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Author
AYALA, FJ

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Book Reviews

Editions and Selections

STILLMAN DRAKE, *Cause, experiment and science. A Galilean dialogue incorporating a new English translation of Galileo's 'Bodies That Stay atop Water, or Move in It'*. Chicago and London: The University of Chicago Press, 1981. xxix + 237 pp. £14·00.

Consider a life-long student of Galileo who plans to translate a work of his into English. Consider also that, to make the text entirely understandable to non specialized readers, he associates to his translation many introductory and explanatory data. Thirdly, since that work (the *Discorso intorno alle cose, che stanno in su l'acqua, o che in quella si muovono*) is one of the less carefully studied, the student also takes his opportunity to declare its importance in the forming of Galileo's thought and in the very origin of modern science. Moreover, he extends himself to discuss conceptual aspects which he thinks to be peculiar to the book and to its author's practice of science. Then, if such a student is Stillman Drake, whose convictions on this subject sometimes differ sharply from those of other authoritative historians, the temptation is strong for him to underline alternatives in interpretation and to point to elements supporting his own. Lastly, the idea of that student is not to produce an usual, scholarly edition of a text; he aims to amalgamate all his materials in a speech, having the form of a dialogue, in which the very distinction between translated text and critical apparatus is almost eliminated.

As is natural, the dialogue's characters are the same with those in Galileo's last works (Salviati, Sagredo and Simplicio, whom Drake identifies with Galileo's colleague at Padua, C. Cremonini), the style and historical situation of which are reproduced. The reason for doing this, our scholar writes (p. ix–x), has been that of Galileo, namely to interest a general public to a specialized subject. This aim, it may be said, has been reached, because the book is readable and able to convey to non-Italian readers something of the historical atmosphere (the dialogue being imagined to happen in Venice). As for the specialists, reliability and opportunity of Drake's translation may be taken for granted, so that other points may be shortly considered. A chief thesis in the book (pp. xi, 220) is that, owing to its openly experimentalist tone, the *Discorso* is in some ways a more direct clue to origin and nature of Galileo's science than are later and more famous works, the mathematical content of which partly conceals that origin, so favouring those 'theoreticist' or 'philosophical' interpretations of Galileo's work that Drake opposes. A connected thesis, which is also found in any of Drake's Galilean writings but takes new life here, is that the point of departure between the classic idea of *episteme* (or *scientia*) and new science was reached when, in front of a model of knowing which was metaphysically oriented, deductively constructed and disputative, another emerged essentially experimental in gathering its facts and mathematical in discussing, connecting and forecasting them. It is difficult (and perhaps impossible, Drake adds on p. xxv), to say when such a model emerged *in principle*, but he still maintains that it was with Galileo that it began to work *in practice* as a definite alternative to the old one. So sharp a distinction, one may observe, is as old as positivist historiography (Drake writes at p. 223 that Galileo's definition of 'cause' looks to him more positivistic than Hume's); he has done much in recent years to bring new evidence in support of it, and here he wholeheartedly defends it against corrections or true refusals coming from Cassirer-oriented, Koyré-oriented, Popper-oriented or even 'anarchically'-oriented historians and epistemologists.

On the whole Drake seems to make some good points against his too philosophical adversaries, yet it may be that he oversimplifies a situation which cannot be made to correspond immediately to today's concepts. It is not a reviewer's task to decide an historical truth, specially when concerning such a general (and vague) problem, but I do not find it entirely convincing to say, as Drake repeatedly does (pp. 209, 211, 215, 230), that Galileo squarely distinguished 'science' and 'philosophy' as being two fields of mental activity entirely different both in subject and method. Among other things the Italian word *scienza*, when used (not commonly) by Galileo, does not have a modern meaning but a traditional one, in which it is a synonym of 'knowledge'; nor did the traditional distinction between mathematics and physics (natural philosophy), to which Galileo referred more commonly, convey the same meaning as the first.

Anyway, these conceptual points, important as they are, may be postponed to others, concerning which the book is an useful contribution. Drake also repeated all the experiments to be found in the *Discourse*, so that he has been able to clarify some previously unnoticed facts and to state clearly the relationship between Archimedes's and Galileo's hydrostatics. But what is to be praised mostly is perhaps Drake's courage and fantasy in attempting something that an academic scholar would not, usually, even dream about: he managed to construct a truly Galilean dialogue. It may be that the only departure from Galileo's characters lies in the fact that Drake's Simplicio looks sometimes a less dogmatic and less narrow mind when compared with the original. This may result from Drake's admission (pp. ix–x) that the Aristotelian position was not always so prejudiced and empty verbalistic as it has been fashionable to represent it. He felt perhaps his literary adventure as being a small, but genuine Galilean revolution in historiographical conventions; knowing his deep concern with everything Galilean, this may well have been his first reason for writing this book.

UGO BALDINI, *via di Novella, 8, 00199 Rome, Italy*

Essays and Biographies

JONATHAN HOWARD, *Darwin*. Oxford and Toronto: Oxford University Press, 1982. viii+98 pp. £1·25.

One of the 'Past Masters' series, Jonathan Howard's short book on Darwin is a clear, well-written exposition of the main tenets of Darwin's thought and the path by which he was led to embrace first evolutionism and then its mechanism, natural selection. The book is really mistitled, however. It is not really a biography of Darwin's life, but an intellectual history of Darwin's ideas. In its very few pages, the book covers remarkably well not only biological and geological concepts such as variation and inheritance, geographic distribution of animals and plants, comparative anatomy and embryology, adaptation and the interpretation of the geological and especially fossil record, but also more general background concepts such as eighteenth century natural theology, the 'economy of nature, perfectionism and progress as they each affected the development of Darwinian thought. A lot is packed into less than 100 pages.

In a brief opening chapter on Darwin's life, the author sketches some of the early influences on Darwin's thinking about natural history: Paley's natural theology, so prevalent at Cambridge where Darwin entered as an undergraduate in 1827; the Beagle voyage and Darwin's first-hand experience in round-the-world nature study; and the profound impact of Charles Lyell's *Principles of Geology* (1830–31), a copy of which Darwin carried with him on the Beagle from 1831–1837. From natural theology, Howard claims that Darwin gained an appreciation of adaptation of organisms and their environment; from the Beagle voyage, direct exposure to the flora, fauna and ecology of wholly new regions, experiences in geology and paleontology, and a general sharpening of his observational skills; and from Lyell an appreciation not only of the principle of uniformitarianism, but also an understanding of the importance of slow, gradual change in producing even the most massive alterations (as in uplifting or erosion of mountain chains). From Lyell, too, Darwin obtained a strong feeling for methods of reasoning, for how to make some sense out of the chaos of rocks and exposed strata one finds on entering a new region. The same chaos might at first appear to a naturalist on entering the new world of a tropical rain forest, or the intricate relationships among animals and plants on a coral reef. But through Lyell's careful but persistent reasoning Darwin learned the apparent choas could often be reduced to clear and logical order. As Darwin said, in tribute to Lyell's influence, 'I always feel as if my books came half out of Lyell's brain,...for I have always thought that the great merit of the Principles [of Geology] was that it altered the whole tone of one's mind' (p. 4).

Using material from various of Darwin's unpublished notebooks (now published) as well as the autobiography, collected letters, and of course, published books, Howard presents a clear reconstruction of the struggles—logical, empirical and emotional—which Darwin encountered between 1837 and 1859 in fashioning the theory of natural selection. Howard argues that Darwin had to deal with three fundamental concepts in structuring his theory: the nature of species, the nature and meaning of adaptations, and the idea of evolution itself. Each one of these concepts had to be painstakingly reasoned out, correlated with facts, and compared to alternative explanations (such as Special Creation) before Darwin was ready to put his ideas into print. In Howard's presentation, the hypothetical nature of Darwin's major postulates (for example, the usefulness of intermediary stages in development of complex structures) stands out in clear relief, and is a good lesson for all those who claim, even today, that evolution is a 'fact'. At the same time, the ultimate power of Darwin's reasoning stands out so clearly and is so eminently believable, that one can sense, anew, the sensation which *The Origin of Species* must have caused when it was first published in November of 1859. Howard's ability to tease out the various strands of Darwin's thinking is a testament not only to his reading and careful use of modern sources, but also to his skill as a writer.

Aside from being short on biographical detail, the book does have one other notable omission. Much of the recent Darwinian scholarship which Howard uses so well in dealing with intellectual history is not invoked to deal with the social history of Darwin's work. Not only do we learn relatively little of Darwin's personal life; we learn even less of his family's social and economic background, the industrial revolution or the developmentof political economy in the early to mid-nineteenth century. For example, in discussing the 'economy of nature', which Howard does admirably in so far as it relates to Darwin's argument about the imperfection of adaptations (pp. 83–85), he says little or nothing about Darwin's reading of Adam Smith or David Ricardo (only Malthus gets some mention among political economists), or of the influence of metaphors from nineteenth century industrial capitalism on concepts in natural history. This is all the more regrettable since by now a good deal has been written on the social context of Darwin's thought, providing the historian with insight into the interaction of science and society.

All in all, however, Howard's book provides a brief, easily-accessible introduction to Darwin's own thought that is hard to beat. It would be particularly useful, I should think, as an introduction to Darwin for courses in history of science/biology at the university level.

GARLAND E. ALLEN, *Department of Biology, Washington University, St. Louis, Missouri 63130, U.S.A.*

LESTER D. STEPHENS, *Joseph LeConte: gentle prophet of evolution.* Baton Rouge and London: Louisiana State University Press, 1982. xix + 340 pp. No price stated.

The University of California stands out as one of the largest and most distinguished institutions of higher learning. Several Nobel Prize laureates and scores of members of the U.S. National Academy of Sciences signal the excellence of its faculty. And it counts nine campuses, more than one hundred thousand students, and an annual budget in the neighbourhood of five thousand million dollars.

This imposing institution traces its origins to lowly beginnings. On 17 November 1868 John LeConte, a natural scientist at the University of South Carolina, received word that he had been hired as the first faculty member of the newly founded University of California. On 2 December, his brother Joseph LeConte, then also at the University of South Carolina, was appointed professor of geology at the new institution. Classes started in the fall of 1869, with John LeConte as acting president and a total of ten professors including the two LeConte brothers. The ageing Henry Durant was appointed first president in mid 1870 to be replaced in 1872 by a brilliant scientist, Daniel Coit Gilman, who resigned at the end of 1874 because of a conflict about educational policy with the Regents of the University and other influential Californians. Gilman believed that a university should be founded upon liberal studies in the arts and sciences rather than be dedicated to practical training. John LeConte was appointed acting president once again on 21 March 1875, and became permanent president from June 1876 to 1881. John continued teaching physics at least until 1889 and died two years later. His younger brother Joseph remained an active teacher and one of the most influential natural scientists of the University faculty until shortly before his death on 6 July 1901, when he was 78 years old.

