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Book reviews

KLAAS VAN BERKEL, **Dijksterhuis: een biografie.** Amsterdam: Uitgeverij Bert Bakker, 1996. Pp. 639. ISBN 90-351-16941. HFL75.

Eduard Jan Dijksterhuis (1892–1965), the eminent Dutch historian of science, belongs, with Pierre Duhem, Alexander Koyré and George Sarton, to the founding fathers of the profession as we now know it. But unlike his fellow-travellers, who had their fair share of attention from biographically minded historians, Dijksterhuis remained just a name, the author of an important book: *The Mechanization of the World Picture*. Klaas van Berkel has remedied this situation.

Dijksterhuis was born into a upwardly mobile family of schoolteachers. His father was head of a small secondary school in Tilburg. After having attended his father's school, Dijksterhuis went to Groningen University in 1911 to study mathematics. Having defended his Ph.D. in the summer of 1918 he left Groningen to return to Tilburg to become a maths teacher at his father's school. At the time it was difficult to find qualified science teachers and when his father called upon him it was a case of being unable to refuse.

Dijksterhuis would stay in Tilburg for the next thirty-five years when he would leave to accept a chair at Utrecht University. Nearly all his scholarly work was done while he was a full-time teacher. But Dijksterhuis did not initially make a name for himself as a historian of science or mathematics but as an educationalist. It was especially the role and function of mathematics in the school curriculum that aroused his passion. At the time Dijksterhuis entered this debate the role and function of science and mathematics in the curriculum was under attack. They were associated with an élitist, materialistic, intellectualistic and deterministic world conception that had become fashionable at the end of the nineteenth century. The virulent attack on the scientific community was a pan-European phenomenon. The Dutch ecotype of this debate concentrated on the place physics and mathematics were to have in the new school curriculum. The drive was very much towards a less prominent and dominant place of the science subjects in favour of more classical humanistic values. Inevitably, part of the argument against the science subjects was based upon the recognition that not all students are interested in it or have the right talents for it. Dijksterhuis would have nothing of this. For him it was a matter of course that all students should have a proper training in both science and arts subjects. In his Utrecht inaugural address he would describe himself as a ferryman between these two opposing camps. This brings one to what unites Dijksterhuis's educationalist writings with his scholarly work on the history of science: for him the history of science could be a bridge between science-minded and humanities-minded people.

While actively involved in his educationalist writing, Dijksterhuis started his research in the history of mechanics which in 1924 resulted in the publication of his first book, *Val en worp* (*Fall and Throw*). The book starts with a discussion of Aristotle's treatment of the subject, quickly passes over to how Galileo dealt with it and ends with an analysis of Newton's view. The structure of his later *Mechanization* is already clearly in place. It is difficult not to see the latter book as a popular version of this book. Those who have read the books tend to prefer the earlier one; it is much richer in historical detail and is historiographically more sensitive. This should not come as a surprise, as the sub-text of the *Mechanization* book – mechanization is mathematization – is not as rigidly driving the whole argument. Of course it is not absent either, for an important part of the better understanding of the mechanics of fall and throw is for Dijksterhuis its mathematical treatment. What is sometimes called the Dijksterhuis thesis – the development of early modern science is dependent upon the development of mathematical techniques – is clearly argued for in this book but it is not, as is *Mechanization*, an apology dressed up in a rationally reconstructed historical narrative for the importance of mathematics in science in general.

Fall and Throw would be the first of his many books on the history of science and mathematics, all of which found their integration in his *Mechanization* of 1951. At the same time he would be actively campaigning for a rigorous treatment of mathematics at school, a campaign that would involve a lot of committee work. His initial minority view would slowly become the dominant one. His educationalist essays and his many scholarly essays did not go unnoticed and by 1933 he was invited to become a member of the editorial board of the prestigious Dutch literary and scientific journal *De Gids* (*The Guide*). This invitation was, however one looks at it, quite an achievement for a maths teacher in a small provincial town.

In the 1920s and 1930s the only way into academia was to accept a job as a teacher and put out a steady stream of essays and the odd book and hope that one day one would be invited to take up a chair. Seen in this light Dijkersterhuis's first book is difficult not to interpret as an open application. It took some time for the invitations to come but in 1932 he was appointed a private professor first at Leyden University and shortly afterwards in Amsterdam as well. Neither appointment was very successful and both were terminated after some years. Dijksterhuis was not able to get many students interested. His style of lecturing is to blame here. For students it would appear that Dijksterhuis was reading his lectures from an invisible book– which has some truth in it as he would learn his lectures by heart.

Nevertheless, in 1942 he was invited to take up a position at the University of Amsterdam. The well-known politicking would take more than two years and in March of 1944 he accepted the offer. Dijksterhuis, who in his educational writings had always stressed the moral qualities of mathematical reasoning, was however incapable of seeing that accepting an appointment at a university that was completely under German control could only be interpreted as a sign of support for the German oppressors. Due to ever-increasing confusion at the end of the war he never did lecture in Amsterdam. After the de-Nazification he was suspended. He was, however, able to keep his old job. Incapable of understanding what he had done wrong he was initially bitter but slowly the wounds healed, especially because after a while he was accepted again as a full member of the academic milieu. He had shown a moment of moral weakness but had not personally harmed anyone. In 1951 he published his *Mechananization*, the book that was to establish his name as a great scholar and a fine historian. In 1953 he was called to Utrecht. Although Dijksterhuis received many international awards during his Utrecht period, his professorship there was not a success. His lectures were not well received, he was too much of a loner to be able to set up a department and his scholarly work dried up. He was, however, to create a new role for himself: philosopher of culture, and he loved it. He was a speaker in high demand who toured the country with a range of standard lectures people could choose from. On 1 January 1959 Dijksterhuis suffered the first of a number of strokes, which left him half paralysed and after some time he would even lose the ability to speak. Six years later he died.

Van Berkel has written a long biography on a man who lived a very unexciting and Kantian kind of regular life. Although it does not follow that a biography on a boring life should itself be boring, this one comes close to it. Van Berkel goes too much into trivia and irrelevant detail, making the book a difficult read. My biggest problem with the book was that Van Berkel hardly ever shows any sign of understanding what made Dijksterhuis tick. The result is a bloodless, shallow story of factual events in Dijksterhuis's life. The one thing I liked about the book was that Van Berkel is able to make a convincing case on the confluence of the two sides of Dijksterhuis's work, that he was trying to defend the central position of science in our society and fought that battle on two grounds: the school curriculum and the history of science. I strongly urge Van Berkel to bring this out in an essay – in English – to commemorate, in two years from now, the fiftieth anniversary of the publication of *Mechanization*.

