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Roderick D. Buchanan. *Playing with Fire: The Controversial Career of Hans J. Eysenck.*

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“scientists do not simply weigh the evidence on both sides of an issue; they also compare the track records of competing theories” (p. 123).

Brush’s empirical approach to evaluating theory acceptance is a welcome tool for historians and philosophers, although it may be more successful in dealing with discrete theories rather than those, like natural selection, that have complicated, layered histories. There are simply too many factors to take into account; and in relying on secondary sources to characterize the positions of so many scientists Brush risks oversimplification, as in his claim that Richard Goldschmidt’s opposition to natural selection in favor of evolution via macromutations “was a reflection of the holistic, neoromantic biology of the 1930s, which was about to be displaced by the mechanistic reductionistic biology of the 1940s and 1950s,” without mentioning the developmental genetics that spawned Goldschmidt’s views (p. 34). Nonetheless, Brush’s endeavor to identify and quantify the salient factors that influence scientists to accept new theories is an important methodological contribution to scholarship.

MARSHA L. RICHMOND

Roderick D. Buchanan. *Playing with Fire: The Controversial Career of Hans J. Eysenck*. xi + 475 pp., illus., bibl., index. Oxford/New York: Oxford University Press, 2010. \$79.95 (cloth).

The author of eighty books and eleven hundred articles, the British scientist Hans J. Eysenck—born in Germany in 1916, he emigrated to Britain shortly after the *Machtergreifung*—was the most highly published psychologist ever; in a typical year during his prime he produced at least one book and almost fifty journal articles and chapters. “Without qualification,” writes Roderick Buchanan in *Playing with Fire*, his important book on Eysenck’s career, Eysenck “was the most prominent psychologist in post-war Britain, and probably the most influential” (p. 425).

Yet Eysenck never enjoyed the status usually associated with such a productive record—he was never named an honorary fellow of the British Psychological Society nor elected a fellow of the Royal Society—probably because he had almost as many enemies as publications. As Buchanan describes him, Eysenck was a polarizing figure who “seemed only too pleased to get up certain people’s noses” and believed that if he was not provoking or offending his adversaries—a group including psychiatrists, leftist social theorists, medical epidemiologists, and tra-

ditional experimentalists—then “he was just not doing his job” (p. 12). But Eysenck also had his defenders, especially among the many researchers who had apprenticed in his lab. As Eysenck’s son, Michael, asked Buchanan, How could anyone be neutral about his father?

Buchanan might not be neutral, but he is indisputably fair. He writes neither to praise nor to bury Eysenck but, rather, to understand the man and the controversies that marked his career. Deprived of the opportunity to look at Eysenck’s files by his widow’s decision to have them destroyed, Buchanan nevertheless affords her the book’s epigraph expressing disagreement with his views, a gracious gesture he was under no obligation to provide. Although the inability to see the files was an obstacle, Buchanan has done a laudable job of interviewing numerous people, both critics and supporters, and tracking down correspondence in archival repositories.

Playing with Fire is organized thematically, each chapter describing a different area of Eysenck’s interest; the sequence is in fact quasi-chronological, since the later chapters discuss interests that emerged later in his career. His earlier work in personality, clinical psychology, and political psychology was controversial within the research community, partly as a consequence of the conflict between psychology’s two disciplines—the individual differences school in which Eysenck was trained and the experimentalists—but even more because of what Buchanan characterizes as mistrust of Eysenck’s data, a suspicion reinforced by his frequent reliance on unpublished research, his reluctance to provide the raw numbers, and a paucity of detail about procedure. But these tempests might have remained confined to the academic teapot were it not for Eysenck’s entry into the race and intelligence debate—an area in which he had done no research and had no data to report—with a hastily composed book consisting largely of lengthy quotations from others, the main contribution of which seemed to be the reenergizing of “right-wing racialist groups” (p. 324). The resulting demonstrations and personal threats led to the cancellation of some public appearances and forced Eysenck and his wife to travel under pseudonyms. In the latter decades of his life Eysenck also accepted lavish funding from the tobacco lobby in exchange for conducting research intended to be useful in litigation and insisting both that nicotine was not addictive and that smoking had no causal relation to illness.