Lester D. Stephens, professor and head to the Department of History of the University of Georgia, has written an authoritative biography of Joseph LeConte, born in Georgia, the son of a large plantation owner, a student of the University of Georgia and a member of its faculty from 1853 to 1855 as a professor of geology. This chair included among its assignments the teaching of French, a duty for which LeConte prepared himself by taking lessons from a native Frenchman as soon as he accepted the post.

This book is archival history, derived from direct research of original sources. And it is loving, almost hagiographic, history. Joseph LeConte is presented with warts and all, including his racial prejudice and opposition to the college education of women. But the accent is on the love for his family, his career as a dedicated educator, his accomplishments as a variegated writer on geology, physiology, evolution and the strains between religion and science.

Stephens traces LeConte's life from birth and early comfortable life through the disasters of the Civil War, which left him without fortune if not truly indigent, to his definitive move in mid-life to California. Migration to California was necessitated by the pitiable condition of the South. From LeConte's view, deterioration of the economic conditions was not the only evil. In the summer of 1868, he wrote to a friend: 'the feeling of depression is so great among us ... [The] legislature now sitting in this place [South Carolina] is composed entirely of the most ignorant men in the State and at least $\frac{3}{4}$ of them are *blacks*. It makes me sick to think of it' (p. 108).

Joseph LeConte wrote nine books and over 190 scholarly articles, by Stephens' count. His *Elements of geology*, first published in 1877, went through four editions, and persisted as a popular college textbook for three decades. But his most successful book doubtless was *Evolution and its relation to religious thought*, published in 1888. LeConte had fully accepted the notion of biological evolution by around 1870, in opposition to his revered teacher, Louis Agassiz, the most renown American naturalist of the time. These two books are good signposts of LeConte's scientific career, for his accomplishments as research scientist pale in comparison to his role as educator and popularizer of science, particularly of the theory of evolution.

Joseph LeConte is not history writ large, rather a documented narration of LeConte's life. But those interested in the U.S. intellectual, social, or economic history of the second half of the nineteenth century will learn much from reading it.

F. J. AYALA, *Department of Genetics, University of California, Davis, California 95616, U.S.A.*

History of Philosophy

Theoria cum praxi. Zum Verhältnis von Theorie und Praxis im 17. und 18. Jahrhundert, edited by Albert Heinekamp and Ingeborg von Wilucki. (*Akten des III. Internationalen Leibnizkongresses, Hannover, 12. bis 17. November* 1977, Band II, *Spinoza. Studia Leibnitiana Supplementa*, vol. 20.) Wiesbaden: Franz Steiner Verlag, 1981. vi+202 pp. DM 48.

When Tschirnhaus met Leibniz in Paris, he had part of Spinoza's unpublished *Ethics* with him, though Spinoza had refused him permission to show it to Leibniz. On his journey from Paris to Hannover in 1676 Leibniz met Spinoza, and in 1678, having read the *Ethics*, he confided to Justel that he regarded the book to be dangerous for those who wished to master it. Leibniz's interest in, and rejection of, Spinoza's philosophy justifies the inclusion in a Leibniz Congress of a section on Spinoza.

In the first contribution to this volume, G. H. R. Parkinson distinguishes between two types of problem concerning the relation of theory and practice—the general theme of the Congress. One set of problems belongs to philosophy of science and the other to the philosophy of mind. His paper on 'Spinoza's concept of the rational act' is concerned with the latter. André Robinet finds that Spinoza and Leibniz have in common a refusal of all metaphysics which is founded on a radical separation of theory and practice. They agree in rendering man homogeneous with nature and the act coherent with its conception but they separate in the analysis of the internal content of this rationality. Eduard Winter discusses the relation of theory and practice in the work of Tschirnhaus and Jean-Paul Wurtz contributes a paper on the relations of Tschirnhaus with Spinoza.

A number of papers are devoted to discussion of various aspects of Spinoza's philosophy and its influence on later thinkers, while the majority are concerned with a comparison of the ideas of Spinoza and Leibniz on such themes. Historians of science may be especially interested in Sergio Cremaschi's analysis of the concepts of force in Spinoza's psychology, which he relates to the

Newtonian concepts. In his account of external and internal freedom in the systems of Spinoza and Leibniz, Waldemar Voisé remarks on the difficulties of adequately covering within the confines of a short paper problems to which others have devoted whole volumes. Other authors must have experienced similar difficulties. Yet some of the papers in the volume—among them that of Albert Heinekamp on Spinoza's ethics—do succeed in providing valuable new insights.

E. J. AITON, *Manchester Polytechnic, Didsbury School of Education, Wilmslow Road, Manchester M20 8RR, England*

JOHN P. WRIGHT, *The sceptical realism of David Hume*. Manchester: Manchester University Press, 1983. xii + 269 pp. £19·75.

Rejecting the rational reconstructions of contemporary analytic philosophers, John Wright has attempted to develop a truly historical interpretation of David Hume's philosophical enterprise which does justice to both the sceptical and realist elements in Hume's works. According to Wright, Hume self-consciously adopted sceptical arguments to show that our ideas of space, time, matter and causation are inadequate representations of reality. The point of such arguments was not, however, to demonstrate that our belief in the objective existence of material substance and a spatio-temporal causal order is epistemologically groundless. Rather, in Wright's view Hume wanted to draw attention to the fact that our fundamental beliefs are based not on our ideas but on natural judgements of the human mind arising from the mechanical operations of the faculty of the imagination. While knowledge and belief are thus not ultimately founded on reason, on this reading they are nonetheless rooted firmly in human nature. Elaborating on this general thesis, in successive chapters Wright clarifies the sense in which Hume was a sceptic, examines Hume's analysis of our belief in body and our ideas of quantity, space, matter and causal powers, and delineates Hume's science of human nature.

Perhaps the most original and stimulating aspect of Wright's study is his discussion of the physiological assumptions underlying Hume's theory of the mind. For too long seventeenth- and eighteenth-century texts on the human mind have been read in terms of modern disciplinary boundaries. Consequently sharp distinctions have been drawn between epistemology, psychology, and physiology, distinctions which thinkers of the period would not necessarily have recognized. Wright eschews such an approach, and argues persuasively that Hume's conception of perception and imagination was informed by physiological ideas derived largely from Malebranche's *De la Recherche de la Vérité*. Guided by the disciplinary presuppositions of modern philosophy, previous writers have overlooked Hume's use of Malebranchean physiology, and it is to Wright's credit that he attends to this facet of Malebranche's influence on Hume. Moreover, Wright's account of the physiological component of Hume's concept of the mind has suggestive implications for further research on the development of philosophy and physiology in eighteenth-century Britain.

Although scholars have not explored the subject in any detail, it is reasonably clear that Locke's 'way of ideas' was premissed on a mechanistic physiology akin to that of fellow medical men such as Willis. Berkeley too acknowledged that theories of vision encompassed anatomy and physiology as well as the analysis of our ideas, and he may well have believed that theories of the mind were similarly constituted. Now that Wright has revealed the physiological dimension of Hume's speculations, we can begin to see that there was an important physiological context within which the theory of ideas evolved. Contemporaries were certainly aware of this context. Thomas Reid, for example, attacked both the theory of ideas and the physiological theories associated with it in his *Inquiry into the Human Mind on the Principles of Common Sense* (1764), going so far as to suggest that the physiological hypotheses he surveyed there were founded on the theory of ideas rather than *vice versa*. While he insisted that the relationship between mind and body is ultimately mysterious, Reid nevertheless believed that the laws governing that relationship could be ascertained, and in this he was influenced by his kinsman Dr John Gregory, who had been taught at Leyden by Boerhaave's successor Jerome Gaub. As his manuscripts show, Reid was also well acquainted with the works of such major figures in eighteenth-century physiology as Stahl, Haller, and Whytt. Reid is thus another instance of an 'anatomist of the mind' who shared a common physiological context with physicians and philosophers alike. Those interested in the history of philosophy and physiology would therefore do well to investigate this common context in the future, as it may shed considerable light on the nature of these practices in the Enlightenment.

Wright's interpretation of Hume may prove controversial in philosophical circles, but it has much to offer historians of science and medicine. By challenging current historiographical orthodoxy, Wright provides potentially fruitful insights into the structure and evolution of the science of man in the Age of Reason.

P. B. WOOD, *Department of Philosophy, University of Leeds, Leeds LS2 9JT, England.*

Philosophical Aspects of Science

C. DILWORTH, *Scientific progress—A study concerning the nature of the relation between successive scientific theories.* Dordrecht: D. Reidel Publishing Company, 1981. 155 pp. Dfl. 70/ $29·95.

This book provides an extremely clear description and critique of the best known contemporary versions of philosophy of science, and a very suggestive if incompletely worked out solution of the general problem of scientific progress. The author describes the strongpoints and especially the weaknesses of the deductive model used by logical empiricists and Sir Karl Popper, the attempt of Lakatos, Kuhn and Feyerabend to transcend the model, and the deficiencies of the set-theoretic approach of J. D. Sneed. Dilworth is particularly acute in pointing out the similarities in the logical empiricist and Popperian methodologies of science and the manner in which many of Popper's ideas are not consistent with his own reliance on the deductive model.

Dilworth's own theory of 'Gestalt change' and what he calls 'the perspectivist conception of science' is an attempt to interpret the notion of incommensurability between theories (not the same as suggested by Kuhn and Feyerabend) as a *positive* solution to the following problem: 'The empiricist view affords no conception of theory conflict, and the Popperian view provides no consistent conception of progress itself' (p. 60). He emphasizes 'the Gestalt model of theory succession' by means of the well-known duck-rabbit picture where incommensurable theories can be alternately understood by starting from different points of recognition. The superiority of one theory to another and hence the reality of scientific progress exists in the fact that one of the incommensurable theories has more accuracy, scope, and simplicity than the other. (The model is either somewhat more like a duck or like a rabbit.)

Dilworth, following Campbell, sharply distinguishes between laws and theories: the first are formal and the second are not. He also claims that scientific theories are about what is empirical, but unlike his Gestalt examples which are taken from sensory experience, what he means by 'empirical' is for the most part trans-sensory. He does not reject the popular euphemism of calling atoms and other physical objects 'theoretical entities' but wants to leave the question of their reality open.