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Masao Watanabe, **Science and Cultural Exchange in Modern History: Japan and the West.** Tokyo: Hokusen-Sha, 1997. Pp. xi + 406. ISBN 4-938424-73-8. No price given.

Watanabe is a pioneering and one of the foremost historians of science in Japan. He has helped establish history of science as an academic discipline in Japan and has been personally involved in the founding of history of science programmes and professorships at several Japanese universities since the 1950s. As a pioneering researcher and educator, he has written about a wide range of topics. One of his books, *The Japanese and Western Science* (Philadelphia: University of Pennsylvania Press), which was translated by Otto Bentey, has received notice among Western scholars, particularly for its chapters on the Japanese reception of Darwinism and on Western science teachers in Japan in the last decades of the nineteenth century.

The present collection of about two dozen essays – some of which are substantial – is less focused than the above-mentioned book. The essays are grouped into three parts. Part I consists of ten case studies in the history of science in the West; they touch upon subjects including Francis Bacon's idea of philanthropy, Newton's theory of mechanics and major theories of heat and motion in the nineteenth century. Many of these essays were first published twenty or even thirty years ago.

Most readers will probably be drawn to Part II, which deals primarily with the introduction of Western science into Japan. The essays in this part of the book include several overviews and case studies of Western science teachers in Japan in the Meiji period. Watanabe carefully describes the motivations, exertions and influences of foreign teachers of various nationalities and religious backgrounds, and he discusses how the differences among them influenced their work in Japan. His pieces on American scientists in

Japan, including E. S. Morse and J. T. Gulick, are fundamental. Watanabe is sensitive to how the Western teachers became interested in aspects of Japanese history and life. Morse, for example, studied Japanese pottery and made a large collection of Japanese ethnographic objects. Similarly, Western scientists in Japan were intrigued by the Japanese 'magic mirror' and investigated its optical properties. Watanabe, however, only hints at the ways these activities can be related to the Japanese reception of Western science as part of what he describes as 'cultural exchange'.

Watanabe is at his best in explaining the introduction of Darwinism into Japan, although his argument is more completely developed in his *The Japanese and Western Science* than in the book under review. Here, fragments of Watanabe's main argument are scattered across a number of essays. He points out that the concept of evolution did not challenge any core values of the cultural tradition of Japan and that the Japanese embraced simplified or distorted versions of Darwinism, especially in the form of Spencerian social Darwinism. The fear of being dominated and conquered by Western imperial powers, he maintains, made the Japanese highly receptive to the view of survival of the fittest, at least as applied to a ruthless competition among nations and races.

Put together, the two long essays (co-authored with R. W. Home) on the comparative study of Australian and Japanese physics from the late nineteenth to the mid-twentieth century make a notable contribution to our understanding of sience in the national and international contexts. These essays trace the development of physics and physics communities in the two countries and discuss the major similarities and differences between them. The authors, however, are aware of the problems inherent in such a comparison. Japan and Australia differed greatly in history, cultural tradition, and population size, and their political and social ties with the West bore little resemblance to each other. The authors deliberately limit their attention to the immediate institutional settings of physics in the two societies and to their respective intellectual connections with Europe. Although this approach does not allow consideration of broad cultural contexts, the authors make important empirical conclusions about the building of physics communities in the two countries.

The essays in Part III are occasional pieces, including research notes on the Japanese 'magic mirror', reports on history of science education in Japan, and two excursions into the Japanese conception of Nature and its relationship with modern science. Watanabe draws a sharp contrast between Western and Japanese attitudes towards nature. The ideology of modern science in the West, he argues, has been built upon a desire for objectivity and for the dominance and control of nature. The Japanese notion of nature, on the other hand, emphasizes harmony, aesthetics and appreciation. While the Japanese view of nature has been a negative factor in the development of science in Japan, Watanabe suggests, the danger of environmental destruction caused by the rapid growth of science and technology seems to call for a new kind of scientific research informed by an appreciative attitude towards nature.

Despite the vintage and miscellaneous nature of the essays, the recurrent themes of the book remain fresh. Watanabe's emphasis on the cultural context of modern science, especially as regards the introduction and reception of modern science in a non-Western society, is highly relevant to recent scholarly developments in the history of science. Many of the essays are strengthened by useful statistical data. In spite of James Bartholomew's *The Formation of Science in Japan* (1989), which makes good use of the research of Watanabe himself, among other Japanese scholars, the two Watanabe books in English still have much to offer.

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José M[a] López Piñero *et al.* (eds.), **Bibliographia Medica Hispanica, 1475–1950, Volumen V, Libros y Folletos, 1851–1900**, Caudemos Valencianos de Historia de la Medicina y de la Ciencia, L. Valencia, Instituto de Estudios Documentales e Históricos sobre la Clencia Valencia: Universitat de València-C.S.I.C., 1996. Pp. 956. ISBN 84-370-2349-1. No price given.

This book is part of a collection devoted to creating an inventory of medical texts exclusively written by Spanish authors. The editors have organized the collection in eight volumes. Six of them (i–vi) comprise books and pamphlets written between 1475 and 1950 and another (viii) contains journals edited between 1734 and 1950. An extra volume is planned to include the analytical index.

The collection began to be published in Valencia in 1987. The Valencian group research programme was, first of all, defined by the intention to recover medical sources. But the research agenda has also been aimed at analyses of these sources using such methodological tools as bibliometrics, semantics and documentation, and prosopography. This was part of a wider social history programme employing the idea of using methodologies provided by different social sciences. Such a thorough task could be undertaken by the Valencian team partly because of it being a numerous group that could afford to do so, but also because the Valencian University Library has probably the best reference books section of all Spanish universities. Last, but not least, it is well known that the experienced bibliographic tradition of López Piñero and Terrada among others has offered the community of historians notable key reference books such as the *Diccionario Histórico de la Ciencia Moderna en España* (Barcelona, 1983).

The volumes, except volume viii (journals, 1736–1950), are solely organized in alphabetical order. This editorial strategy decreases the usefulness of the collection to the user, given that the analytical index is still unpublished. The collection's utility rests in its exhaustive and rigorous compilation of sources based on an array of catalogues and libraries (Spanish National Library and Valencia History of Medicine Library). In this sense it can be said that these volumes represent the best inventory of Spanish medical sources available although the Royal Academy of Medicine Library is not recorded. The importance of having such a compilation inventory could have a different meaning for Spanish rather than for British historians if we compare the standards of our libraries and archives, even taking into account the recent funding cuts suffered by the British infrastructure. In our present situation, having this inventory could avoid laborious pilgrimages through geographically scattered libraries, as historical sources are usually the last to be included in databases, not to mention our authorities' persistent lack of awareness about the preservation of historical sources. However, the usefulness of this collection will increase when the analytical index is published.

