BOOK REVIEWS

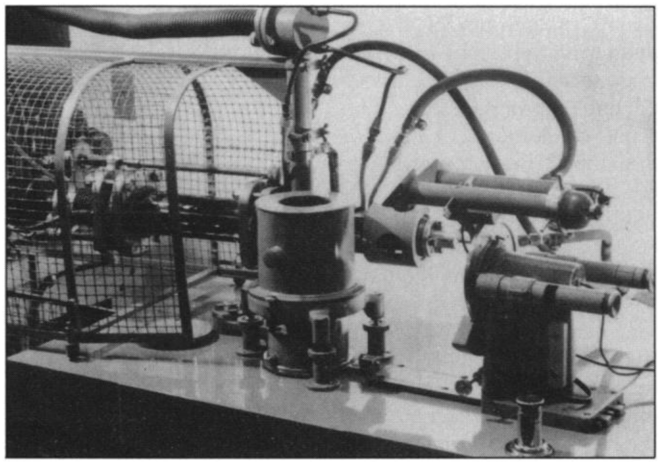
Biology and Broader Agendas

The Molecular Vision of Life. Caltech, the Rockefeller Foundation, and the Rise of the New Biology. LILY E. KAY. Oxford University Press, New York, 1993. x, 304 pp., illus. $49.95. Monographs on the History and Philosophy of Biology.

Who today needs telling that there is a glorious future just around the corner in which we will be able to control what it is that controls us, our genomes or genetic blueprints? Today it is not just talk of Mendelian factors, or even of genes, but of DNA sequences. This molecularization of genetics is giving mankind tools for therapy at the "wellspring" of human nature. If, indeed, what we are is spelled out by our DNA, the prescription seems straightforward enough. Once upon a time there was a subject called "eugenics," which Francis Galton defined as the study of those processes under social control by which the nature of man can be improved. The study of the distribution of hereditary characters in a population, Galton believed, gave man the grounds for embarking on a policy of selective breeding to improve the quality of future generations. Eighty years on, Joshua Lederberg envisioned that the "ultimate application of molecular biology would be in the direct control of nucleotide sequences in human chromosomes, coupled with recognition, selection and integration of the desired genes," and added that "these notions of a future eugenics are, I think, the popular view of the distant role of molecular biology in human evolution." True, the compulsion of the old authoritarian eugenics is absent from what Daniel Kevles in 1986 christened "reform eugenics," but it is disturbing to read in *The Molecular Vision of Life* of the suggestion of Linus Pauling that possessors of the sickle-cell gene be marked with a symbol tattooed on their foreheads.

The thesis of Kay's book is that it was the fundamental aim of social control that lay behind the Rockefeller Foundation's funding of molecular biology and the California Institute of Technology's trustees' support of that institution's expansion into genetics and structural chemistry. The Rockefeller philanthropies supported specific projects in the early phase of eugenics—for example, the sterilization campaign of the National Committee of Mental Hygiene and programs of research into social problems conducted by the Bureau of Social Hygiene—but they did not establish their own eugenic programs, thus avoiding duplication of the work of the Carnegie Institution. Following the carnage of World War I and the taint of destructive powers that settled upon the physical sciences, Rockefeller policy turned toward the sciences of biology and psychology. In the 1920s the foundation supported the psychologist Robert Yerkes's researches in primatology as promising a basis on which to develop a program of "human engineering." This term reflects the popularity at the time of attempts like Frederick Taylor's to solve problems in the workplace by science, and the focus on man was the outcome of the diagnosis of social ills as due to "cultural lag." This commitment to the human sciences found specific emphasis in 1934 when Max Mason introduced a program of "psychobiology." It was a modified version of what the zoologist Frank Lillie had suggested to the foundation 10 years before under the title "racial biology," but the ill repute that eugenics and racial biology had since acquired necessitated a change of label.

Mason brought in his former student Warren Weaver to head the program. After he attended the 1934 conference of the Social Sciences Research Council on human relations, Weaver's prognosis for a successful science of man was to make these sciences more professional and scientific. This, he concluded, could best be done by concentrating on the study of life in an interdisciplinary manner at the subcellular level. This was the direction in which to go to achieve the long-term aim of building a "foundation for the understanding and rationalization of human behavior," declared Weaver in 1935.