The book is so clear, informative, and suggestive that this commentator's only criticism is that it is too brief and in its emphasis on methodological problems in science and philosophy of science no room has been left to explicate the author's presupposed foundation theory. This is especially unfortunate, since it is precisely differences in foundation assumptions which appear to be most responsible for apparent incommensurabilities between scientific theories. For example, people who believe in universals and those who do not will commonly understand the nature and importance of scientific laws very differently. Also, people who identify the external world in direct empirical or conscious terms will often have trouble understanding theories based on the notion that the external physical world is always and necessarily beyond what can be made empirical or conscious.

Dilworth is quite correct that apparent incommensurabilities can almost always be penetrated by those who are patient and *uncritical* enough to persist in studying alternative theories, but if he had emphasized the role of foundation theory and foundation assumptions he would have given the reader a better notion of *where to look* to make seemingly incommensurable theories in science understandable and hence scientific progress itself more comprehensible. He is very good at directing the reader to relevant literature. In spite of the brevity of the book, there is a good bibliography and index.

As for the notion of understanding the empirical world in Gestalt terms as a way of transcending both logical empiricism and the indirect realism of Planck, Einstein and Popper, this approach has recently been developed in the still untranslated work of the Japanese philosopher Wataru Hiromatsu.[1] Following the lead of Dilworth and Hiromatsu (who have very different foundation assumptions) many historians and philosophers of science may soon discover how a Gestalt approach to methodological problems may well give rise to a whole new way of understanding them.

JOHN BLACKMORE, *Institut für theoretische Physik der Universität Wien, Boltzmanngasse 5, A-1090 Vienna, Austria*

[1] My thanks are due to Setsuko Tanaka for her general account of Hiromatsu's philosophy. Hiromatsu has also written with Ryoichi Itagaki a still untranslated book on Mach and Einstein.

Springs of scientific creativity: essays of founders of modern science, edited by R. ARIS, H. T. DAVIS, and R. H. STUEWER, Minneapolis, Minn.: University of Minnesota Press, 1983. ix + 343 pp. $32·50.

The editors tell us in their Preface that, in planning the series of lectures on which this book is based, they did not expect to achieve any 'essential characterization' of creativity. Instead, they simply asked for accounts of the intellectual lives of several creative thinkers. The result is a collection of essays, mostly on outstanding physicists, some quite good in their own ways, but with little coherence as a book.

Only one author, C. W. F. Everitt in a 70-page study of James Clerk Maxwell, seems to have taken the title of the book seriously and tried to find the *sources* of his subject's remarkable achievements rather than merely describe them. Thomas P. Hughes devotes a large part of his much shorter chapter to an explicit comparison of the creativity of the inventor Elmer Sperry and Thomas Mann's fictional composer Adrian Leverkühn, but does not provide the extensive family and personal background which lends plausibility to Everitt's analysis of Maxwell. Other contributors—Richard S. Westfall on Newton, John N. Howard on Lord Rayleigh, and Erwin N. Hiebert on Nernst—address this issue in brief remarks tacked on to their chapters, but do not systematically explain how discoveries were made. In fact, Hiebert states explicitly that scientific creativity should be analysed 'by trying to illuminate the historical and contingent circumstances of scientific discovery rather than honing in, *post facto*, on the logical or reconstructive dimensions of what happens during acts of creative insight' (p. 226).

Everitt demonstrates that a third alternative is sometimes possible. In addition to describing the historical context in the broad sense, but without attempting to determine precisely what thoughts were passing throung the scientist's mind at the instant of discovery, the historian can learn enough about the heritage and psychological background of the scientist to gain some understanding of the dynamics of his or her personality as it may affect creativity. Thus, for example, Everitt argues that Maxwell sought pain and conflict, and was strong enough to force into constructive paths emotions that would be debilitating to others less gifted or tenacious. His weaknesses in dealing with other people were more than coincidentally related to his power in dealing with ideas and facts.

In most cases I would agree with Hiebert that it is more helpful to study the context of discovery than to speculate about the psychological processes in the mind of the discoverer, simply because in most cases we have so little reliable information about the latter. As it happens, however, this book also provides an example of a careful study of context which, though quite admirable as a piece of scholarship, leaves this reader more confused than enlightened about a major discovery. Thomas B. Settle reports an experiment carried out by Donald R. Miklich to test the claim by Galileo and several sixteenth-century writers that, when a person drops a light and a heavy object at the same time, the *light* one falls faster (at least at first). Photographs of iron and wooden spheres dropped by students show that Galileo's claim was correct in 88% of the trials. Settle carries the contextual anti-Whig doctrine to an extreme by refusing to give any explanation of this curious phenomenon in terms of modern physics.[1]

If one ignores the overly-ambitious title of the book and its lack of unifying themes, one finds many valuable features. The authors are generally quite knowledgeable experts on their subjects: D. S. L. Cardwell on Joule, Martin J. Klein on Gibbs, Stanley Goldberg on Einstein, Linda Wessels on Schrödinger, William T. Scott on Polanyi, Herman H. Goldstine on von Neumann, in addition to those mentioned above. They write well and have interesting (if not always new) things to say. In view of the scarcity of books on the history of physics which are both readable and accurate, I would recommend *Springs of scientific creativity* as quite suitable for an advanced undergraduate course, and for physical scientists with a nascent interest in history.

S. G. BRUSH, *Department of History and Institute of Physical Science & Technology, University of Maryland, College Park, Maryland 20742, U.S.A.*

[1] See B. L. Coulter and C. G. Adler, 'Can a body pass a body falling through the air?', *American Journal of Physics*, 47 (1979), 841–46 for a plausible explanation.

Bibliography and Reference

Cinq cent ans de bibliographie hippocratique, edited by G. MALONEY and R. SAVOIE. Quebec: Les Éditions du Sphinx, 1982. v + 291 pp. No price stated.

This book lists under date of publication those editions, commentaries and discussions of 'Hippocratic' texts which were published between 1473 and 1982. A preface hints at some of the inevitable restrictions which have been applied to this vast field; the emphasis rests firmly on western Europe and the Americas, ancient commentators are omitted, and discussions of the role of 'Hippocratic' theories in modern medicine are set aside in favour of philology.

Repetition, omission and simple typographical errors may perhaps be excused in an undertaking on this scale, if the end result is particularly valuable. Unfortunately the arrangement by date makes the book difficult to use, unless one's interests lie in determining which text was in vogue at a given date, or in watching the patterns of reprinting across the world. Is it significant that 1555 (19 entries) was a 'good' year, while 1553 (5) was not?

There is an index of authors, but this is little consolation in the case of a prolific author/translator such as Leoniceno or Cornarius, after each of whose names over 100 entries appear without further subdivisions. An index by text would have been useful, as would some distinction between edition, commentary and comment.

In some cases the practice of giving titles in full leads to unnecessary repetition; in others, the description is not full enough. It requires some prior knowledge to know that no. 420, which appears to be 1567 edition of Aesop's *Fables*, is in fact included because pp. 126–8 give the *Oath*. The rationale behind other entries can only be guessed at; 774, the *Hygieia*, at least includes sections based on Hippocrates, while 860, *Hippolytus Redivivus* of 1644, does not even mention his name. The general reader will be confused, the specialist frustrated; but it is to be hoped that each will at least find something new in the 3,332 volumes listed.

HELEN KING, *Newnham College, Cambridge CB3 9DF, England*

Eastern and Oriental Sciences

J. NEEDHAM, with the collaboration of LU GWEI-DJEN, *Science and civilization in China*, Volume 5, *Chemistry and chemical technology*, Part V, *Spagyrical discovery and invention: physiological alchemy*. Cambridge: University Press, 1983. xxxiii + 574 pp., 94 illust. £49·50.

Until fairly recent times the would-be interpreters of alchemical texts were divided into two camps—those of chemical persuasion who saw in them nothing but vain attempts to transmute base metals into gold or silver or to prepare an elixir of life, while others of theosophical bent regarded alchemy as an allegorical expression in chemical terms of a spiritual quest, namely, the redemption of man. Present opinion, based mainly upon a knowledge of Hellenistic philosophy and Gnosticism, regards both views as compatible in so far as they represent the two facets of a unified concept of matter and spirit. The late C. G. Jung offered a plausible explanation of the alchemical *opus* in psychological terms, particularly convincing in respect of the imagery displayed in European alchemical texts. All of this is ably described by Dr Needham, and together with the information contained in the previous parts of Volume V forms a concise history of European alchemy.

When Chinese alchemical texts became known to westerners even the earliest interpreters, such as Edkins and Martin, recognized that there, also, appeared a dualism, though they were somewhat unclear as to its nature. The dualism was markedly different from that displayed in the European works; whereas western alchemy was regarded as either practical or spiritual in nature, that of the east consisted of *wai tan*, the attempt to attain longevity or even physical immortality through the ingestion of a prepared elixir, and *nei tan*, an art employing various means by which physiological changes brought about within the body of the adept could halt the ageing process.

In Part V Needham and Lu Gwei-Djen give what is surely the first detailed and accurate account of the nature of, and the practices involved in, *nei tan* (though Lu earlier dedicated to Needham a useful review of the subject); admittedly, earlier writers such as O. S. Johnson (1928) and Arthur Waley (1930) recognized the difference between *wai tan* and *nei tan*, though introduced some confusion by their use of the terms 'exoteric' and 'esoteric' for the respective practices. Clearly, there is no likeness between western 'spiritual', or 'psychological', alchemy and *nei tan*; the processes involved in the latter consisted essentially of physiological changes induced within the adept's body, while the former operated through mental processes perhaps explicable in the psychological terms of Jung.

The ideas underlying the feasibility of attaining longevity or physical immortality derive from pre-Han times (before 200 B.C.) and have been examined in previous volumes. Needham now discusses in great detail the essence of *nei tan*, its development and the techniques employed; like many other oriental arts it sounds a strange story to western ears.

Though the term *nei tan* does not appear in the literature until around the time of the Sui dynasty (A.D. 581–618), the first text to propound the theories behind both *wai tan* and *nei tan* was almost certainly the *Tshan Tung Chhi* of Wei Po-Yang (around A.D. 140). Both elixir alchemy and physiological alchemy operated side by side up to about A.D. 800, after which time the former began to decline. Their manuals frequently shared a common nomenclature, though in the case of *nei tan* the use was symbolic in that chemical imagery was employed to denote physiological reality.

