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Review of Joan M. Fujimura, *Crafting Science: A Socio-History of the Quest for the Genetics of Cancer*

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Review of Joan M. Fujimura, *Crafting Science: A Socio-History of the Quest for the Genetics of Cancer*

Abstract

This is a bloodless book, literally and figuratively. It is bloodless in the sense that it excludes from its frame the cancer patients whose blood and tumors are the central physical materials of the high technoscience described here. And it is bloodless in the sense that it deals with a pressing moral issue—how some forms of cancer research become public and scientific priorities—without a moral perspective. This divorce from human need and vulnerability necessarily precludes consideration of the book as a work of history.

Disciplines

Genetics and Genomics | History of Science, Technology, and Medicine | Oncology

ment funding determine their interpretation. Their names change according to which interest group is uppermost, a nominalism that even John Locke would not have recognized. Hematological disease, it turns out, is like Gertrude Stein's description of Oakland, California: when you get there, "there is no there there."

Wailoo's is a highly original way of writing history. You could call it interpretive narrative. Wailoo sticks to the most classical of sources, such as published accounts in medical journals, and builds up a detailed, periodized story of each disease and its technologies and treatments. Moral management, liver extract, splenectomy—he slots them into the complex situations of their day in a way that allows us to understand the persuasiveness of their theoretical basis and the short-lived success of the therapy based on it. We can learn much, he says, by studying technologies, and treatments, that have not survived.

Wailoo's most striking story concerns the linkup between the test for sickling and the supposed inheritance of sickle-cell anemia as a Mendelian dominant. The detection of this symptomless trait in about 10 percent of Blacks allowed white physicians of the 1930s and 1940s to claim that sickling was a latent disease of Negroes and a marker for the detection of Negro blood. It not only marked Blacks as sickly but suggested that cases found in non-Blacks indicated hidden "black blood" in a family. The racist ideology that condemned miscegenation could point to sickle-cell anemia as an example of the dangers of allowing the Negro access to white society. By the 1950s and 1960s, the emphasis on the trait as a racial stigma was beginning to give way to the painful "sickling crisis" as a source of suffering in affected children. As scientific racism receded, it turned out that the trait was a recessive, not a dominant, and that only if a child inherited it from both parents would he or she be symptomatic. Ironically, that finding implied that miscegenation should be encouraged as a preventive measure. In addition, the newer technique of electrophoresis showed that there were many different abnormal hemoglobins, scattered throughout human populations. Their presence did not necessarily cause any problem. As the historian Owsei Temkin wrote in 1963, "Danger arises lest specific diseases be postulated which have no clinical reality." It is a danger that today's searchers for defects of the human genome might bear in mind.

Wailoo's cross-linkages between diagnosis, technology, professional and commercial interests, and the rise and fall of ideological tides make his work a very stimulating, insightful

book. It is, in addition, well written and a pleasure to read. Not only that, individual chapters have a stand-alone structure that will make them ideal for "readings" and discussion. I foresee that Wailoo's book will be a very successful text, one that will have a good influence on the field.

PAULINE M. H. MAZUMDAR

Joan H. Fujimura. *Crafting Science: A Socio-history of the Quest for the Genetics of Cancer.* xii + 322 pp., illus., bibl., index. Cambridge, Mass./London: Harvard University Press, 1996. \$45.

This is a bloodless book, literally and figuratively. It is bloodless in the sense that it excludes from its frame the cancer patients whose blood and tumors are the central physical materials of the high technoscience described here. And it is bloodless in the sense that it deals with a pressing moral issue—how some forms of cancer research become public and scientific priorities—without a moral perspective. This divorce from human need and vulnerability necessarily precludes consideration of the book as a work of history.

Joan Fujimura explores the rise of the proto-oncogene theory in the 1970s and 1980s. She considers the experimental systems that researchers used, the genetic technologies they employed, the arguments that guided their theories, and the kinds of problems this package of technology and theory solved. Her goal is to pull apart the processes involved in the making of theories and facts, with proto-oncogenes serving as a case study.

Proto-oncogenes are normal human genes that resemble genes in viruses that cause cancer in laboratory organisms. The theory that they could be triggered to produce human cancers was accepted by the scientific community by the late 1980s. The theory was novel in that it found the cause of cancer in normal genes. It was successful in the sense that it could be used to bring chemical carcinogenesis, radiation effects, tumor progression, and other phenomena under a single umbrella. It was successful too in the sense that it attracted funding agencies and was recognized within the scientific community as an appropriate part of grant applications. It involved materials that could produce relatively rapid results, and it created manageable problems.