Returning to the content of the volume, I appreciated the wider concept of 'scientific or medical text' employed, the genres ranging from scientific monographs to a memoranda on clinical activities and from obituaries to class notes. The author's option of privileging no text in the compilation, I think, is not only historiographically useful but also intellectually fertile and coincidental with some of the recent trends in texts and discourses that argue that any text can be relevant in its context (among others White's *The Content of the Form*, Baltimore, 1987). The books and pamphlets collected are also comprehensive in terms of the disciplines that they embrace: public health, hydrology, surgery, homoeopathy and so on. To this point I have only to remark that volume v includes 5708 references.

Future work on these sources will show how far this research group has included recent methodological perspectives on scientific texts as part of their research tools. But any work providing an entire picture of the production of Spanish medical texts will be welcomed by historians.

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FRANCIS BACON, **Philosophical Studies, *c.* 1611–*c.* 1619**, edited by Graham Rees. The Oxford Francis Bacon, VI. Oxford: Clarendon Press, 1996. Pp. cxvi + 503. ISBN 0-19-812290-X. £80.00.

This is the first of a projected twelve-volume critical edition of Francis Bacon's works being prepared under the general editorship of Graham Rees and Lisa Jardine. Intended as a new complete and critical edition in accordance with the principles of modern textual scholarship, it should prove to be a great improvement upon the standard edition prepared in the nineteenth century by James Spedding, Robert L. Ellis and Douglas D. Heath. Ordered according to broadly chronological criteria, this first volume, containing a set of texts composed between 1611 and 1619, has been designated as the sixth in the series. It includes a number of important natural philosophical works: *Phaenomena universi*, *De fluxu et refluxu maris*, *Descriptio globi intellectualis*, *Thema coeli*, *De principiis atque originibus*. It also provides an authoritative text of *De viis mortis*, a newly discovered work, and an English translation of the *Phaenomena universi*, which remained untranslated in the standard edition.

The introduction, notes and commentaries by Graham Rees recapitulate the latest results of his own studies on Bacon's speculative ideas and the destiny of his writings from the manuscript to the press. Rees situates all the pieces in the general context of Bacon's work, all of them being considered to be early and fragmentary contributions to the six-part sequence of works known as *Instauratio Magna*. The introduction provides a careful examination of the dating of each work, together with generally plausible arguments, whenever needed, as to why Rees's date should be preferred to those offered by earlier scholars. Rees is always careful, however, to acknowledge the conjectural and provisional nature of the dating of any work which is grounded on scanty evidence. Such is the case of Bacon's *Urtext*, from which was copied the scribal draft of *De viis mortis*, or of *De principiis atque originibus*, the date of which has been vigorously discussed.

Rees's summary of Bacon's speculative philosophy constitutes a comprehensive and systematic approach to this otherwise quite ignored aspect of Bacon's thought. Bacon's tenets are said to be connected with many diverse traditions which converge in an interesting eclecticism. Rees makes clear that the views of Bernardino Telesio, Joseph Duchesne and Alpetragius were combined in the elaboration of Bacon's cosmology. He also points out the role of theology in demarcating the boundaries of Baconian science. Theology embraces a set of revealed data with which all cosmological theories must be consistent and Bacon framed his own cosmology by taking account of this regulative point of view. Finally Rees gives a panorama of Bacon's semi-Paracelsian cosmology and its connection with his astronomical and what we would call his biological ideas. In particular he pays detailed attention to Bacon's interests in the prolongation of life and the conceptions of vital and inanimate spirits, which constituted the main goal of *De viis mortis*.

The texts are presented in an accurate critical edition of the original Latin with facing-page English translations by Graham Rees and Michael Edwards. The translations try to give a fluent modern expression of Bacon's thought. This very difficult task sometimes generates, in my view, slight deviations from the original meaning. But imprecision at this level is unavoidable even with the utmost care, and Bacon scholars will be easily consoled by the ready availability of the original Latin. Every work is accompanied by commentaries, which generally provide references to external sources or other Baconian works. Special mention should be made of *De viis mortis*, the edition and translation of which probably demanded the most effort. Rees has been working on this piece since approximately 1980, when it was discovered by Peter Dear. The manuscript (namely MS Hardwick 72A, at Chatsworth House in Derbyshire, first edited for publication by Graham Rees as a BSHS Monograph, *Francis Bacon's Natural Philosophy: A New Source*, 1984) is very confused and abstruse. The first part is a scribal draft alternated with many deletions, revisions and additions in Bacon's hand. The second half was written by the author himself but finally abandoned unfinished. Rees conjectures that the work may have been destined for Part 1 or Part 5 of the *Instauratio*.

Appendices Two and Three of this edition offer a detailed record of the manuscripts and their transmission and a scheme for the probable final organization, which Bacon wanted to give to the *De viis mortis*. The first appendix provides detailed information about Bacon's *Scripta Philosophica* as edited by Isaac Gruter (Amsterdam 1653), which was effectively the first edition of the works included in this volume (except of course of *De viis mortis*). Since the editors decided to present all the edited texts together with their original copy-signatures (printed books) or folio number (manuscripts), the fourth appendix provides a table of correspondences between the signatures or folio numbers and the page numbers of the Spedding, Ellis and Heath edition (Latin original texts and their respective

English translations). Judging from the high standards of this initial volume, the Oxford critical edition will offer more careful and complete texts than have ever been available to Bacon scholars before, together with modern textual apparatus. Undoubtedly this will contribute to the revival of research upon Bacon's 'classical themes', and to the rediscovery of some disregarded corners of his thought.

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Charlotte Methuen, **Kepler's Tübingen: Stimulus to a Theological Mathematics**. Aldershot: Ashgate, 1998. Pp. 288 + xi. ISBN 1-85928-397-7. £45.00.