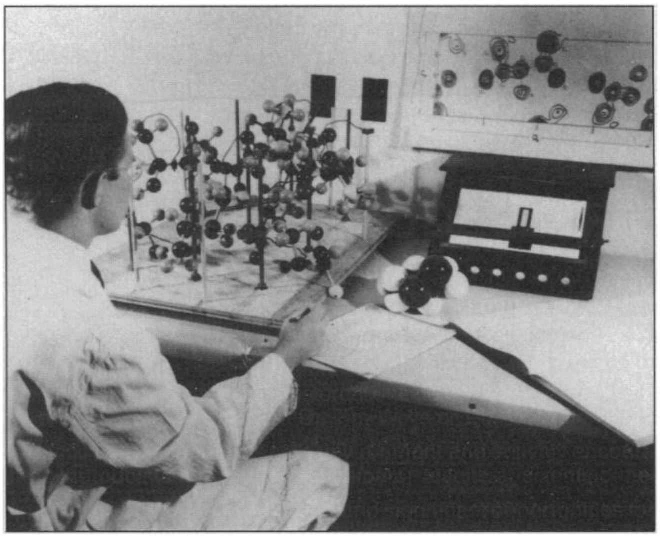
"Caltech's x-ray diffraction apparatus for studying the crystal structure of amino acids and peptides, ca. 1939." At Caltech "up until 1938, x-ray and other physicochemical apparatus had been built in the astrophysics shop, but the backlog of astrophysical apparatus impeded the rate of construction. . . . With [Warren] Weaver's approval, a fully equipped chemistry shop was set up, substantially accelerating the x-ray program." [From *The Molecular Vision of Life*; courtesy of the Rockefeller Archive Center]

It is unnecessary to repeat here the evidence in support of the claim that behind research in recombinant DNA technology and the application of its results to humans there has been the belief in the power of this science to solve many of the social and political problems in society today through manipulation of the genetic material. As the author reminds us, for example, such views were clearly articulated at the Ciba Foundation symposium on "Man and His Future" in 1963. Moreover, Kay comes to the conclusion that the existence of these long-term goals in the Rockefeller Foundation's program did not amount to a Machiavellian subversion and co-optation of academia. Rather, cultural hegemony was achieved "through the explicit and tacit constitutive processes of consensus formation." And though there existed a formal separation between the heads of business corporations and the foundation to which they gave rise, there was no separation, claims Kay, at the level of underlying purpose. Both were fueled by "Protestant values and technocratic visions." They wanted science that could be applied, not science for its own sake, but they were prepared to be patient, not demanding quick results.

Kay places alongside her account of the policy of the Rockefeller Foundation a perceptive analysis of the forces that shaped the establishment and expansion of the California Institute of Technology. Racial views were endemic in turn-of-the-century California, and eugenics flourished—indeed, the Presbyterian minister Robert Freeman administered it in undiluted form from his pulpit in Pasadena. Meanwhile, Caltech's trustee Harry Chandler was busy diverting the Owens River and ruining the small-scale farming that depended upon it while guaranteeing Los Angeles the water it needed to expand its borders. Along with Robert Millikan, Chandler also belonged to California's eugenic Human Betterment Foundation. In this context of the industrialization and eugenic "cleansing" of the western seaboard Caltech became the spearhead of the movement in the West for progress by technology and science. In 1927 T. H. Morgan, luminary of the eastern seaboard, was attracted to Pasadena to head the new biology division. There, with Linus Pauling heading the chemistry division, Caltech became the favorite recipient of funds from Weaver's program for what in 1937 he was to call "molecular biology."

The traditional view of the rise of molecular biology has been to attribute it to the inherent momentum of the reductionist program whereby the exact sciences have invaded and appropriated the preserves of biology. Long ago we dispatched that contender to the realm of the living, the vital force. Organic chemistry ceased to be known as the branch of chemistry confined to the products of living systems that could not be made in the laboratory. Yet that magic substance of the cell—protoplasm—still hid the secrets of life from us. Then came colloid science, introducing a special state—the colloidal state—and special laws covering it. Protoplasm existed in this state and behaved in accordance with its laws. When the ultracentrifuge was invented to separate out these colloids and measure them, many were found to behave like true molecular compounds, albeit extraordinarily large ones. These studies allied with the growing field of polymer science gave rise to macromolecular chemistry. One group of such compounds—the proteins—possessed many subtle properties that made them the prime candidates as repositories for the remarkable variety and specificity to be found in the reactions of living things. Thus did the protoplasm view of life give place to what Kay calls the "protein paradigm," and from this came what she and I have called the "nucleoprotein theory of the gene" and finally the DNA theory.