Fujimura tells the story of scientific interpretations of the proto-oncogene by following what she calls traces of continuity, distributed authority, co-construction, theory-methods packages,

articulation work, and doability. There is no linear narrative, a form she explicitly rejects. There are some interesting observations and suggestions. Fujimura, for example, explores how the analytical place of biological materials can change: the tumor-causing virus served as the focus of a major research program in the 1970s, then, after 1980, shifted to become a technology for the exploration of a different biological entity, the proto-oncogene. She also maps negotiations across disciplines, institutions, and lines of research. The part I found most compelling was her discussion of those researchers who refused to be enrolled and resisted the “bandwagon.”

Fujimura’s methods and language are broadly sociological, and her writing style tends to be dense. She identifies herself as a proponent of actor-network theory, although she also employs many other frames. She considers disciplinary constraints inherently limiting. The book might therefore be understood as an exploration of what it would mean to study science without standing in any particular spot on the scholarly map.

SUSAN LINDEE

Ilana Löwy. *Between Bench and Bedside: Science, Healing, and Interleukin-2 in a Cancer Ward.* viii + 370 pp., illus., index. Cambridge, Mass./London: Harvard University Press, 1996. \$39.95.

Ilana Löwy, a senior researcher with INSERM in Paris, is a triple threat in the field of science studies: she is a historian of science who initially trained as an immunologist, and through her work she has gained sociological expertise, particularly in ethnographic research. She is, in addition, an excellent writer. In *Between Bench and Bedside*, she provides a rich analytic and descriptive study of the interplay between the worlds of basic biomedical and clinical research in the “war” against cancer. The initial focus of her research was a firsthand study of clinical trials of interleukin-2 (IL-2) at the Cancer Foundation in Paris. IL-2, first identified by immunologists in 1976, is a protein secreted by white blood cells that subsequent research showed to be a nonspecific stimulator of the immune system. The IL-2 molecule, Löwy states, “was immediately perceived as having important therapeutic potential” for immune deficiency conditions, including the type often seen in advanced cancer. When the IL-2 gene was cloned in 1983, research groups, biotechnology and

pharmaceutical companies, and the media rapidly pronounced it the newest experimental anticancer “miracle drug,” “a natural weapon against cancer.” Early human trials in the United States, however, indicated that IL-2 was more toxic than expected, and the “clinical results in cancer patients were not very impressive.”

Despite these results, many leading cancer centers in the United States and Europe launched clinical trials of IL-2 between 1985 and 1990, one of which was the Cancer Foundation research that Löwy observed from November 1986 until July 1990.

In response to the results of the trials that had been conducted by the time the Cancer Foundation’s trial began, the scope of Löwy’s research, and the eventual contents of the book, broadened, for she became intrigued by the question of *why* the trial was being conducted, as well as *how*. This question led her to explore and embed the IL-2 trial in a broader historical and sociological context: the development of a distinctive culture of clinical experimentation in oncology; the scientific and social history of cancer immunotherapy from the 1890s through the 1980s; and the interrelationships among the scientific, medical, and sociopolitical cultures of the Cancer Foundation itself.

In the end Löwy found that the Cancer Foundation trial “turned out to be a different enterprise from the one [she] had expected to follow.” But the actual nature and outcomes of the trial itself, and the issues it led her to consider, generated an absorbing study. I was particularly struck by her account of the relationships between basic and clinical research—the interplay between bench and bedside—in the IL-2 trial. “One of the trial’s main declared goals,” Löwy notes, “was the demonstration of smooth and fruitful collaboration between scientists and physicians” in the immunology and hematology labs and the oncology clinic. There was, indeed, collaboration between the lab and the clinic, which Löwy ascribes to the “science-laden” development of orthodox cancer therapy. But despite the mention of “cancer ward” in the subtitle of the book, Löwy’s observational work, and thus most of her analysis, was confined to the laboratory. The Cancer Foundation’s labs and oncology wards were separated not only by actual physical walls but by many nonmaterial barriers as well—not least among them the fearful reluctance of the scientists, and the observer herself, to leave the confines of the laboratory and enter the world of the desperately ill cancer patients and the physician-investigators studying the effects of IL-2 on them and their disease. If there is an element