In assessing the character of change in early modern Europe, historians of science have concentrated in the last twenty years on explaining how and why studies of nature became mathematized. In the sixteenth century, the Aristotelian disciplinary distinction between natural philosophy and mathematics was widely accepted by university-educated men. While natural philosophy discussed the reality of the physical world through causal investigation, astronomy, as a mathematical (or 'mixed') discipline, at best advanced hypotheses that saved the phenomena. The question, then, is why Copernicus, Brahe, Kepler or Galileo believed a mathematical system could describe legitimately and fruitfully the reality of the physical universe. Recent answers to this question have come from a sociological direction, highlighting the new forum for the study of nature (outside universities) – the court – in which Brahe and Galileo, for instance, pursued their studies of nature. A new form of knowledge was developed in a new kind of institution. Methuen's book also addresses this central question, but answers it from the direction of the old institution, the university. She argues through a careful study built on primary sources (many of them archival) how theology provided the inspiration for changes in an old forum for learning. The university in question is Tübingen– well known, of course, since Kepler studied there. Building on the works of Hofmann and others (and often correcting them through her own archival readings), Methuen first gives a detailed picture of how the scholarship system was set up in Württemberg to train teachers and Lutheran pastors. Kepler and many of his teachers were trained through this system (the maps and the chart of university posts appended at the end come in very handy here). She then goes on to contrast the different attitudes towards philosophy of the Reformers Luther and Melanchthon. Luther, always intent on focusing on the Christocentric message, was wont to emphasize the limits of human reason, distinguishing between knowledge of Gospel and Law, though he did see moral philosophy as potentially useful for the recognition by human reason of its own finitude. In contrast, Melanchthon, although similarly insistent on the distinction between Law and Gospel, saw a greater role for philosophy – above all, he sought moral authority in nature, God's Creation, and, in studies of the order of that nature, Melanchthon vigorously promoted the benefits of natural philosophy, astronomy, geometry and arithmetic. In the next two and central chapters, Methuen compares the thoughts of the teachers at Tübingen with that of the *Teacher of Germany*. Here Methuen's strength as a careful Church historian shines through. Based on extensive readings of primary sources, she proceeds to show how the teachers at Tübingen variously saw the importance of the study of the Book of Nature in a theological context. She deftly shows how subtly but significantly the positions differed amongst important figures such as Michael Maestlin, Nicodemus Frischlin, Jacob Heerbrand, Martin Crusius, Andreas Planer and George Lieber. She offers the best account yet in the English language of their religious and philosophical positions.

Methuen's contribution to one of the central questions in early modern history of science may not seem dramatic or spectacular, but is significant nevertheless. This is precisely the kind of careful and painstaking research needed for our understanding of Protestantism (or Lutheranism in this case) and natural philosophy. As I have pointed out elsewhere, the study of a particular kind of natural philosophy is not directly implied or required from Luther's doctrine of *sola fide*. Nature, or Creation in which God constantly participates, can be a rich and legitimate study for a good Christian, but it does not follow that every Christian has to do it. In other words, when people do decide or bother to undertake natural philosophy in a particular way, they have their own reasons for doing so, be they religious, vocational or personal. As Methuen shows, there are multitudes of reasons and therefore as many different ways of understanding how philosophy, theology and astronomy might be related, although the teachers at Tübingen all agreed that Law is not Gospel and that the providential plan is visible in nature.

The picture that Methuen presents of Tübingen is one of the dominance and vitality of Aristotelian philosophy, which, she argues convincingly, provided the intellectual environment for Kepler, who famously said that 'since we astronomers are priests of the highest God in regard to the book of nature, it befits us to be thoughtful not of the glory of our minds but rather, above else, of the glory of God'. (Max Caspar, *Kepler*, trans. C. Doris Hellman, New York 1993, 88). At the end, Methuen rightly puzzles over the absence of sources for Kepler's Platonism at Tübingen, which, according to her, seems to have been acquired more or less through private studies. Here, Simon Grynaeus' editions on Platonic and Neo-Platonic works as well as 'unofficial' but standard university readings such as Scaliger's *Exercitationes exotericae* or Cicero's *De natura deorum* may hold the key, but that would be asking her to write another book.

This, then, is a solid and important study that addresses one of the central questions in the history of early modern science. Scholars interested in 'Science and Religion', Aristotelian philosophy and the history of universities, as well as specialist Kepler scholars, will all find this book helpful.

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Charles Coulston Gillispie, with the collaboration of Robert Fox and Ivor Grattan-Guinness. **Pierre-Simon Laplace, 1749–1827. A Life in Exact Science.** Princeton University Press: Princeton, New Jersey, 1998. Pp. xii + 322. ISBN 0-691-10850-0. $49.50, £35.00.

The present book is a revision of the biographical entry on Laplace by Gillispie and his collaborators, published in 1978 in the *Dictionary of Scientific Biography*, of which Charles Gillispie was the primary editor. The work was of exceptional length for the format of the *DSB* and its manifest scholarly qualities fully merit a revision and its issue as a separate volume. This book, along with his monograph on Lazare Carnot, establishes Gillispie's place as a historian of the exact sciences, with work which stands high among his varied and accomplished contributions to the history of science.

The book remains the only modern systematic treatment of its subject, perhaps because Laplace offers a forbidding face to the biographer. This is not only because of the severely mathematical cast of his work. The narrative of the history of science has long been shaped by revolutionaries and innovators, but Laplace was no Galileo or Newton. He was an indefatigable calculator and a vindicator of the Newtonian system of the world, seeking to demonstrate its cogency and intrinsic accuracy. Gillispie finds the main substance of Laplace's life in his memoirs and famous treatises on celestial mechanics and the theory of probabilities. He is of course alert to a broader biographical narrative. Laplace played a full part in the politics of science at a crucial period of French history, to which Gillispie pays due attention. Laplace's distinctive philosophical outlook, manifest in his earliest writings, as well as his role in shaping mathematical physics as a scientific discipline, also provide scope here for broader historical reflections. (I must note, however, that the reference to James Clerk Maxwell on p. 275 makes unfortunate errors in spelling and chronology.)

Following the scope of the *DSB* entry, the focus here is on Laplace's mathematical work. Gillispie handles these difficult topics with elegance and clarity, showing how Laplace's early investigations of probability led him to problems in celestial mechanics. Laplace thus framed the twin central issues of his mathematical work early in his career. The main substance of the text, with occasional interludes on Laplace's public career, provides a detailed and chronologically ordered account of Laplace's scientific work. The argument encompasses all necessary detail, and philosophical issues are expounded cogently. The systematic bibliography is of exceptional value. Like its progenitor, the *DSB* article, Gillispie's account of Laplace's scientific career is enriched by authoritative contributions by Robert Fox on Laplacian physics (the velocity of sound, short-range forces and the Laplacian school), and by Ivor Grattan-Guinness on the Laplace transform (Laplace's integral solution to partial differential equations, and its later history).