The key to the attainment of longevity and immortality lay in a reversal of the ageing process—a return to youth—hence various practices were adopted in the attempt to achieve that aim. Such techniques as breath control, gymnastics, sexual practices, meditation, phototherapy (exposure to the rays of the sun and moon) etc., were intended to produce within the adept's body an 'inner' elixir for the restoration of youth—or, to use words coined from the Greek by the authors, an 'anablastemic enchymoma'. One important consequence of this was thought to be a reversal of the flow of certain body fluids, especially saliva and semen; in this way was it thought that the process of bodily decay was reversed and that the individual regained the state of a newly-born child. The 'primary vitalities' possessed by such a child—*ching*, *ch'i* and *shen* (semen, breath and spirit)—and their patterns of deterioration with growth are discussed very explicitly with reference to the many texts emerging during the history of *nei tan*; particularly valuable is the wealth of photographs and line-drawings associated with the many processes thought to occur within the adept's body.

As was mentioned earlier, there was a borderline between *wai tan* and *nei tan*, and this is interestingly considered, as is the possible interrelationship between the latter and Indian tantric Yoga practices. Ideas seem to have flowed in both directions, though there is some evidence to suggest the influence of Taoism on Tantrism.

Long before the demise of physiological alchemy, the laboratory process petered out, mainly on two scores: the poisonous nature of many metallic and mineral elixirs and also the lack of progress of the protochemistry of the times. Hence, it is hardly surprising to learn that the human body came to be regarded as the true laboratory; in this way 'the quest for rejuvenation was, in our view, an anticipation of endocrine physiology', say the authors. The work concludes with an interesting section showing how Chinese iatrochemists from the tenth to the sixteenth centuries A.D. were successful in preparing from urine mixtures of androgens and oestrogens for medical uses—an achievement antedating western endocrinology by several hundred years.

With this, Part V of Volume 5, the story of Chinese alchemy ends, as never told before. As a comprehensive work it is unlikely to be superseded except, perhaps in some few details; the style is what we have come to expect from Dr Needham—lucid and frequently enlightened by flashes of intuition and even wit. Like previous Parts, it is clearly printed, extremely well-indexed and repeats with addenda the magnificient Bibliography which in itself is a mine of information. It is difficult to give adequate praise to such a work; those interested in western alchemy will find in it a fascinating contrast to the modes of thought and purpose with which they are familiar—but at a price that they may not be able to afford!

H. J. SHEPPARD, *The Lodge, Blackdown, Leamington Spa, England*

Technology and Engineering

RICHARD H. SCHALLENBERG, *Bottled energy, electrical engineering and the evolution of chemical energy storage*. Philadelphia, Penn., American Philosophical Society, 1982. (*Memoirs*, Volume 48.) xvi + 420 p. $20·00.

The storage battery, for so long taken for granted, is once again the focus of innovation. It is therefore apt that a detailed account of the development of the modern storage battery has been written by the late Richard Schallenberg. He shows how the lead–acid storage battery was originally invented ahead of any demand for such a battery, and how it survived several changes of end-use to finally achieve maturity as the energy-source for the starter motor in the petrol-powered automobile. We are, therefore, presented with a case-study in the 'evolution' of a technological artefact in the course of which the 'environment' changed as well as the storage battery. Schallenberg is particularly interested in the interactions between the changing pattern of demand for batteries and the response of the electrical engineers to these changes.

The lead–acid cell was originally developed as a primary battery for use in telegraphy, but the advent of electricity generation led to its being used as a storage battery to improve the load-factor of DC electricity generation during the 'electrical revolution' of the 1880s. When the 'Battle of the Systems' was settled in the 1890s in favour of AC generation, the storage battery was exploited as the motive-power in trolley-cars and electric automobiles. Although electrically-driven transport employing storage batteries was soon found to be impractical except over very short distances, this episode bridged the gap between the earlier period and the large-scale employment of batteries in the petrol-driven motor-car from the 1920s onwards. This 'bridging of the gap' is very interesting and can be found in other branches of technology. The book ends with a look at the relatively unsuccessful alkaline battery. It is well printed, and is equipped with a good index. However, like may modern books with stiff covers the binding is suspect. The spine of my copy became cracked after only one reading.

The main difficulty with *Bottled energy* is that Schellenberg clearly writes with several aims in mind and there exists a degree of conflict between them. On one plane he is writing a narrative history of the storage battery with explanations why certain events occurred. For example, he argues that the uncharacteristic American lag in the 1880s was a result of Edison's opposition to storage batteries, and he shows that this gap was eliminated in the 1890s. Unfortunately this narrative is very detailed and often technical. Ideally the reader requires a good understanding of battery technology, and the general reader may well find the book heavy going. On another plane Schallenberg picks up the interaction between technology and society, demonstrating for instance that the DC distribution system and the electric car were a product of wealthy people living in the city centre. Once they moved out to the suburbs at the turn of the century, the storage battery had to find a new *raison d'être*.

At these two objectives Schallenberg is reasonably successful, but it is clear that he had a third aim in mind, namely the thesis that technology does not act exogenously on society; it evolves and interacts with its technological and economic 'environment'. Leaving aside the question of how far this is true, and I believe that he over-stresses the biological analogy, it is evident that the case-study is not subordinated to this aim. Indeed it is difficult to find any discussion of the question within the text except for the point that the early electrical engineers and battery innovators were strongly influenced by their involvement in telegraphy. To make it more difficult, such references that exist are buried within the narrative and are easily missed. At no point does

Schallenberg attempt to discuss battery development as an example of innovation, nor does he refer to the modern literature concerned with technological innovation. Hence his idea of an artefact evolving alongside its environment is considered in isolation from any other technological development, and from any other theories of technological development or innovation. It should also be said that the book is rather long with 397 pages of text, and some editing would have made the book easier to read.

Nevertheless, Schallenberg's monograph is an important and informative contribution to a much neglected field of the history of technology and it should be a key case-study for the debate about technological innovation. It will be of interest to scholars working on the development of electrical transport and the motor-car as well to historians of the early electrical industry.

PETER J. T. MORRIS, *Faculty of Arts, Open University, Walton Hall, Milton Keynes, Buckinghamshire MK7 6AA, England.*

Instruments and Measurement

MICHAEL A. CRAWFORTH, *Sovereign balances: 1—standard rockers*. Published in England (Sunderland) for the International Society of Antique Scale Collectors, 20 North Wacker Drive, Chicago, Illinois 60606, U.S.A., 1983. 48 pp. £3·00.

The English sovereign rocker was the most popular balance for the detection of counterfeit coins of the nineteenth century, being simple and pocket-sized. This particular design appeared in 1817 at the same time as the new sovereign and half-sovereign, although the principle had been in use earlier. As the author points out, the principle is that of the ducking stool for a scold.

This booklet deals with the history, design and technical development of the instrument, and, unusually for a 'collector's' book, with the performance. The author has tested 87 of the same type of balance, and shows that only one was dead accurate, 37 were accurate to one grain, and the worst accurate to only seven grains.

There follow a list of makers, chiefly in Birmingham, 17 transcriptions of makers' labels photographs (20) and line drawings (60) of a variety of balances. Ten items are listed under references. This is a useful addition to the literature on technical, social and economic history.

G. L'E. TURNER, *Museum of the History of Science, Broad Street, Oxford OX1 3AZ, England*

Mathematics and Logic

The Scottish Book: Mathematics from the Scottish Café, edited by R. DANIEL MAULDIN. Boston, Basel and Stuttgart: Birkhäuser, 1981. xiii + 268 pp. Sfr. 54.

The Scottish Book originated not in Scotland but in Lwów, Poland! (The city is now in the U.S.S.R.) In the 1930s there was a very active group of mathematicians, both teachers and students, in Lwów. Stefan Banach (the outstanding mathematical analyst after whom Banach spaces were named) and S. Ulam, S. Mazur, and M. Kac were members of the group. Beyond the formal lectures they frequented two cafés, the Café Roma and later the Scottish Café, in order to discuss the problems of mathematics that interested them. Eventually Banach decided that it would be a good idea to record the problems which they investigated, so he bought a large well-bound notebook in which to write these down. The notebook was kept at the Scottish Café. The first problem was entered on 17 July 1935 and the entries continued until 31 May 1941. Thus in the Scottish Book we have a record of the energetic mathematical activity of a group of keen students and teachers and also of some visitors to the group.

After the group ceased to meet (because of the war) the book was somehow preserved. Much later, in 1957, Ulam translated its contents into English and distributed copies of his translation, thereby renewing interest in this piece of mathematics in the making. By 1979 there was sufficient interest in the book that a conference about it was held at North Texas State University. The conference included lectures by some of the Poles who were involved with the original café group and by others who were active in mathematics at the time.

The volume under review contains a translation of all the 193 problems of the original Scottish Book, together with solutions and commentaries for some of them. The problems clearly show the broad range of interests of the group. The volume also contains five introductory essays which derive from the lectures given at the Texas conference. In the first essay, 'An Anecdotal History of the Scottish Book', Stanisław Ulam fills in the background to the book, while in the second essay, 'A Personal History of the Scottish Book', Mark Kac gives his own view. A. Zygmund, who was not a member of the group in Lwów, offers a more general picture of Polish mathematics between the wars in his essay 'Steinhaus and the Development of Polish Mathematics'. Paul Erdös provides further problems related to those in the original notebook in his contribution 'My Scottish Book "Problems"'. Finally Andrzej Granas presents a technical mathematical essay on 'KKM-Maps and their Applications to Nonlinear Problems'. The volume also contains an introduction by the editor, R. D. Mauldin, and two prefaces by Ulam reprinted from the 1957 and 1977 editions which he made of the Scottish Book.

This volume will be of interest primarily to mathematicians rather than to historians of mathematics and science. It is a record of mathematical activity with some commentary, a kind of mathematical scrapbook. The historical recollections are largely anecdotal in character. Still, the value of the volume should not be denied. Between the two world wars Poland was one of the leading places for mathematical research, particularly in logic, set theory, topology and analysis. The impressive record of problems attacked by the lively group of mathematicians meeting in the cafés of Lwów illustrates this fact nicely. By examining this edition we get something of an insight into how mathematicians go about creating their subject. Problems, after all, are the very lifeblood of mathematics.

DALE M. JOHNSON, *Department of Mathematics, The Hatfield Polytechnic, Hatfield, Hertfordshire AL10 9AB, England*

J. SESIANO, *Books IV to VII of Diophantus' Arithmetica in the Arabic translation attributed to Qusṭā ibn Lūqā*, (Sources in the History of Mathematics and Physical Sciences, Volume 3.) New York, Heidelberg, Berlin: Springer-Verlag, 1982, xii + 502 pp. $77·60.

Sesiano's book is one of the most important books published in recent years on the history of algebra. It fills a gap which has been long felt in the history of that science and specially in our knowledge of the production of the greatest algebraist of Antiquity.