According to Buchanan, much of the controversy surrounding Eysenck resulted from his

approach to science “as a *competitive game* characterized by a debunk-and-destroy ethos rather than caring-and-sharing” (p. 258). Having adopted an adversarial model for his work, it was thus natural that Eysenck sought adversarial issues, especially those that would satisfy his desire for the spotlight. Utterly uninterested in gender differences in the early 1960s, for example, Eysenck had a rapid change of heart a few years later, after the topic became a hot-button issue.

Buchanan’s treatment of his subject is commendably evenhanded. While he gives ample attention to the claims of Eysenck’s critics, he does not hesitate to point out their own excesses. In addition, he offers generous interpretations of questionable behavior, finding it unfair, for example, to criticize Eysenck’s relationship with the tobacco industry, since “many scientists . . . face perennial funding crises” (p. 406). Unfortunately, this impressive scholarly work is poorly served by an inadequate index. That aside, *Playing with Fire* is likely to be the definitive work on one of psychology’s most controversial figures.

WILLIAM TUCKER

Paul N. Edwards. *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming.* xxviii + 518 pp., illus., tables, index. Cambridge, Mass.: MIT Press, 2010. \$32.95 (cloth).

For historians of science interested in global environmental issues, two intertwined questions stand out. First, how have we known that we have global environmental problems? And second, how has this knowledge shaped political responses to these problems? Paul N. Edwards’s extensive book sheds much light on these questions by telling a grand history of the observing systems, the models, and the scientific and political institutions that have grown up around the study of climate, climate change, and global warming. He also provides an insightful framework for analyzing the knowledge systems on which our understanding of global environmental problems has depended.

The bulk of *A Vast Machine* traces the history of the observing systems, models, and disciplinary communities that have studied weather, climate, and climate change. The narrative extends at least from John Ruskin’s stirring 1839 vision of “a vast machine”—a global network for weather observation—to the current politics of global warming. The breadth of the topics covered is staggering and impossible to relate here

in full. Key episodes include the development of international weather-observing networks and the rise of the World Meteorological Organization in the late nineteenth and early twentieth centuries; the first attempts at numerical weather prediction in the interwar period; the advent of computer models of the atmosphere in the work of John von Neumann and Jule Charney after World War II; the International Geophysical Year of 1957–1958 and the World Weather Watch of the 1960s and 1970s; the emergence of climate change as a key issue in international politics in the 1980s and 1990s; and ongoing debates over how to model the atmosphere and study the global climate.

Throughout, as his title suggests, Edwards tells this story not as a history of ideas, instruments, or political developments alone, but as a history of an integrated “knowledge infrastructure,” “robust networks of people, artifacts, and institutions that generate, share, and maintain specific knowledge about the human and natural worlds” (p. 17). This infrastructure can make knowledge about the climate in a couple of ways. The first is by “making global data” (p. xiv), gathering together observational records of the key parameters describing global weather and climate. The second is through simulation models, grounded in knowledge about the physics of the atmosphere, that can predict future weather or climate. Such models, coupled with data analysis algorithms, can assist in “making data global,” taking the heterogeneous observations available to scientists (from ground observations, balloons and radiosondes, and satellites) and generating data points that cover the entire global atmosphere. These processes of the knowledge infrastructure are, however, impeded at various times by “frictions” that result from problems of data standardization and collection, as well as limitations of computational power. The story Edwards tells therefore unfolds as a tug-of-war between frictions of all sorts that inhibit the development of a stable and accurate picture of the global atmosphere. Prior to World War II, advances in weather data networks overwhelmed processing abilities; subsequently, the development of electronic computers reversed this situation as modelers demanded increasing amounts of data. The computer in turn catalyzed the reunification of meteorology with climatology, two fields that had moved apart in the nineteenth century to focus on divergent goals and timescales.

Throughout the book, Edwards spins off myriad arguments and suggestions that directly address literatures in political science, science and technology studies, and the history of science.