ation of the nonconformist or the "uppity Negro," and perceptions of black deviancy, including mental illness and more general "disease"? Haller has not so much answered questions as furthered inquiry, and he shows that historians have been no more successful in making sense of the role of science in social tragedy than have contemporary scientists in a variety of disciplines.

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## Historical Interpretations

**The Origins of Theoretical Population Genetics.** WILLIAM B. PROVINE. University of Chicago Press, Chicago, 1971. xii, 202 pp. $7.75. The Chicago History of Science and Medicine.

This is a little book about a big subject. It covers a field which has received little attention from historians, and one sufficiently difficult that even understanding it is a serious problem for an outsider. Add to this the fact that the history of science itself is as yet a rather immature discipline, and the task of writing a history of population genetics becomes most formidable. We should be most grateful for so useful an introduction, even if further work soon necessitates different interpretations.

Provine has circumscribed his study to concentrate upon the more influential figures and schools. He stresses the conflict between the Mendelians and biometricians, and the work leading up to the syntheses of Fisher, Wright, and Haldane. This approach has obvious difficulties, in that the works examined may not adequately reflect what is going on. Indeed, one of the points that emerges from this study and others like it is how little the arguments presented in research papers reflect the actual reasons why scientists advocate theories. Peripheral influences may not be sufficiently recognized, and one has to be very careful in accepting what one scientist says about another. Provine has taken advantage of personal interviews to learn how Wright thinks he has been misinterpreted. One isn't always in a position to obtain insight in this way, however, and Provine rather uncritically goes along with Fisher's interpretations of Darwin.

Provine documents some general conclusions which give his work unusually great interest for working scientists. The unfortunate lack of communication between those who make history and those who write it may result in our not appreciating how right he is or how much it matters. First, he notes that "personality conflicts are sometimes very important in the development of scientific ideas." I dare say that this should be given as a rule rather than an exception, and the rule casts a great deal of light on the history of biology. Provine finds it curious that Huxley, one of Darwin's "staunchest supporters and admirers," advocated saltationism. There are two basic reasons, neither of which is generally recognized, for Huxley's attitude: he didn't understand natural selection, and he was jealous. The failure to understand natural selection helps to explain as well why Huxley opposed Darwin on genealogical classification. Also, Darwin was a Cavalier of biology, Huxley a Roundhead. Darwin had independent means, but Huxley had to struggle to make a living. Darwin made it as a theoretician, Huxley succeeded through professionalism and public service. I wonder how much of Huxley's advocacy of Darwinism was ultimately motivated by a desire to get even with Richard Owen. Second, "the acceptance by scientists of a new idea is sometimes more dependent upon its a priori acceptability than upon its scientific proof." We might add that the very idea of "proof" is suspect. Was Bateson more reasonable in criticizing the chromosome theory of inheritance than he was in championing Mendel's genetics? Finally, a "field of science can begin with a theoretical structure which is far from consistent," and in the field under study here the inconsistencies are still with us. The problem goes far beyond resolving contradictions. When we follow the misadventures of blending inheritance, genetic load, the "bean bag" approach, and the like, it may seem dubious whether gratuitous assumptions can ever be totally excluded from our thinking. The trouble with theoretical population genetics, now as always, is that so many of its basic premises are false. It needs continued support from empirical population genetics. Our universe is not populated by mathematical models, and if the organisms contradict the theories it is not the organisms that have to be corrected.

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## A Tussle with Orthodoxy

**Homeopathy in America.** The Rise and Fall of a Medical Heresy. MARTIN KAUFMAN. Johns Hopkins Press, Baltimore, 1971. xii, 206 pp. $10.

This instructive account traces the strange American odyssey of homeopathy from its tardy arrival in the second quarter of the last century to its present moribund state. In so doing, the book sheds light on the history of the orthodoxy with which homeopathy constantly wrestled in a tussle sometimes bitter, sometimes gentlemanly.

With its mild medication, homeopathy won countless converts among patients tired of heroic bleeding and purging and so forced regular medicine to relax its heroism. In their turn, many homeopaths forsook the purity of Hahnemann's systematic theories and borrowed what seemed useful from the burgeoning medical revolution. This posed a continuing identity crisis for homeopathy: adapt the new and become lost within the regular medical profession; hold to the old and become quaint, outmoded, and perhaps barred from practice by tightened licensing laws. The major wing adopted the new sufficiently that in 1903 the American Medical Association accepted homeopaths to membership, but the homeopaths still held on stubbornly to a unique materia medica, a retention that probably prevented the AMA from granting homeopathy status as a therapeutic specialty.

Kaufman might have done better than he has at explaining homeopathy's 20th-century reluctance to abandon its special materia medica and at characterizing homeopathy as a therapeutic mode. He does not mention the homeopathic pharmacopoeia, a volume that was lofted to an official status in federal drug regulation with the enactment of the 1938 Food, Drug, and Cosmetic Act because the law's chief sponsor, Senator Royal S. Copeland, was a homeopathic physician. Nonetheless, even though denied access to crucial records, Kaufman relates a much fuller story than has heretofore been told about homeopathy's recent history. By 1923, as a result of the radical upgrading of medical education, only two of the 22 homeopathic medical colleges that had existed in 1900 remained, and these two lost their homeopathic distinctiveness a decade later when an AMA council refused to continue the approval of schools teaching "sectarian medi-