The *Arithmetica* of Diophantus (*ca.* A.D. 250) originally constituted thirteen books devoted to the treatment of determinate and indeterminate equations of degree $\leqslant 9$ with rational positive solutions. Unfortunately, six books only out of the thirteen books have come down to us in the original Greek text. This loss has been recently alleviated to a very great extent by the discovery of the manuscript of the Arabic translation of four other books of the *Arithmetica.* This translation is attributed to Qusṭā ibn Lūqā (fl. *c.* 860), a Christian of Greek origin who lived in Syria and Iraq and died in Armenia. The manuscript belongs to the library attached to the Shrine of Imam Reza at Mashad (Iran). It is listed in the catalogue of the mathematical manuscripts of the library, published in 1971 by A. Gulchin-i Ma'ānī; and it was Professor G. J. Toomer who, early in 1973, informed Sesiano of the existence of the manuscript. It is to this Arabic translation that Sesiano's book is entirely devoted.

The book is composed of five parts: an Introduction, an English translation of the text, a mathematical commentary, the critical edition of the Arabic text and an Arabic Index. To these five parts, Sesiano added a conspectus of the problems of the *Arithmetica,* a very extensive bibliography, and a general index (itself divided into six parts).

In the Introduction (82 pages) Sesiano proceeds to an exhaustive study of the Arabic books from a historical and philosophical point of view. After having demonstrated that the books contained in the Mashad manuscript are authentic parts of the *Arithmetica*, Sesiano proceeds to show the order in which these Arabic books should be placed in the whole of the *Arithmetica.* Thus he demonstrates that the Arabic books IV and V must follow the third Greek book and that, although there are no decisive arguments for the place of Arabic books VI and VII, these must follow IV and V because the only prerequisite needed for their understanding are the first three Greek books and because they are easier than the three others.

Sesiano proceeds then to an exhaustive history of the *Arithmetica* in the Arabic and Byzantine worlds. He traces all quotations of Diophantus found in Arabic authors such as Abū l-Wafā, al-Karajī and others. After that, he undertakes a very detailed study of the different Greek manuscripts which existed in Byzantium, showing how and by whom they were commented and explaining how they contributed to the revival of Greek mathematics.

As for the extant Arabic text, Sesiano devotes to its philological analysis a very lengthy chapter II in which all aspects of the text (grammatical aspects, commentaries which have been made of it, interpolations, peculiarities of its mathematical expressions etc.) are thoroughly studied. Finally, in the third chapter of the Introduction, he proceeds to a 'Tentative reconstruction of the history of the *Arithmetica*' in Greek and Arabic as well, in which he studies the genuinness and origin of some problems of the ten actual extant books. The chapter ends with a remarkable genealogical tree of the Mashad manuscript.

The second part of the book is devoted to the English translation of the manuscript (87 pages). Let us immediately say that, on the whole, it is almost perfect. A few criticisms can be made of it, which we shall mention later on. Sesiano has fortunately followed the golden rule in such translations, namely in translating literally the Arabic text. He also reproduces in the right hand margin the lines of his edition of the Arabic text, so that the reader who knows Arabic may immediately compare between the translation and the original text.

The mathematical commentary follows step by step the problems of the four books (44 problems in the fourth book, 15 in the fifth, 23 in the sixth and 18 in the seventh). The commentary consists in writing in almost modern form all the calculations, which are written in words in the Arabic text, adding to them very often very useful explanations and commentaries. These either explain the calculations and solutions of the problems or give some general characteristics of the problems and comparisons between them, with sometimes very interesting historical remarks.

The fourth part of the book is devoted to the Arabic text (147 pages). In the right hand margin the author has numbered the lines of the text, five by five: the whole text is composed of 3,590 lines. In the left hand margin, he has indicated, as in the translation, the pages of the Mashad manuscript. The critical apparatus is remarkably precise and exhaustive.

The fifth part of the book is an Arabic Index containing all important words encountered in books IV–VII of the *Arithmetica.* It is ordered by roots and gives generally the Greek equivalent of the Arabic words as well as the most important occurrences of the word in Arabic books of algebra.

Finally, the book ends with a conspectus of the problems of the whole of the actually known *Arithmetica*, i.e. the four Arabic and the six Greek extant books. This conspectus is followed by a very extensive bibliography and a general index, which is itself composed of six parts: critical notes, manuscripts used or mentioned, Greek scientific words defined or discussed, authors, the problems of the Greek *Arithmetica* referred to in the book, and finally an index *rerum.*

The printing of Springer-Verlag is excellent. The equations are printed in a very clear and very precise way even when printed in small type in the footnotes. The Arabic text, although typed and not printed, is very clear and very carefully done.

Few criticisms can be made of this scholarly and remarkable work; we shall just mention some of them.

Concerning the plan of the book, the study of some points is scattered sometimes through different chapters (e.g. the comments on the relations between al-Karajī and the *Arithmetica* (*passim*, Part One, chap. I, pp. 10–11; chap. III, pp. 57–60) or the study of the interpolations (*passim*, Part One, chap. II, pp. 30–4; chap. III, p. 53) or the study of the extant Arabic manuscript in chap. II and its genealogical tree in chap. III). One would have also liked to have in the Introduction or at the beginning of the mathematical commentary a synoptic view of the content of each of the four Arabic books and the specific characters of each of them. Of course, Sesiano gives some general comments at the end of books IV, VI and at the beginning of book VII (beside the very interesting specific comments he adds very often a modernized version of the problems). But the reader has to go through the commentary of every book to discover the general comments, as they are not mentioned in the table of contents, and the only reference to them is in the general index (index *rerum*): 'Character of the Arabic books' (p. 501).

As for the translation, which Sesiano rightly wanted to be literal, it is somewhat surprising to see some important Arabic mathematical expressions translated in modern symbolism. Thus the word '*šay*'' (thing, *res*) which designates the unknown in the algebraic Arabic texts is always 'translated' by 'x'. The Latins used the *res*; why not use 'thing' in English? As for the important expressions of *māl*, why not try to have found an English translation of this expression (e.g. 'power', or simply borrowing the Greek word 'dynamo' instead of 'translating' it by 'x^2'? It would have been much more genuine to read in the English translation expressions like: 'dynamo–dynamis' instead of 'x^4' or 'dynamo–cube' instead of 'x^5'. The substitution of modern symbolism for Arabic literary expressions deprives the text of some of its conceptual wealth. The understanding of the text would not have been lessened by that, as Sesiano gives in his mathematical commentary the entire calculations of all the problems in almost complete modern symbolism. The same can be said of the word 'coefficient' (p. 92) which has no equivalent word in Arabic, and of the word 'dividend' (p. 147). Also, in writing the numbers, the Arabic text always adds to them the word '*aḥad*' (one). So the numbers are always explicitly stated as a certain number of 'ones', whereas Sesiano does not add the 'one' in the translation. For large numbers, of the order of millions, Sesiano writes them simply in full numbers. For instance, he writes (p. 125): 19,518,724, which the European reader will of course read: 19 millions... without being aware that the word 'million' does not exist in the text, which states literally: 10 thousand thousand...

Concerning the editing of the Arabic text, which is so scrupulous and precise, one may just ask if it was absolutely necessary that the critical apparatus be written in Latin. Why not in Arabic or simply in English? Today Latin is unknown to readers of such books.

As for the general index, we think that three remarks obtain. The first is that a short note at the beginning of the index indicating the signification of the different numbers used would have made it easier for the reader to make use of this index. Concerning the Arabic index, perhaps would it have been sufficient to keep it to the limits of the technical words occurring in the Arabic text with the proposed English translation (beside the Greek words given by Sesiano). Too many literary words are listed which are not quite relevant to the subject. Also, as the words, written in transliteration, are obviously ranged following their roots and the Arabic alphabet, the roots should have always been mentioned and typed in particular type so that the reader may easily find the word he is searching for. As to the index *rerum*, it is not in alphabetical order as is usually the case.

Finally, we think that Sesiano should have mentioned, at least in the bibliography, R. Rashed's edition of Ibn Lūqā's Arabic translation of books IV–VII of Diophantus[1] and his two lists of the problems put in modern form, preceded by a small introduction.[2] Of course, Rashed's work is hasty and his critical apparatus is very poor, almost nonexistent. Moreover, his commentary is reduced to a minimum and his work, on the whole, contains many mistakes. This is why it has in fact become completely obsolete after the publication of Sesiano's book. But still, it should have been mentioned.

At the end of his Preface, Sesiano says that his book is his first one. As they say in French: 'Pour un coup d'essai, ce fut un coup de maître'.

KHALIL JAOUICHE, *C.N.R.S., Centre Alexandre Koyré 12, rue Colbert 75002 Paris, France*

[1] R. Rashed, *Sinā 'at al-ǧabr li-diophantus*; al-munša'a al-miṣriyya li l-kitāb, Cairo, 1975.
[2] R. Rashed, 'Les travaux perdus de Diophante', *Revue d'Histoire des Sciences*, 27 (1974), and 28 (1975).

Physical Sciences

An inventory of published letters to and from physicists, 1900–1950, edited by BRUCE R. WHEATON and J. L. HEILBRON. (Berkeley Papers in History of Science, no VI.) Berkeley, California: Office for History of Science and Technology, University of California, 1982. viii + 102 pp., 4 microfiches. $20·00.

This thin volume is part of a set of tools prepared by the authors. In 1981 they issued *Literature on the History of Physics in the 20th Century*, a compendium of full citations, each identified by a letter and number key. The following year this *Inventory* appeared, presenting, as its title states, *published* sources of physicists' corrrespondence during the first half of this century. A companion inventory of some half a million *unpublished* letters in various depositories is due to appear soon.

The *Inventory* is meant to be used with *Literature*, for only the keys appear in the former. Almost 25,000 quotations from 776 physicists' letters are cited. The information is presented on four microfiches. For each item one can learn the author, recipient, date, language, whether facsimile or transcription, whether the quotation is fragmentary or the entire letter, and the page number of the keyed source.

The authors also provide printed lists of the physicists, of non-physicist correspondents, and the sources identified by their authors' names next to their keys.

LAWRENCE BADASH, *Department of History, University of California, Santa Barbara, California 93106, U.S.A.*

Astronomy and Cosmology

GEORGE ERNEST WEBB, *Tree rings and telescopes: the scientific career of A. E. Douglass*. Tuscon, Arizona: The University of Arizona Press, 1983. xiii+242 pp. 16 plts. $19·50.