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I. S. Glass, **Victorian Telescope Makers: The Lives and Letters of Thomas and Howard Grubb.** Bristol and Philadelphia: Institute of Physics Publishing, 1997. Pp. xiii + 279. ISBN 0-7503-0454-5. £30.00, $50.00.

Is it a paradox that Ireland, with little in the way of manufacturing industry, lacking a serious commercial engagement in the production of machine tools or motive engines, with scarcely anything in the way of conventional raw materials for a metal industry, and where technical education was understood by its promoters to be inadequate, created one of the world's leading manufactories for a class of instruments that combined heavy machinery, fine optics and precision engineering? The question deserves to be addressed, but the reader will not find an answer in this account of the lives and work of Thomas and Howard Grubb. The author explains in the preface that he is not offering 'a straightforward biography' of the Grubbs, but will 'let them speak for themselves through their letters'. A biography would have been a much less straightforward project than the one adopted here, which results in something closer to a source book than a history, but which lacks the rigour of an edition of letters and papers.

After an outline account of the lives of Thomas and Howard and the fortunes of their company, the material is organized according to the succession of their telescopes. The first commission, in 1831, was an equatorial mount for a 13.3-inch object-glass by Cauchoix acquired by E. J. Cooper of Markree, County Sligo, and this was followed by a 15-inch reflector on an equatorial mount for the Armagh Observatory. The astronomer at Armagh, Thomas Romney Robinson, was a loyal supporter of the Grubb business, taking opportunities presented in his long career to advance its international reputation.

Robinson played an influential part in the Grubbs's capturing the contract to build the 'Great Melbourne Telescope', one of the best-known and most controversial reflectors of the nineteenth century. The book adds little to this story, but it is already the most thoroughly written aspect of the firm's career. The Melbourne commission resulted in the founding of a new optical works with Howard in charge, taking over from his father, while another famous instrument, the 'Great Vienna Telescope', occasioned the building of the 'Optical & Mechanical Works' at Rathmines. When built, this 27-inch telescope was the largest refractor in the world. Howard was best known for a series of refractors in the later part of the century, whereas his father had previously been more associated with reflectors. One appendix to the book is a valuable list of all the larger Grubb telescopes, curiously arranged not by date but by size.

Glass's inclination to copy extensively from archives has paid off in the correspondence between Howard Grubb and David Gill, where there are a number of interesting letters, and in general the book contains much valuable material. But in the surrounding text, quotations are often not referenced and factual statements frequently not supported by sources. This is a step towards an adequate appreciation of the Grubb phenomenon, but there is much more to be said from the perspectives offered by biography, straightforward or not, and history.

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Aleksandr Yakovlevich Kipnis, Boris Efimovich Yavelov and John Shipley Rowlinson, **Van der Waals and Molecular Science.** Oxford: Clarendon Press, 1996. Pp. ix + 313. ISBN 0-19-855210-6. £60.00.

This is a book of fine symbolism and implicit associations. The cover illustration, Claude Monet's *The Drawbridge*, *Amsterdam*, is not the stage for the scientific activities of van der Waals, the famous Professor of the city's newly founded

University, but a symbolic representation of the main theme of his immortal contribution to science, for which he would be awarded the Nobel Prize for physics in 1910: a misty atmosphere, which becomes heavier and heavier as the temperature falls, has forced those crossing the bridge to open their umbrellas, because the gaseous state of water vapours has passed into a familiar liquid state – it is raining again! Moreover, it has done so in a continuous path, as their great compatriot has shown in his seminal doctoral thesis with the now famous title 'On the continuity of the gaseous and liquid state'.

Van der Waals's name is so famously connected with this concept of continuity that few historians would deem it important to ask the simple and obvious question: what was unique about this 'continuity', missing therefore from similar conceptualizations of the same period – Andrews's or James Thomson's, for example? Readers may be surprised to see how many subtle points are discussed and clarified, and what kind of meaningful and interesting associations are brought to our attention by the authors of the present book in relation to this concept.

The notion of continuity is linked to the use of geometrical representations. This signified not only a choice of form of presentation of numerical results, but more importantly a 'habit of thought', which resonated with what van der Waals believed to be central qualities of science: the pictorial, intuitive, explanatory dimensions of it. Although this view was not peculiar to van der Waals, for it characterized much nineteenth-century molecular discourse, the analysis in this book highlights the fact that such 'habits' are not adequately explained as signs of a *zeitgeist*, but need to be understood in relation to the pedagogical, intellectual and institutional environments that nurture them and provide the means for their expression. In this vein, we are informed that geometry was the first subject that van der Waals studied at the University of Leiden, and that his teacher was a leading Dutch mathematician with a particular fondness for geometry. It is left to our empathic involvement with the narrative to appreciate the impact that this first class might have had on a young man with a passionate love for science whose family's scanty means had deprived him of a proper education. And although the teacher–student relationship is much discussed throughout the book, because van der Waals experienced intensively both sides of it, it is of merit that it invites more insights than it affords. For example, to what extent did the fact that van der Waals had been a secondary-education teacher before he became a student in the University affect his perception of science? Did it lead him to make a conscious effort to cross the boundaries between different scientific fields (physics and chemistry)? Did it lead him to synthesize different approaches (thermodynamics with the equation of states), or to build on and elaborate other scientists' work (such as Laplace and the elusive quantity *K*, Clausius, Maxwell and the mean free path, Joule, Thomson and their experiments on adiabatic expansion, Gibbs and the equilibrium surfaces), or to combine theoretical intuitions and experimental justifications into a beautifully balanced scheme? How did the fact that his first authoritative involvement with science primarily required an ability to 'explain', rather than to 'prove', affect his style of reasoning and his predilection for underlying causes, in the form of explanatory and yet unproven molecules?

Continuity also meant 'identity' of the two states of matter, and more specifically identity of the material particles in both states. And this insistence on 'identity' was actually an attack on hypotheses suggesting that during condensation the molecules of the gas clustered together and gave rise to molecular complexes. Yet again, the theme of 'identity' shifts our attention continuously from the material particle that retains its integrity while gross matter undergoes turbulent transformations, to the man and his sense of personal and national identity, his boldness and independence of spirit, his audacity to attack the most fundamental question of his time, and his moral integrity.

Finally, the notion of 'continuity' is juxtaposed with the 'discreteness' of individual molecules, whose real existence was, according to van der Waals, an undeniable fact. The reader may think that this apparent contradiction probably carries an interesting implication about the most private agonies of the human spirit which, in the case of van der Waals, took the form of an unanswerable question: am I mad, or am I doing a great thing? Success, fame and recognition followed, but the reader will probably doubt that these erased the memory of this agony.

