

Book Reviews

impressive. Spain, Italy, France, Germany, and Sweden all lost reigning monarchs, in the days when monarchs really reigned, to the disease.

Dr Hopkins has not been satisfied with tracing the European history or even the Western history of the disease; he takes in the Asian dimension, and the African one, and details the effects of smallpox on the ruling houses of China and Japan, and among the South American Incas. It is a very impressive compilation, although it inevitably suffers from a superfluity of royal – or ruling – name-dropping and the narrative breaks down slightly in the chapters on other continents when the unfamiliarity of many of the rapid progression of names makes it difficult for the reader to grasp fully the historical context.

It seems ungrateful in the circumstances to single out minor inaccuracies which have crept in, but for the sake of possible future editions an English reviewer cannot pass unopposed the casual comment that Edward Jenner was never “made a member of the Royal Society of Medicine” (p. 80). That Society did not, in fact, see the light of day until 1905 when it was established as an integration of a number of different London medical, pathological, obstetrical, and epidemiological societies. Its main parent body was Medical and Chirurgical Society of London, which was formed in 1805 and of which Jenner was a founder member. By then, he had long been a Fellow of the Royal Society of London; he was, however, never admitted to the Royal College of Physicians.

The present volume is an impressive guide to the influence of one major disease on the working and ruling classes of the world. Dr Hopkins' own close involvement in the eventual eradication of the disease gives him a special insight into many aspects of its history and makes for a very valuable comment on its chequered career. There is an excellent bibliography of nearly 900 books and papers, and Dr Hopkins has made good use of them all. How he ever found the time is hard to understand, but the result is a welcome addition to the smallpox literature.

Lise Wilkinson
Royal Postgraduate Medical School

PHILIP F. REHBOCK, *The philosophical naturalists. Themes in early nineteenth-century British biology*, Madison, Wis., and London, University of Wisconsin Press, 1983, 8vo, pp. xv, 281, illus., £25.50.

Rehbock divides his book into two parts, following the preDarwinians who themselves applied the epithet “philosophic” to two distinct groups – transcendental morphologists and biogeographers. The first part extends the work of Dov Ospovat and surveys the life and publications of a select group of morphologists, primarily Knox, but including also Carpenter, Barry, Roget, and Owen. The second portrays the quite different ecological concerns of Knox's pupil Edward Forbes, whose 1846 “Connexion” essay is the *raison d'être* of this section – an extraordinary essay which Forbes himself teasingly called “a coup d'oeil of the history of the British flora and fauna” (p. 184).

Rehbock gives good account of Knox and his Edinburgh students, Goodsir and the transcendental chemist J. G. MacVicar. He also tackles Knox's career problems and publishing decline, although his bafflement at Knox's waning influence and reliance on medical journals (pp. 54–55) might have been lessened if he had looked at Knox's “savage radicalism” and the primary medical context of higher anatomy in the 1830s. Similarly, Rehbock overstates Roget's and Forbes' roles as metropolitan innovators (pp. 56–57, 70); in fact, rival transcendental anatomies were well established by the mid-1830s among bourgeois reformers and conservative romantics. With his science disconnected from its medico-political base, one gets little feeling for the social functioning of higher anatomy. Yet it is precisely because morphological science was modified for different social ends that it is hazardous to treat British transcendentalism as a single movement “legitimized” by the writings of the Presbyterian minister James M'Cosh (p. 98).

Methodological quibbles aside, for me Rehbock's achievement lies in his elucidation of Forbes' “zoo-geology” and innovative use of palaeofaunal data. Still more did I delight in his investigation of claims that the reputable academic Forbes had actually appropriated part of

Book Reviews

that data from the Lamarckian phrenologist H. C. Watson, rationalizing this on the grounds that Watson had uncovered transcendently true facts that were now free for the taking (p. 180). This, indeed, is rich material on the plight of the “outsider” in a professionalizing culture.

Adrian Desmond

JEFFREY M. MASSON, *The assault on truth. Freud's suppression of the seduction theory*, New York, Farrar, Straus & Giroux; London, Faber, 1984, 8vo, pp. 308, £9.96.

Nothing sells books like scandals. The spate of biographies and autobiographies of the Watergate culprits would be proof enough of this. But here we have a scandal in the academy. The chosen heir of the Freud Archives goes public (in the *New York Times*, no less) and claims that Freud was a fraud. Jeffrey Masson's struggle with the Freud Archives and the psychoanalytic establishment's rejection of him and his views on Freud have coloured every review of this book. It is the scandal about Masson, not the book, which has been reviewed. I come to this book with mixed feelings. On the one hand, I doubt the “truth” of *The assault on truth*; on the other, as a philologist turned medical historian, I am in great sympathy with Masson, a philologist turned medical historian.

Let me begin with what is evidently wrong with the book. Masson's thesis is, quite simply, that Freud turned away from the seduction theory for the aetiology of neurosis (a theory which Masson implies is valid) because of his transference with his friend Wilhelm Fliess. Fliess' incompetence had led to a near fatality in the treatment of one of Freud's patients. Freud wished to dismiss this and was forced to see the error in the patient's response. This pattern, once established, dominated Freud's perception of his patients' illnesses and led him to reject all “reality” as the source of neurosis and substitute “imagination”.

Masson's case is, as many commentators have pointed out, weak. First, Freud never rejected sexual abuse of children as a “reality”. As late as the 1920s, he commented on a case of sexual abuse in order to differentiate it from the fantasies of sexuality omnipresent in childhood. Freud's knowledge of such abuse, to which Masson devotes a chapter, was never in question. He, like all medical practitioners, knew the general medical and legal literature on child abuse. The legal literature was usually summarized in the back of most medical textbooks! That Freud attempted to shield Fliess is without doubt true, and here is where the material in the book is interesting. Max Schur began the unravelling of the background to the key dreams in the *Traumdeutung* in an often-quoted piece in the Hartmann *Festschrift*. Masson continues this and provides much detailed information. His chapter on Emma Eckstein is a valuable addendum to Schur. But is his interpretation right?

Schur and Masson are valuable precisely because no one else has had access to the manuscript material. We will have to rely on this interpretation until the German originals (not Masson's translation) appear. At that point we shall be able to stop reading the Freud-Fliess relationship as penitence through the interpretations of scholars with axes to grind. Here the Freud Archives have done themselves, as well as the history of medicine, a disservice. Either close the material or open it! Stop playing games with favourites, either in regard to the Marie Bonaparte material or the Freud material. Let qualified scholars use it, so that each forms a check on the others, or let no one use it and some lucky scholar born about now will uncover it long after our deaths, like the Boswell papers, hidden in the loft of the Library of Congress.

Masson's book has a specific thesis. But many scholars will mine it for quotes and innuendoes in spite of the thesis because Masson, at least, had access to the material. Masson's book is not good history of science. It is not Sulloway or Ellenberger. But it will be used. In spite of my own sympathy with Masson as a philologist turned historian I am not convinced by his argument. Indeed, I am quite appalled at his use of evidence, which is spotty and superficial, guilt by association and omission. His book is shaped by the scandal his views raised in the psychoanalytic establishment. Psychoanalysis survived Freud's mistakes and will certainly survive Masson's. The underlying truths of the psyche as outlined by Freud (not those coloured by his age and time) are not affected by his motivation. Did Galileo's fudging of his mathematical evidence to disprove Ptolemy mean that the sun *did* revolve about the earth? So too the discussion of Freud (especially when undertaken in analytic terms) does not undermine the validity of his views. Here Masson should read Juliet Mitchell in some detail.