Astronomy has seen a number of long-running debates during the past century. Sometimes these have resurfaced more than once as sources of controversy, lying submerged and partially forgotten in between times. Beliefs concerning solar-terrestrial relationships provide a good example. The discovery of the approximately 11-year solar cycle in the mid nineteenth century was followed almost immediately by the discovery of a corresponding magnetic cycle on Earth. As the number of sunspots increased and diminished, so did variations in the terrestrial magnetic field. It was widely accepted that this correlation indicated a direct influence of the Sun on the Earth. It was correspondingly natural to look for other examples of such influence. Some, such as the frequency of occurrence of aurorae, were soon established. Others proved less easy to confirm and so led to disputes concerning their validity. This is true, in particular, of the attempts to find a correlation between sunspot numbers and the terrestrial climate.

A. E. Douglass began his astronomical career towards the end of the nineteenth century, when the debate concerning the effect of sunspots on terrestrial weather was at its height. One of the side-issues then being aired, which attracted his attention, was the possible link between climatic conditions and the rate of growth of trees. (J.C. Kapteyn, the eminent Dutch astronomer, had already carried out some studies of this question.) Douglass saw that, if such a link existed, trees might provide excellent evidence for cyclic changes in the Earth's weather. Since a number of cycles would need to be sampled to establish this, it would clearly be necessary to measure rings either from long-lived individual trees, or from groups of trees which had grown in sequence over a long period. In following up this idea, Douglass developed an acceptable method for cross-dating tree sections, and so became a pioneer of dendrochronology. His work on this topic, and the collection of tree sections he built up, had established their value for archaeological dating before the end of the 1930s.

However, Douglass was still concerned to link his data to cyclical variations of climatic conditions. Like many before and since, he found that such cycles were difficult to pin down. He therefore constructed a mechanical device (which he called a 'periodograph') to try and extract evidence for periodic episodes from his 'noisy' data. His analysis led him to the supposition that variations in sunspot numbers were related to the relative positions of the planets and the Sun. This view happens to be old—dating back to not long after the discovery of the sunspot cycle. It is a pity, therefore, that in discussing the background to this part of Douglass's work the author sketches in only part of the history of the idea. It would have been worth explaining why previous attempts had failed.

If, in terms of scientific research, Douglass is now mostly remembered for his contribution to dendrochronology, his earlier involvement with the Harvard College Observatory and subsequently with Percival Lowell is equally interesting to historians of science. Douglass was responsible for selecting the site for the Lowell Observatory (there is a good description of how Flagstaff came to be chosen). Much has been written about Lowell in recent years. Here we are provided with the viewpoint of one of Lowell's assistants: an assistant who, moreover, grew increasingly disenchanted with his employer as time passed. In 1901 Douglass wrote to W. H. Pickering at Harvard: 'It appears to me that Mr. Lowell has a strong literary instinct and no scientific instinct'. In the same year, Lowell dismissed Douglass for his criticism, and, after some delay, Douglass obtained an appointment at the University of Arizona. There he was instrumental in starting the Steward Observatory, another astronomical institution familiar to modern astronomers. His struggles—not least against that twentieth-century bugbear, electric lighting—are well described.

Tree rings and telescopes is a useful addition to the literature on the development of nineteenth- and twentieth-century US science, despite (or, perhaps, because) Douglass is not one of the well-known names. The author makes good use of both archival and secondary sources. The main criticism must be that he does not always do full justice to the astronomical or historical background. For example, his discussions of the solar rotation rate and his comments on astronomical observation in the Southern hemisphere are somewhat confusing. But these are relatively minor deficiencies in the narrative taken as a whole.

A. J. MEADOWS, *Department of Astronomy and History of Science, University of Leicester, University Road, Leicester LE1 7UH, England.*

Earth Sciences

MOTT T. GREENE, *Geology in the nineteenth century: changing views of a changing world.* Foreword by L. Pearce Williams. Ithaca and London: Cornell University Press, 1982. 324 pp. $29·00.

Though recently appeared, Mott Greene's *Geology in the nineteenth century* has already attracted a good deal of attention, incidentally for the misnomer of its title and more importantly for its contents. The book is certainly *not* a comprehensive history of nineteenth-century geology. By establishing a hitherto unrecognized chain of topics and influences, however, Greene helps us to see the inadequacy of our previous histories. In particular, he radically diminishes the prominence hitherto accorded British figures.

One's overall view of the history of geology is necessarily determined to a large extent by underlying assumptions. Greene's approach was unusual in that he began working with late nineteenth-century Continental geologists, then traced backwards. Though this path took him in the direction of such major British doyens as Charles Lyell and James Hutton—considered virtual founders of the modern science—Greene found both surprisingly irrelevant. The founder of *his* geology was instead Abraham Gottlob Werner (1749–1817), whom previous, nationalistic histories of geology, written by British and American authors, had generally dismissed as Hutton's thoroughly misguided opponent.

This same line of influence, Greene found, could then be extended forward into the twentieth century, where it (rather than the Hutton–Lyell tradition) gave rise to Alfred Wegener's theory of continental drift, the grandfather of plate tectonics. Thus, we have previously been all wrong as to which of our esteemed forbears got us to where we are.

Greene is not alone in seeking to demote such past idols as Hutton and Lyell. Having authored the most sweeping of these attempted revisions, however, he is all the more interesting in challenging as well the famous paradigm theory of Thomas Kuhn. 'No credence', Greene declares, 'can be given to the idea that geology has advanced and changed in a series of clear-cut victories of one theory over another, followed by a period of unanimity and orthodox interpretation. In no period from the eighteenth century to the present has such a state of affairs existed across all national boundaries' (p. 15). I shall first review Greene's new-found line of influence, then suggest weaknesses and strengths in his conception.

In chapter one, Greene recalls the well-known opposition of Hutton and Werner, especially as treated in histories of geology by Karl von Zittel, Sir Archibald Geikie, Frank D. Adams and E. B. Bailey, all of whom revered Hutton. (That Zittel, a major Continental historian, did so is an embarrassment Greene prefers not to stress.) Lyell, as often now, emerges as an unfair chronicler of his own science who maligned the name of Werner. (But Robert Bakewell and G. B. Greenough had done so earlier, and we know that one of Lyell's major sources was the French geologist Georges Cuvier.) Gordon L. Davies, in Greene's opinion, began the dethronement of Hutton by demonstrating the traditional nature of his ideas. Following an informative defence of Werner, Greene reviews the debates of their followers. Werner's, not Hutton's, he suggests paradoxically, effected the demise of Neptunist geology—that series of Flood theories so often attributed to bibliolaters and the Wernerians themselves. Werner, however, seen primarily as a systematic stratigrapher and mineralogist, continued to be honoured as a founder of geology by

Continental naturalists, while the more theoretical and somewhat confused Hutton (whom Greene mistakenly represents as a Catastrophist)[1] was, outside of Britain, largely ignored.[2]

From Huttonians and Wernerians, Greene moves on to Uniformitarians and Catastrophists. These paired terms both arose during the 1830s (in a review of Lyell's *Principles of Geology* by William Whewell to distinguish those who believed in slow, uniform geological process over vast periods of time from those who preferred short, violent episodes. A major issue in this debate, never satisfactorily resolved by Lyell, was the origin of mountain ranges. Among Continental geologists, however (for whom Lyell was correspondingly less central), this was the major geological question of the century (p. 76). Thus, while British geologists idly debated aspects of change, followers of Werner—Léonce Elie de Beaumont (1798–1874), in particular—were advancing geology more fundamentally. With his extensive discussion of Elie de Beaumont, Greene's book gains strength. For several chapters thereafter, we follow British, Continental and American reactions to the unsound but provocative theory of global mountain chains which Elie de Beaumont proposed and ceaselessly revised. Several geological theories of unquestioned importance are here discussed adequately for the first time, and the hitherto neglected centrality of several American contributions is evident.

The next major topic, again requiring several chapters, is *The Face of the Earth*, a multi-volume work by Eduard Suess (1831–1914), which was completed only in 1904 after a quarter-century of effort; it, too, has been neglected by historians. Suess, the keystone of Greene's argument, developed a comprehensive geological theory out of the orogenic tradition adumbrated by Greene; he revised the history of geology as well, demoting Hutton and Lyell as Greene has done. A major response to Suess's earlier work was renewed theorizing about the creation of the Alps. This in turn (Greene asserts, but surely it had occurred earlier) made structural analysis integral to geology, a contribution as central to its progress as William Smith's stratigraphy had been. Structural analysis assumed global proportions in later theories and was soon allied with geophysics. This latter area had a prior history as well, including the concept of isostasy and a theory for the origin of the moon which Greene erroneously attributes to George Darwin.[3] Finally, Greene connects the work of Suess, T. C. Chamberlain, Frank B. Taylor, and other figures with that of Alfred Wegener (1880–1930), who substantially originated the idea of continental drift. Greene's review then ends arbitrarily at 1912.

As a student of British geology prior to 1860, I view the first seventy-five pages of Greene's book with numerous reservations, for the history they contain is even more slanted than Lyell's. Reading it in isolation, one would think that British theorists had scarcely heard of mountains, let alone seen them. In actuality, however, mountain ranges and their formation were of vital interest to British writers from the late seventeenth century onwards, beginning with Thomas Burnet and John Ray. Though the theories then proposed were heavily influenced by theology, they none the less underlie subsequent considerations of global tectonics. As successive creations of mountain ranges came to be recognized by eighteenth-century Continental writers, moreover, English travellers and other literati reflected this new understanding in their accounts. Greene fails almost completely to acknowledge the attention given mountains by Hutton and his followers. Though he applauds John Murray's response to an explication of Hutton's theory by John Playfair, for example, he does not point out that Murray ignored problems associated with mountains, upon which Playfair had written at length.[4] Other British theorists, like G. B. Greenough in 1819, utilized the orientation of mountains and valleys (an approach similar to Elie de Beaumont's later on) to establish the reality of the Deluge.[5]

[1] Greene, pp. 24–25. The passage he cites from Hutton, *Theory of the Earth* (2 Vols., Edinburgh, 1795), as being on II, 124, is actually in II, 444–6, and means precisely the opposite; see I, 182, 195 as well.

[2] We know very little about Hutton's reputation on the Continent, particularly during the early nineteenth century. See, however, *Life, Letters and Journals of Sir Charles Lyell, Bart.*, edited by K. M. Lyell, 2 Vols., (London, 1881), II, 47–50. I discussed 'James Hutton and His Public, 1788–1802' in *Annals of Science*, 30 (1973), 89–105.

[3] Greene asserts his erroneous attribution four times: pp. 244, 251, 257, 280; see also p. 285. No work of George Darwin's appears in Greene's bibliography, however; the actual reference is to *Erasmus* Darwin's scientific poem, *The Botanic Garden* ('The Economy of Vegetation', 1791, canto II, lines 73–82).