The notion of continuity acquires substance through the narrative of the book which gradually creates – one might say through a continuous and well-plotted path – the portrait of a man. Was he religious? What were his motives? What amused him in his leisure time? Would a good joke make him laugh? Was he a smoker? Was he an affectionate father? These are the details that a historian must provide in order to turn a mere name into a real person. The authors know this and perform their narrative duties in an excellent manner in order to make van der Waals known to us in all possible ways. The authors offer an excellent reading to those who appreciate the polymorphic quality of written history. They synthesize a coherent whole out of an 'internalistic' analysis of van der Waals's work, a vivid account of the Dutch context and an intelligible presentation of the network of people who were related to, or influenced or were influenced by, van der Waals. They bring together the private and the public, the questioning mind and the wounded soul. They allow the historical personae to speak for themselves, but at the same time they have the vast knowledge required to correct them when signs of subjectivity appear, and the confidence to fill in, with reasonable inferences of events that have not survived in the historical record, some finishing touches which give to the narrative the texture of real life.

When the biographical story comes to its expected end, in the tenth rather than the last chapter (and rightly so), the authors spare us the details. The short presentation of van der Waals's last days, in comparison with the grand story of his scientific achievements, leads the reader to a moment's reflection on the fragility of human beings and the potentially lasting influence of their thoughts and deeds. As a consequence of this rhetorical strategy, but also as a matter of choice which makes it obvious that the authors are fully aware of the danger of backward projection when dealing with scientists of van der Waals's magnitude, the presentation of the fields of study to which he mainly contributed – molecular physics and physical chemistry – are given separately, in Chapters 11 and 12.

From such a book one has expectations that go beyond the faithful and interesting exposition of facts, beyond the proper evaluation of a great scientist's intellectual work, even beyond the necessary contextual support. One expects to see the reflexive effort of the historian in action, and this book satisfies that expectation. Through a subtle, gentle but firm revaluation of the existing historical literature, the authors manage to highlight some errors which have affected our understanding of nineteenth-century molecular science. Hence we are warned that the frequently assumed unpopularity of molecular ideas, which also formed part of van der Waals's recollections, should be treated with caution. Otherwise the antipathy towards real atoms, which was clear at the turn of the century, can easily lead to unwarranted extrapolations concerning nineteenth-century science in general (p. 31). Maxwell's reaction to van der Waals's work is minutely analysed and this analysis brings to our attention the fact that the two men had more in common, in terms of ideas and methods (p. 134), than historians have so far admitted.

On the other hand, situating van der Waals's work in its broader context of nineteenth-century molecular discourse, which was characterized mainly by an attempt to visualize the unseen level of physical reality (p. 133), whilst adopting at the same time a minimalistic position as regards the specificities of the individual particles which were, as van der Waals described them, 'small bodies with a real volume' (p. 127), does not overshadow the significance of his breakthrough in the slightest. Even if one ignores the tremendous impetus that his work on corresponding states gave to the field of low-temperature physics, by specifying the conditions under which helium would be liquefiable, and by giving to Kamerlingh Onnes the certainty that his effort was not a chimera, the fact that van der Waals managed to transform the simple and elegant equation of the ideal gas into an equally simple and elegant equation of the real fluid was an achievement of immense value at a time when the question of the adequacy of mathematical abstractions to describe properly the complexities of real phenomena had given rise to great anxiety in certain scientific circles.

Above all, this book provides an example of what would be the appropriate historical treatment of certain opinions concerning national schools of thought. Hence the discussion of the Liberal government's reforms of the Dutch system of education around the middle of the nineteenth century, a reform which proved crucial to van der Waals's career in more than one instance, which raised the standards of scientific education and which increased both teaching and research positions, makes it obvious that one needs more than some superficial similarities in genres of writing to sustain the claim that a national thought-collective existed. It shows that one needs to trace the ways in which a social structure creates the conditions that give rise to a discourse which merges national pride with scientific achievement. Starting with the political choices of a given society, one can then proceed to examine the way in which different individuals enact this vision and give substance to it in their life and work. Personal authority and power, but also the charisma of a teacher to inspire his students, were the vehicles of consolidation of specific ways of thinking. And if van der Waals combined, in his own version of the co-ordinates of national science, the nineteenth-century rhetoric of 'national character' with the more realistic evaluation of the significance of the 'system of education' (p. 142), it was his influential position as an administrator of scientific affairs that defended, and sometimes explicitly enforced, the specific choices which made Dutch science what it was. If there was a 'Dutch school of thought', van der Waals was, both literally and metaphorically, its greatest teacher.

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Leila Zenderland, **Measuring Minds: Henry Herbert Goddard and the Origins of American Intelligence Testing.** Cambridge: Cambridge University Press, 1998. Pp. vii + 466. ISBN 0-521-44373-3. £45.00, $64.95.

Intelligence testing has been the object of heated debate and controversy since its introduction in the early part of the twentieth century. In *Measuring the Mind*, Leila Zenderland examines the establishment of intelligence testing in the USA through the life and career of the psychologist Henry Herbert Goddard. Subsequently vilified and ridiculed, Goddard's work has been used to exemplify, amongst other things, the dangerous impact of personal values on scientific enquiry, the naïvety of extreme hereditarianism and the horror of eugenicist politics. In short, Goddard has been portrayed as one of the baddies. In this fine book, Zenderland encourages the reader to put aside present-day judgements and to follow her more historically sensitive analysis. She manages this skilfully, never allowing the reader to doubt that condemning eugenics and bad science is right and proper, but making it clear that her agenda is different: to understand Goddard's work and the rise of intelligence testing in context.

The book establishes the importance of intelligence testing in the early twentieth-century USA by example rather than by assertion: it discusses its impact on institutions for the feebleminded, on schooling, on disputes over criminal responsibility, on the claimed causes of poverty, on political movements and on procedures in military recruitment, leaving the reader with little doubt about the importance of the testing movement. The breadth of this impact as well as the quality of Zenderland's exposition make *Measuring the Mind* of interest to a wide range of audiences.

The early chapters of the book examine Goddard's childhood and youth and make it clear that his Quaker upbringing and the nature of his education were critical in establishing guidance and care as themes that persisted through his career. The author returns repeatedly to a related idea: that both Goddard's work and the early appeal of intelligence testing can be understood in terms of a rich blending of older moralities with new science. In the remainder of the book, Zenderland concentrates on the period from when Goddard first encountered the work of Alfred Binet in 1908 to the end of the First World War. Her decision to concentrate on this period is an important counter to those many histories of intelligence testing that have taken the First World War and the mass testing of American soldiers to be the moment when such testing gained a place in American society.

