laboratories only by major discoveries, an attitude that has led to the sacrifice of in-depth studies in favor of the more glamorous pushing of frontiers. From this distorted perspective, CERN seriously lagged behind American laboratories in the early '60s, for example missing the discovery of the two types of neutrinos (Brookhaven, 1962), the Ω^- particle (Brookhaven, 1963), and violation of CP (or equivalently, time-reversal) invariance (Brookhaven, 1964). To the long list of explanations for why CERN lagged in that period (a list that includes inexperience, lack of equipment and qualified experimentalists, conservatism, and the committee problem), Pestre and Krige add a bold new interpretation: a greater mismatch in Europe between the engineers' desire for perfection and the physicists' more urgent need "to have an 'imperfect' piece of equipment ready at the right moment than a 'perfect' one ready when the dust of the battle had settled." In the United States, Pestre and Krige claim, the gap between physicist and engineer had closed some time between the '30s and '60s by the emergence of a "profound symbiosis previously unknown in basic science," involving "a new kind of researcher," at once a "physicist, i.e., in touch with the evolution of the discipline and its key theoretical and experimental issues," a "conceiver of apparatus and engineer, i.e., knowledgeable and innovative in the most advanced techniques," and an "entrepreneur, i.e., capable of raising large sums of money [and] of mobilizing several kinds of human, financial,



"A facetious 'advertisement' devised by Lew Kowarski during a meeting of the CERN Management Committee in January 1961." [From *History of CERN*, vol. 2; CERN archives]

and technical resources." Strong American laboratory leaders, such as Ernest Lawrence, Luis Alvarez, Edward Lofgren, Edwin McMillan, Wolfgang Panofsky, and Robert R. Wilson, "who imposed their rhythm on world science," shared a characteristic "pragmatic and utilitarian approach notable for its clear stress on 'getting numbers out.'" Studies of American laboratories, such as the team histories of Los Alamos and Fermilab, now in preparation, explain how this "symbiosis" was fostered by the American empirical tradition, institutional structures, and military pressures. They also document the transplantation of this American approach from military big science to high energy laboratories during the '60s and '70s, supporting the CERN group's hypothesis.

The appearance of this volume marks a point at which the pioneering efforts of the 1980s to employ team research in historical studies of large-scale scientific subjects can begin to be evaluated. The CERN Study Team's achievement underscores the power of the collaborative approach in writing well-documented, deep histories of complex developments over decade-long spans of time. The only serious difficulty concerns presentation. The group early on made the reasonable decision to publish the work as a collection of individually authored papers, thus allowing the younger scholars on the team to establish themselves. But the separate voices do not always harmonize, and the reader must struggle to extract the themes.

Rewriting the studies in one voice would have yielded a more elegant and readable history, but in this case the multiauthored presentation effectively mirrors the extent to which the committee approach dominates the enterprise under examination.

This volume is a basic text for historians of big science, large laboratories, particle physics, and particle accelerators. It will be of cultural and intellectual interest to "big" scientists eager to grasp the underpinnings of their cultural heritage, as well as to "small" scientists still flourishing in research marked by individual identity and freedom of action apart from committees.

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Some Other Books of Interest

A Statistical Model. Frederick Mosteller's Contributions to Statistics, Science, and Public Policy. S. E. FIENBERG, D. C. HOAGLIN, W. H. KRUSKAL, and J. M. TANUR, Eds. With the collaboration of Cleo Youtz. Springer-Verlag, New York, 1990. xviii, 283 pp., illus. \$39. Springer Series in Statistics.

The statistician Frederick Mosteller achieved his 70th birthday in 1986, and to mark the occasion his colleagues, with apologies for the delay, have now produced this