[4] The two books were John Playfair, *Illustrations of the Huttonian Theory of the Earth* (Edinburgh, 1802) and John Murray, *A Comparative View of the Huttonian and Neptunian Systems of Geology* (Edinburgh, 1802). Playfair mentions the Alps in his numbered paragraphs 40, 108–13, 176–80, 199, 295–8, 303–8, 321, 323–8, and especially 338–67 *passim*. In replying to Playfair, Murray virtually ignored Alpine examples of geological phenomena.

[5] George Bellas Greenough, *A Critical Examination of the First Principles of Geology* (London, 1819), pp. 149–67. Greenough, not Lyell (Greene, p. 25), originated the phrase 'principles of geology'.

Anyone familiar with British art and literature knows that English-speaking tourists revisited the Alps in great numbers as soon as the peace of 1815 allowed them to do so. Lyell, for example, went to Mont Blanc in 1818 and like everyone else found the place overrun with his own countrymen. His initial attempt to explain the origin of mountains, in his *Principles* of 1830, may have been as inadequate as Greene suggests; but it was no more faulty than concurrent European alternatives and probably closer to modern views than theirs. Furthermore, Lyell continued to wrestle with the problem of orogeny; see, for example, his tenth edition, in which Elie de Beaumont's theory is tellingly refuted.[6] Finally, there is Greene's fuller claim that his line of influence, rather than the British one, established our present geology. In contrast, Raymond Siever argues convincingly for Hutton.[7]

Having exposed some of Greene's lapses—most of them in treatments of various early figures—I wish now to emphasize his strengths. Between a weak start and an arbitrary conclusion, he has written a very good book, detailing episodes of such importance to the history of geology that one can only suspect an enterprise which has allowed them to remain undiscovered for so long. Such traditional dichotomies as Huttonian–Wernerian, Uniformitarian–Catastrophist, Lyell and anti-Lyell, science–religion, British–Continental and European–American, moreover, are temporized or redefined, if not eliminated. Similarly, the arbitrary boundary of 1850 once established by Charles C. Gillispie (in his influential *Genesis and Geology*, 1951) is now more clearly meaningless. That British geology was less creative—though no less diligent—after mid century has often been supposed. Greene shows us where the vigor fled, and in so doing outlines the latter nineteenth century for our further study. His own explications of major developments within that outline generally achieve a hard-won clarity, and the often complicated narrative is nicely managed. With this prolegomenon behind him, Greene is presently at work on a biography of Alfred Wegener.

Despite its title and other flaws, *Geology in the nineteenth century* speaks forthrightly to a burgeoning interest in more recent science and may well become, as Gillispie did, the starting point for a new wave of geological historians. If so, Greene's overall impact will probably be less, because the history of science is more fully developed now and late-nineteenth-century geology had fewer immediate cultural implications; its practitioners were able, but seldom famous, men. By 1860, a dryer, professionalized geology was losing out in the public mind to the more exciting field of evolutionary biology, human history in particular. From our point of view, however, Greene's book is the first adequate historical response to the advent of continental drift and plate tectonics. Others having different suppositions but the same goal may well follow. In the meantime, one effective reordering of traditional history is now before us, argued with a cogency that no serious scholar can ignore.

DENNIS R. DEAN, *Humanities Division, University of Wisconsin–Parkside, Kenosha, Wisconsin 53141, U.S.A.*

[6] Lyell, *Principles*, 1867; I, 121 ff. Another British discussion of Elie de Beaumont to consider appeared in Joshua Trimmer, *Practical Geology and Mineralogy* (London, 1841), pp. 468–72.

[7] Raymond Siever, 'The Dynamic Earth', *Scientific American*, 249 (September 1983), 46–55.

Life Sciences

R. E. KOHLER, *From medical chemistry to biochemistry. The making of a biomedical discipline.* Cambridge: Cambridge University Press, 1982. ix + 399 pp. £22·50.

Brewing, dyeing, poisoning and therapeutics are ancient aspects of biochemistry. It, and metallurgy, could therefore be regarded as the original sciences. Here and there in this book, Kohler shows awareness of the antiquity of the subject, but he chooses to restrict attention to its connection with medical teaching and practice. There is little here about the history of biochemistry, and much about the history of medical teaching—especially in Germany, the U.K. and U.S.A. since the middle of last century.

Biochemistry, alongside other sciences, became a theme for active academic research earliest in Germany. Kohler suggests that it became institutionalized most extensively in the U.S.A.

because increasing interest in the subject coincided there with general reform of medical education and the hospital service. Recognition was slower in the U.K. and seems there to have been more affected by accidental and personal factors: in all three countries there was opposition to the establishment of the necessary new departments. Physiologists argued that biochemistry was no more than the chemical aspects of their subject; chemists argued that they were better qualified to teach it than were people who had 'wasted' part of their time learning biology; clinicians disliked having to consider opinions on medical matters emanating from people without medical qualifications. Several examples are quoted of clinicians refusing cooperation and access to clinical material.

Part of the trouble stemmed from nomenclature. As Kohler puts it, 'No discipline had so many aliases as biochemistry did prior to 1900: — ambiguous names impeded public recognition of biochemistry as a distinct profession' (p. 197). Today, nutrition, dietetics, food science etc. may be in a similar position. Proper recognition of the importance of these aspects of biochemistry would probably be hastened if they were unified under some such name as 'trophology'.

Kohler's sources extend far beyond conventional published material. He has ransacked the minutes of committees which controlled or influenced the institutes surveyed, and collections of letters from administrators, departmental deans, professors and other influential people. This must have involved an enormous amount of work. The resulting notes are a curious mixture of dense, perhaps unnecessary, detail, and racy, uninhibited comments on individual capacities and failings. For example, Sir Walter Fletcher is quoted writing to A. V. Hill in 1927 that Hopkins's laboratory in Cambridge 'bristles with clever young Jews and talkative women'. W. J. Crozier's apparently flippant comments on J. B. Conant's ideas on photosynthesis are not quoted verbatim, but he is quoted on G. N. Lewis: 'The childish freedom wherewith chemists speculate about biological matters often gives me a cold fury'. The temptation to quote more must, unfortunately, be resisted.

Biochemistry had slightly different forms in the three countries discussed. It tended to be basically structural and reductionist in Germany; perhaps, as Kohler argues, because of the persisting harmful effects of Liebig's outlook. Rubner is also disparaged, but for his lack of teaching skill rather than his preoccupation with energy. In the U.S.A., biochemistry was initially firmly tied to medical courses which were more prolonged than in any other country. Examples are quoted of student resentment at having to learn more than the rudiments of blood and urine analysis. In U.K., largely through the influence of Hopkins, his pupils, and Benjamin Moore, the subject had a more philosophical tone and encompassed the chemical behaviour of all types of organism. Kohler says: 'There was no equivalent in America to Hopkins' school'. Personality rather than university structure may explain that. Jacques Loeb contributed greatly to the subject and had an outlook similar to Hopkins, but Loeb was aggressive and selfish whereas Hopkins was gentle and generous.

There are many brief character sketches of the principal actors, and accounts of the manner in which they became involved in the play. Some, e.g. Van Slyke and Folin, appeared on the scene largely by accident: others, e.g. Edsall and those who taught at Yale, came on in a more logical manner. The information and judgements are not always reliable. As a serious example: too much weight is put on Hopkins's brief apprenticeship in an analytical laboratory. He probably learnt useful technique there, but his autobiography demonstrates that he was temperamentally a biologist from boyhood. As a trivial example: I am described as a physical chemist! A few characters are mentioned once only without description; they seem to be irrelevant. The status of some frequently-quoted letter writers is not adequately explained. It is not always clear whether phrases such as *lured away* and *trotted out* are used with the pejorative connotations they have in English usage.

The final chapter, called 'Toward a molecular biology', is the least satisfactory. Kohler seems to think that biochemists before the 1950s lacked the imagination to envisage what he calls the 'great problems' which were then beginning to be tackled. By what canon are fermentation and respiration not 'great problems'? These were the ones we had the tools to tackle in the first half of this century. When tools such as electron microscopes and isotopes were widely disseminated, a wider range of topics could be tackled. The main distinction is that biochemists are concerned with everything that goes on in organisms whereas molecular biologists have more restricted interests. I have discussed that issue at greater length elsewhere.[1]

N. W. PIRIE, *42 Leyton Road, Harpenden, Herts. AL5 2JB, England*

[1] N. W. Pirie. 'The nature of biochemistry', *British Journal of the History of Science*, 16 (1983), 273 *ff*; and 'The false-dawn of nucleic acids', *Trends in biochemical sciences* (in press).

MICHAEL RUSE, *Darwinism Defended.* Foreword by Ernst Mayr. Reading (Massachusetts) and London: Addison-Wesley Publishing Co., 1982. xvii+356 pp. £6·95.

This is a curious book: on the one hand it deals with a current topic of considerable interest, current criticisms of Darwinian theory; on the other it is unclear in its focus and perplexing in the usefulness it will find for a wide variety of readers. The main purpose, as emphasized clearly enough by the title, is to provide a defence of Darwinism against the attacks it has received in recent years from both inside and outside the biological community. Inside the biological community that attack has come from paleontologists (with alternative theories of rapid, discontinuous evolutionary change to supplant the Darwinian concept of slow, gradual, continuous change); outside the biological community the attack has come from social scientists who oppose the intrusion of Darwinian models into anthropology and sociology (as sociobiology), and from various forms of Creationism, which offer some slightly up-dated versions of Biblical lore as a substitute for evolutionary—and specifically, Darwinian—thinking. Ruse attempts to show how updated Darwinism can withstand all these criticisms, and emerge intact.

Ruse's defence of Darwinism, as reflected in the organization of his book, is a curious mixture of history, philosophy, textbook biology and social/moral polemic. The book is organized into five sections: Section 1, 'Darwinism Yesterday' (a brief historical account of Darwin's own path to the theory of natural selection, and the response from biologists and theologians; Section 2, 'Darwinism Today' (Mendelian genetics, the synthesis of genetics and Darwinism, and what is called 'Neo-Darwinism', or post-synthesis work in speciation; and, finally, philosophical issues about whether Darwinism is science or metaphysics, a tautology, etc.); Section 3, 'Darwinism Tomorrow' (the origin of life, population ecology, animal sociobiology, and the challenge from paleontology—namely, punctuated equilibrium); Section 4, 'Darwinism and Humankind' (a history of the study of human evolution; human sociobiology, and finally, evolution and morality—i.e., is there an ethic implied in Darwinism?); and Section 5, 'Darwinism Besieged' (scientific creationism expounded, considered and refuted). As one can see, this is a formidable array of topics to cover in a 350-page book, and, as a result, many are dealt with only cursorily. The main thrust of the book, as the title suggests, is to consider the various arguments that have been raised in the past, or are being raised at the moment, to standard Darwinian thought, and to show their weaknesses, or, at least, how they can be incorporated into the modern synthetic theory.