Zenderland places great emphasis on discontinuities between this early period of intelligence testing and later discussions of issues such as heredity. She demonstrates how Goddard used heredity as a concept that embraced environment and morality as factors affecting inherited intelligence and in so doing shows how he does not fit readily into later classifications of psychologists as either hereditarians or environmentalists. The analysis of this discontinuity and others realizes one of the main stated aims for the book, which is to take discussion of intelligence testing beyond the repeated interpretation of it solely in terms of our present conceptions of heredity–environment. It becomes clear that other issues framed the scientific debate and the promotion of intelligence testing, such as how one was to diagnose deficiency, who required care and what form of education should be provided for those diagnosed as deficient. In her attempt to reconstruct the mental perspective of those involved in the establishment of testing, Zenderland also provides a more subtle and nuanced view of Goddard than we usually receive. He becomes a complex character who, while offering reforms that repeatedly emphasized the need for a science that would inform the care and education of the feebleminded, could also advocate sterilization of the feebleminded. It is a tribute to the quality of this book that such tensions are made understandable.

In Zenderland's account intelligence testing played a part in creating new ways of seeing and measuring children as well as in helping to establish psychologists as a new, if controversial, class of expert. This places intelligence testing within a larger movement whereby children became simultaneously objects of scientific investigation and of intervention. Nevertheless, the establishment of intelligence testing in the USA was not without opposition. The book highlights how testing was contested and how many of the early criticisms of intelligence testing anticipated later (and continuing) attacks on it: disputes over the nature of the purported object of investigation, criticisms of the many inherent biases in the tests, the erosion of the role of clinical judgement, arguments over expertise and jurisdiction, the debatable interpretations of test scores and the weaknesses in many of the sampling methods and statistical analyses. However, it becomes clear that not all of the critics, including that most insightful of journalists, Walter Lippmann, had as their aim the eradication of testing. What they did ask of the testers was a greater modesty in their claims and a more circumscribed use of the tests.

At times Zenderland's reluctance to address broad theoretical themes is a little frustrating and I would have liked a writer of her calibre to give the reader more of her thoughts on issues which run through the book. For example, she quotes Goddard's own reluctance to explain his career in terms of conscious motivation and she touches on the difficulty of ascribing motivation to historical actors at a number of points. I, for one, would like to have read more on this and the general problem of agency in histories of science. Also, despite her explicit rejection of a critical history, I did sometimes find myself hoping for a dash of self-confessed presentism. For example, Zenderland writes of the way in which intelligence testing became 'a scientific product whose popularity was rapidly outstripping their [the psychologists'] ability to control it' (p. 235). Although a passing remark, it is a situation having relevance to many disputes around science and claims to expertise. But these are really requests for a longer book and at 466 pages it is already quite long enough (and I would not have welcomed any excisions from the existing text). Zenderland has presented us with an excellent piece of scholarship.

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V. Y. Frenkel, **Yakov Ilich Frenkel. His Work, Life and Letters**. Basel, Boston and Berlin: Birkhauser, 1996. Pp. viii + 323. ISBN 3-7643-2741-3 (Basel); ISBN 0-8176-2741-3 (Boston). DM198 (no sterling price given).

Whatever the trend in history of science, it seems that physicists and their publishers just cannot get enough scientific biography and autobiography. The last few years have seen a steady flow of accounts of the 'lives and letters' of eminent and not-so-eminent physicists of the recent past, all anxious to attribute scientific credit to their subjects and to promote the virtues of a life in twentieth-century physics. The subject of this volume, Russian physicist Yakov Ilich Frenkel (1894–1952), would seem to be a wonderful candidate for a decent biography. He made significant contributions to several branches of physics, most notably the elaboration of wave mechanics in the context of solid and liquid state physics (he developed the theory of crystal structure which gave us 'Frenkel defects'). He also contributed to electrodynamics and to nuclear physics. A protégé of the Leningrad physicist Abram Ioffe, he travelled in Europe and the United States in the interwar years, and was a central figure in the establishment of Russian physics from the 1930s through to his death in the year before Stalin's. A prolific writer of textbooks and scientific papers, he trained many other leading Soviet physicists and played an important role in the development of the institutions and ideology of Soviet physics.

An artist and musician as well as a scientist, and an important player in the debates over the relationship between physics and Soviet intellectual culture which rocked Russian science in the 1930s and 1940s, Frenkel positively demands a critical biography. Sadly this volume, while adequate in its own way, is deeply disappointing from the historian's perspective. A substantially revised version of a book first published in Russian in 1966 by Frenkel's son (himself a physicist), and compiled with a careful eye to the censor at that time, it has now been edited to remove some extended passages of commentary and to include instead some new material which has emerged since the book's first appearance. Perhaps understandably, its style is personal and idiosyncratic, on occasion veering towards the irritatingly hagiographic. It alternates between lengthy excerpts from Frenkel's correspondence with his family, extended expositions of his scientific work (from the published papers) and anecdotes and homilies by his friends and colleagues. Relentlessly presentist, it attempts to assign to Frenkel the scientific credit which the author takes as his father's due. Even so, much of the material – particularly the ideological debates around Soviet physics, relativity and quantum mechanics – is treated superficially. Though the book is copiously illustrated with photographs of and pictures by Frenkel, unhelpfully there is only a name index.

Whatever their historiographical and stylistic deficiencies and however much their authors would praise great men, scientific biographies can be useful to historians in many ways. It is always good to have otherwise relatively inaccessible foreign material in translation, of course, and in this case Frenkel's own correspondence gives fascinating insights into the social worlds of mid-twentieth-century physics. The volume contains useful material for an understanding of the construction of a career in physics in post-revolutionary Russia, for example. And through Frenkel's reports of his journeys through Germany, France, England and America we see something of the establishment of wave mechanics in the interwar period and its translation across disciplinary and geographical boundaries to become the new orthodoxy in theoretical physics. Here even the anecdotes contain occasional gems which will delight the *cognoscenti*, as when Frenkel wrote to his parents from Göttingen in 1926: 'in the middle of June [Paul] Ehrenfest will arrive with the retinue of his colleagues, a Ceylonese parrot among them which has been taught by Ehrenfest to pronounce the phrase "Aber, meine Herren, das ist keine Physik". This parrot is recommended by Ehrenfest as a chairman of forthcoming discussions of the new quantum mechanics'.