The claim of this book, however, is that this series of events did not follow from an inherent self-propulsion. Strenuous efforts were made in bidding for huge sums of money. Talent was spotted and harnessed to the task. Other programs in biology were left in comparative shade. Kay asks why molecular biology was "propelled . . . to its dominant disciplinary status." She concludes that it was an "expression of the systematic cooperative efforts of America's scientific establishment—scientists and patrons—to direct the study of animate phenomena along selected paths toward a shared vision of science and society." Although she has concentrated on the period to 1953, she gives a very brief summary of subsequent events. She concludes that despite the rejection of the protein paradigm and a change in the source of funding, the underlying assumption of the preeminence of the genetic material in determining what we are survived and "has acquired even greater intellectual vigor and social legitimacy."



"Representations of molecular structure of the amino acid L-threonine. Crellin Laboratory, 1947." Built in 1938, "the Crellin Laboratory, a physical link between Caltech's biologists and chemists, epitomized the Rockefeller Foundation's program, by now renamed molecular biology." [From *The Molecular Vision of Life*; courtesy of the Rockefeller Archive Center]

It follows from this analysis that one might find some institutional frameworks more appropriate to the prosecution of this program than others. By contrasting the six institutions that received the most support from Weaver's program—Chicago, Caltech, Stanford, Columbia, Harvard, and the University of Wisconsin—Kay comes to the conclusion that the involvement with the medical curriculum and strength in evolutionary biology were hindrances that Caltech, thanks to its unique institutional structure, lacked. The cases of Chicago and Columbia are curious. Chicago like Caltech received $5 million between 1930 and 1955 but contributed little to the new biology, apart from putting J. D. Watson through its undergraduate program. One wonders if there is more to learn here than Kay reveals. Perhaps Robert Maynard Hutchins's leadership benefited the humanities at the expense of the sciences. But it also raises the question of the influence of particular individuals. Did not Caltech biology decline until, after strenuous efforts by Pauling and the Rockefeller Foundation, which Kay describes, George Beadle was brought back from Stanford to Pasadena? As for Columbia, the fact that Erwin Chargaff was seeking to move to Caltech suggests that the environment at the College of Physicians and Surgeons was not conducive to molecular biology despite the presence of Hans Clark. And what might have been achieved at Caltech if Pauling had not brushed off Chargaff's request to move there?

How, one wonders, would the Rockefeller Institute for Medical Research (now Rockefeller University) fit into Kay's story? Indeed, how would Pauling's incursion into the biomedical sciences have developed without the collaboration of such leading members of that institute as Karl Landsteiner, Alfred Mirsky, and the Rockefeller-trained x-ray crystallographer Robert Corey?

Obviously one misses the European dimension in this book. The author admits that it is written from the viewpoint of Caltech and the Rockefeller archives. What would the European counterpart to Kay's account be like? In Britain the most striking feature in the development of molecular biology is the location of much of the work in groups funded by the Medical Research Council and free from the responsibilities and limitations of the teaching curriculum of university departments. I am not aware that eugenic concerns lay behind the funding here during the 1950s and '60s.

As a contribution to the history of the American involvement in molecular biology Kay's book is a work of considerable value, and it is written with clarity and elegance. The reader who studies her scrupulous documentation will be aware that parts of her story have also been researched by others, but she can claim that she has paid more attention to the broad intellectual and social agenda that lay behind the rise of this science. Her knowledge of the relevant archives is outstanding, yet she uses documentary evidence economically, keeping in view the broad features of the larger picture. Some details of the science cannot be right—ultracentrifuges are not usually used to shear colloids "into fragments" (though they do shear in the sense of putting under strain). William Astbury,

"Max Delbrück conducting an informal lunch-time seminar in virology, ca. 1948." On moving to Caltech in 1947 Delbrück "after nearly a decade and a half of freelancing in biology . . . could at last set up a permanent phage center for research and training." He wrote to his mentor Niels Bohr, "I am very happy about this because it signals the completion of my metamorphosis into a biologist, and because I believe that Caltech in the coming years will be to biology what Manchester was to physics in the 1910s." [From *The Molecular Vision of Life*; courtesy of the Rockefeller Archive Center]

J. D. Bernal, and Dorothy Hodgkin (née Crowfoot) discussed diketopiperazine-like ring structures in proteins, but Astbury's alpha-keratin structure was a folded chain, not a ring structure (the hexagonal folds were not closed by covalent bonds). Indeed, one feels that European scientists get short shrift in the book even when they find a mention. But Kay has her eye on more serious issues. Her analysis will provide a welcome resource for debates on the future development and application of the human genome project. There are those who feel that in our enthusiasm for genetic manipulation we are losing sight of the simple ways in which the quality of life of humanity can be improved. The publication of this book at a time when America is overhauling its health-care policy is opportune.