The strongest points of the book are its discussion of certain controversies, such as that between 'classical' Darwinians of today and the paleontologists who support a model of punctuated equilibrium. Here Ruse lays out clearly the issues involved: the role of empirical data, semantic problems with terms like 'rapid' or 'slow', and the nature of scientific models in leading to predictions and new tests of a theory. I found this discussion lively and clear, although I think Ruse does some disservice to the reader by leaving too strong an impression that the controversy may be mostly semantic—that is, differences over what 'slow' and 'rapid' mean to paleontologists and geneticists.

Other selections which are similarly clear and well-done include discussions of speciation *á la* Darwin's finches, colonization of archipelagos by the Hawaiian *Drosophila*, the adaptionist programme and its critique, and alternative theories to natural selection which have abounded over the past 100 years (neo-Lamarckism, saltationism, orthogenesis). Taken in themselves each of these discussion are useful and provide important insights into problems investigators have, or continue to see in Darwinian theory, and the Darwinians's response.

Despite these strong points, Ruse's book is not an overall success. There are several major problems and a few minor ones which detract significantly from what I perceive to be the author's purpose. The first, and probably most fundamental problem is the extent of coverage. By trying to range over such a broad array of topics, Ruse inevitably has to deal with many problems—often quite complex ones—cursorily. While his discussion of some of the biological cases is quite thorough and clear, that of many of the philosophical problems—ironically, given his background as a philosopher of science—is often superficial to the point of confusion. Even on the biological issues, many general principles are covered largely in captions to illustrations, sometimes by graphs and charts which are themselves poorly explained. Some chapters are no more thorough than one would expect from an introductory college textbook (for example, Chapter 7, 'Population Ecology'), and in many cases more confusing since the explanations are less than sufficient. The fact that much of this material seems largely irrelevant to the author's main argument only adds to the problem for the general reader.

Another problem is the definition—and, indeed, the picture which emerges as a result—of Darwinism itself. Ruse's view appears to be of Darwinian theory of the 1940s and 1950s. It is not an untrue picture, but as he himself points out there has been much exciting work in recent years which has extended and refined the picture of post-synthesis Darwinian thought. Little is said, for example, about the studies of variation in natural populations since Dobzhansky's work of 1947–1951, even though the more recent work, especially with the use of electrophoresis to detect hidden variability, has led to the proliferation of many new populations and ecological models of speciation. And, unfortunately, much of modern evolution ecology—for example, newer work on niche occupancy, habitat, diversity, experimental models of island re-colonization (area-species relationships)—is not mentioned at all. On the other hand, a great deal of space is devoted to old, post-synthesis case studies—for example, Darwin's finches, sickle cell anemia, evolution of the horse, melanic moths, and the like.

A third, and to my mind more serious problem, given Ruse's area of interest and expertise, is the lack of a clear and systematic discussion of philosophical issues. Particularly disturbing is his persistence in calling evolution a 'fact' (indeed, he uses italics and capital letters!), somehow distinguishing this from a 'theory' and 'model'. I appreciate his desire to counteract the creationists' use of the term 'theory' (in saying that creationism is a theory on equal footing, philosophically, with evolution or natural selection), but failure to define 'theory' and 'fact' more explicitly leads to all sorts of confusion, and even potential entrapment by the creationist argument itself. I am no philosopher of science, and may be guilty of making over-simplistic definitions, but it seems to me that a working definition that distinguishes 'observation', 'fact', and 'theory' is not only desirable but possible. I consider an observation to be a discrete item of sensory data, observed directly or mediated through some technical apparatus (microscope, cathode ray oscilloscope, scintillation counter, etc.). A fact I would define as a confirmed set of observations, repeated enough by independent observers that it is recognized as a phenomenon in the real world. A 'theory' I would define as any more general inference, or causal statement extending beyond observations or facts, which either relate one set of facts to another, or explain causal relationships. (I realize other terms such as 'model', hypothesis', or 'inference' are related to 'theory', but they need not concern us here.) Now, it seems clear to me that these are not merely arbitrary definitions; they do relate in some ways to general usage. Certainly I do not see the advantage of calling evolution a 'fact' any more than calling the theory of universal gravitation a 'fact'. One does not observe gravitation—one observes objects falling, tides ebbing and flowing, planets moving from place to place among the fixed stars. The *theory* of universal gravitation, and its precise mathematical formulation (as the inverse square law), is a conceptualization linking together, but extending beyond, a number of observations or facts themselves. So with Darwinism. For all his zeal to defend Darwinism and evolution as science, Ruse should not gloss over those extremely important distinctions.

Given the present controversy, it is important to point out that both evolution and Creationism are, philosophically speaking, theories. Their philosophical differences lie elsewhere than the 'fact' verus 'theory' issue. To me, it seems the main distinction (which Ruse never mentions) is that creationism is based on philosophical idealism, whereas evolutionary (and specifically, Darwinian) thinking is based on philosophical materialism. This is not the place to enter into a discussion of these matters in great detail but it seems to me that this philosophical distinction lies at the heart of creationist–evolutionist debates, while disputes over whether one or the other is more theory than fact are peripheral. I was disappointed that, well-renowned and highly qualified a philosopher of science as he is, Ruse did not deal more directly and thoroughly with these issues.

Another problem—really a diversion as I see it—is the author's chapter-long attempt to justify human sociobiology as both good science and good morality. The former claim is contradicted by Ruse's own admission. As he says, when starting to review the 'evidence' for sociobiological theories applied to humans, 'It must be conceded that, with respect to most of the broader claims about societal functioning, much of the discussion is very hypothetical! There is, for instance, absolutely nothing that backs up the speculations about the links between genes and religion... Sadly, not much is offered on the much-touted subject of xenophobia...' (p. 255). These statements seem to contradict the claim made five pages earlier that while in the past (he cites the work of Philip Ardrey, of *Territorial Imperative* fame) 'such arguments... have veered toward speculation and sensationalism,...' 'in the past decade, things have changed drastically'. How is sociobiology so much different from Ardrey and Desmond Morris, save in using a little more technical jargon, some mathematical model-building and the specific supposition that genes, rather than simply evolutionary trends, lead to specific human behaviours? In the remainder of the chapter, Ruse builds on the widespread existence of incest taboos to conclude that sociobiology has good grounds for future development as a new branch of Darwinism.

Not only do I disagree with these conclusions, but it seems to me the whole discussion is both irrelevant and actually detrimental to the author's main purpose. To defend Darwin from Creationists it should not be necessary to drag in a shoddy and unreliable poor cousin like human sociobiology. On the other issue—that sociobiology is not morally pernicious (Ruse's phrase)—I can only say that Ruse's argument holds only if one fails to take a look at how ideas like sociobiology have been used in the past in their social/historical contexts. I am not arguing guilt by association, but perhaps (to coin a phrase), guilt by context. For example, in a social and political environment permeated with racism, any theory of the biological difference (let alone biological inferiority) between races (in significant social behaviours such as intelligence, personality, artistic abilities, etc.) is bound to be used in a racist way. This is doubly true when the 'science' is based largely on speculation, in turn itself more than likely informed by common social prejudices.

Ruse takes sociobiology too much as an abstact science. He has not even used the data from the history of sociobiology itself to show that virtually all popularizations of it build up and reinforce already-existing social stereotypes (male-female sex roles, racism, socio-economic hierarchies). One can turn Ruse's own argument about Creationism onto Human sociobiology, and claim that it is merely old wine in new bottles, and has no more business being taught as science than any collection of unbridled and untestable speculations. I really wonder why, having already written a book defending sociobiology (*Sociobiology: Sense or Nonsense*, 1979), Ruse chose to go over the same ground again in the present book. Certainly, sociobiology is not itself an attack on modern Darwinism—indeed, it sees itself (as does Ruse) as extending and supporting Darwinian thought. The space could have been more profitably used for more in-depth discussion of the historical arguments against Darwin, or for a more thorough treatment of the final section dealing with the present-day movement of 'Scientific Creationism'.

The final chapters, toward which in some sense the whole 'defence' of Darwinism, is aimed, are disappointingly shallow. After a brief historical treatment of the Scopes trial in Tennessee (1925), Ruse outlines some of the recent attempts of creationists to influence school curriculum, culminating in the Arkansas test case of 1982. Ruse's treatment of creationist arguments is rather sketchy, but covers most of the gambit of traditional objections: gaps in the fossil record, controversy among scientists about the age of the earth, evolution contradicting the second law of thermodynamics (!), the implausibility of fine adaptations resulting from chance events, and the newer, post-Popperian argument that both creationism and evolutionism lead to predictions which can be tested (and when this is done Creationism comes out ahead!). Ruse is effective in pointing out that Creationists are muddled about their use of terms like 'theory' and 'model'—but as mentioned earlier, he does little to clear up this confusion. He is also right in pointing out (and documenting) how Creationists misrepresent what Darwinians claim for their theory. He moves quickly from one point to another, ending up with three general reasons why Creationism should not be taught: (1) It is really religion disguised as science; (2) Creationists have intertwined evolution with morality, claiming that the very idea of evolution is evil (the product of Satan, one current Creationist claims); and (3) Creationism is a step backwards in the history of thought like ideas of geocentricity or the flat earth; we owe it to our children to pass on accumulated and progressive wisdom. Creationism has had its day in the arena, and has been proved wanting. In all, I do not disagree with these conclusions. I only think the points would have been more effective had they been contained within a book where they were the focus, rather than the end-point of a rather diffuse series of arguments. The impact of Ruse's critique of creationism is weakened by the polyglot character of the book as a whole.

All in all, then, *Darwinism Defended* has far more weaknesses than strengths. For those readers wanting a more focused discussion of the biological aspects of creationist argument (and the Darwinian response), an excellent, and very modern source is Douglas Futyuma's *Science on Trial* (Pantheon, 1983). For more detailed discussion of the sociology of the current controversies (with some historical background), the reader is referred to Dorothy Nelkin's *The Creation Controversy* (Norton, 1982). Both books provide more of an in-depth treatment of their respective subjects.

GARLAND E. ALLEN, *Department of Biology, Washington University, St Louis, Missouri 63130, U.S.A.*