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Efforts at Internationalism

Denationalizing Science. The Contexts of International Scientific Practice. ELISABETH CRAWFORD, TERRY SHINN, and SVERKER SÖRLIN, Eds. Kluwer, Norwell, MA, 1993. viii, 301 pp., illus. $149. Sociology of the Sciences, vol. 16. From a conference, Abisko, Sweden, May 1991.

In this volume the authors set out to explore the "new" world of denationalized science, one different from the familiar one organized by and in nation-states. Defining "transnational science" as science involving persons, funds, or equipment from more than one country, they examine its dimensions in nine case studies.

In only one of the case studies does the author successfully make the case that internationalization has had substantial and definitive cognitive results in a given field. Abir-Am demonstrates not only that molecular biology was a field that emerged in "international space" but that inhibiting factors were present in national scientific cultures that would have made its emergence difficult otherwise. Thus, to take one example, Watson and Crick were able to achieve a breakthrough only by escaping the ideological limitations of their own national schools: one school was limited to empirical x-ray work on protein structure and had no genetic input, whereas the other was limited to phage genetics, with no molecular input.

Thus, according to Abir-Am, international space was required for suspending the conceptual control exercised by local or national traditions, locally sustained cultural bias, or institutionally induced prejudice (as in the case of the deprecation of biochemistry by phage geneticists). "International space," she concludes, "thus enables the process of construction, validation and authority formation of transnational objectives by creating social conditions which unlimit [*sic*] the range of human and material resources available within the subculture of a given national research tradition and practice."

If molecular biology may be reckoned a proof of the positive action of internationalism on scientific ideas, the other cases present a less clear profile. Elzinga's study of science in Antarctica shows that research agendas in that continent organized "by and for science" are more politically than scientifically dictated and that, in spite of an international bureaucratic superstructure to keep the research moving, what has resulted is less of an international effort rather than an example of "transnational collectivism."

Fischedick and Shinn examine the early history of the International Phytogeographic Excursions for evidence of denationalization. What they find is that the participants in these studiously international events all strove to push their national research programs and personal methodologies, although a certain amount of terminological and conceptual standardization was achieved. Jamison, examining the emergence of systems ecology after World War II, finds a British cognitive stream (the ecosystem concept) merging with German (systems theory) and Russian (economic planning) streams to produce a distinctive form of "scientific praxis" in the United States. The field was then diffused to Sweden, where a climate receptive to environmental management favored its successful introduction. I am not convinced that the case has been made for a new world of science here; the diffusion of ideas has, after all, been a motor of cognitive change in science from antiquity on.

Pallό studies the way in which Hungary was implanted in the Soviet system of "World-Science" in an account that is a succinct and illuminating introduction to the Soviet scientific system. Here the problem is that the institutional constraints were so great on Soviet bloc science, which lost its spontaneity and was run "along enforced lines," that one might well admit internationalism but wonder to what extent "science" was being done. That is, is science that has lost such basic structural features as peer review and most conventional modes of scientific communication and is embedded in a society that condemns "organized scepticism" really science? In any case, as Palló shows, the internationalism of the Soviet system was of a limited and quirky nature.

Krige's study of multinational physics at CERN gives a fascinating look at the way in which the conventional practice of physics has been altered to fit the technical specifications of high-energy physics. Here too, however, although it is clear that in social and institutional terms this is indeed a new world of international science, no case is made for any unusual cognitive dimension of this structure.

Finally, Sörlin provides some prehistory of modern internationalism by looking at scientific travel in the Enlightenment, the age of Cook and La Pérouse. This travel was global, but—Sörlin asks—was it international? His hypothesis is that national interest, both military and mercantile, formed a synergetic combination with science to stimulate the great expeditions; but their internationalism was only a patina covering the dark ambitions of imperial and commercial rivalry, particularly between France and England. I think a somewhat different case can be made, to the effect that the cumulative results of successive expeditions produced an outcome whose whole was greater than its parts. One only has to observe Malaspina studying La Pérouse, La Pérouse perusing Cook, Cook puzzling over Bougainville's charts, and Bougainville reprising Wallis and Dampier to get the picture of a cognitively interrelated international enterprise that transcended national agendas even against the will of the participants and that was more than "circumstantially transnational."

Part of the problem with this otherwise