While Frenkel's letters home during his year-long stay in the United States in 1930–1 offer a tantalizing glimpse into the culture of American physics in the ascendant, and demonstrate the wide interest there, among scientists, in the Soviet experiment, the least satisfactory parts of the book are those devoted to Frenkel's wartime experiences and the postwar years in Russia. In its heavily retrospective attempt to demonstrate Frenkel's rectitude in matters of ideology and his 'gigantic scientific legacy', the book glosses over his wartime work in what is by far the shortest chapter. Though it briefly discusses matters of ideology and debates about the role of dialectical materialism in Russian physics, the chapter on

Frenkel's postwar career sheds little new light on the complexities of the context in which he worked or on the ways in which his science reflected that context. It is unfortunate that the book does not refer to any of the excellent historical studies of twentieth-century Soviet physics which have appeared in the last few years – indeed it is a constant puzzle why physicists routinely ignore the writings of historians of science when they make their own forays into history; evidently the elementary principle of the literature survey does not cross disciplinary boundaries. A case here, perhaps, for the re-education of physicists and their publishers alike.

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Michael Riordan and Lillian Hoddeson, **Crystal Fire: The Birth of the Information Age.** New York and London: W. W. Norton & Company, 1997. Pp. x + 352. ISBN 0-393-04124-7. £27.50, $56.95.

Confused by the title? There are bright sparks of an excellent history of the invention of the transistor in *Crystal Fire*, but they are hard to spot among the gaudy fireworks of breathless pop science writing. The guiding metaphor – 'the throbbing heart of this sweeping global transformation is the tiny solid-state amplifier invented by Bardeen, Brattain and Shockley … [the] crystal fire they ignited during those post-war years has radically reshaped the world' (p. 10) – is arresting but feels false: semiconductor devices are insidious, hidden and visually inert, not bright and fiery. The title (and subtitle) are partly therefore conventional hype: more heat than light. But Riordan and Hoddeson also display good scholarly work: they found in the notebooks of the driven, paranoid, repulsive manipulator of people William Shockley the dreamlike jotting: 'Idea of setting world on fire, father proud' (p. 231), a characteristically twisted moment of Shockley hubris, but one which makes the reader feel that the authors' primary research has led to biographical insight. Although this is a popular book, co-authored by a science writer and an academic, it is properly researched, referenced, indexed and provided with a good bibliography. A happy compromise then? Not quite.

The scene is set with an uncontroversial potted history of early twentieth-century physics, and the early biographies of the three protagonists. John Bardeen, son of a Wisconsin medical school dean and an interior decorator, and Walter Brattain, who grew up on a Washington State farm, were adept manipulators of things soon showing signs of technical expertise, dismantling Dodge cars and building crystal radio sets. Shockley's young life was more cosmopolitan and prosperous, if unsettled: his father a roving mining engineer and consultant who died when William was 15, his mother an artist and Stanford graduate, they lived in London before settling in California. Shockley gained a place at MIT, and drove there in 1932 across the United States wearing a beret and leather jacket (there is a remarkable photograph of him posing bare-chested in the Arizona desert) – briefly being detained by Jersey City police who 'pegged him to be a suspicious character' (p. 72). After a Ph.D. on energy bands in crystalline sodium chloride, he moved first to Princeton and then to Bell Laboratories where a culture of research excellence was being built up under Mervin Kelly. The quiet Bardeen also passed through Princeton on the way to Bell Labs, where Brattain already worked, having been recruited in 1929.

A few months before the United States' entry into the Second World War Brattain witnessed a remarkable demonstration of a phenomenon presented by Bell's Dutch-Pennsylvanian Russell Ohl: a massive jump in voltage when a silicon rectifier was exposed to light. Ohl's findings, from work that had nearly been cut by Bell Labs, were mysterious, but seemed to depend on the presence of two types of silicon: lower-grade 'commercial' and 'purified'. (As an aside, there are productive resonances, that the authors could have played upon, between these terms and kinds of research.) Put on the spot by Kelly, Brattain hypothesized the effect in terms of a 'barrier' formed at the junction between the two silicon types. War both concentrated and diverted work at Bell: electronic innovation became central, but it sat on the potentially lucrative P–N junction discovery. American laboratories such as Bell were enviably placed at the war's end: facilities undamaged, expertise built up, and ready to exploit military technologies.

In 1939 Shockley had tried unsuccessfully to devise an electronic amplifier based on the unpredictable semiconducting properties of copper oxide films. He was mortified when, in 1947, Bardeen and Brattain, by pushing a tungsten wire into a block of P–N silicon surrounded by electrolyte, managed to control and even amplify a current. Shockley was informed and he suggested modifications. Further manipulations and trials of different materials (gold foil, germanium, glycol borate 'gu') and arrangements followed and the amplification effect was teased upwards. In December the semiconductor amplifier was demonstrated to Bell executives. Shockley, jealous of Brattain and Bardeen's practical success, wanted a share of the credit, and after important wrangles over patents, returned feverishly to research and, remarkably, in early 1948 proposed a second way of making a semiconductor amplifier: the N–P–N (or P–N–P) junction. The uneasy relations between Shockley on the one hand and Brattain and Bardeen on the other now developed into a rift. However, organizational politics shaped the way the three men have been seen and allocated credit. While privately tensions ran high, the public face of Bell was a display of teamwork and apparent harmony. Kelly insisted that any published photograph of Brattain and Bardeen must also include the hierarchically senior Shockley. Given the context, the deceptively amicable September 1948 cover of *Electronics* magazine was astounding: Bardeen and Brattain stood passively overlooking the active Shockley, who was hunched at the lab-bench, hands on microscope – the direct reverse of the roles taken. ('Boy, Walter sure hates this picture', Bardeen has recalled.) Considerable care was taken to choose the right exciting name: 'iotatron' was rejected in favour of John Pierce's suggestion, 'transistor' (Pierce went on to write science fiction). Further demonstrations, to the military and the press, were stage-managed. The undoubted highlight of this book is the sustained combination of historical insight, relevant detail and sensitivity to both material practices and organizational pressures found in those chapters (Seven and Eight) dealing with the late 1940s.

The remainder of the book is less satisfactory. A very disjointed chapter traces the development of various transistor and diode designs at Bell, before jumping to Shockley's Korean War application of the devices to mortar shell proximity fuses (without making clear to what extent the fuses were used), and then to Bardeen's departure for Illinois. The semiconductor industry was shaped by both military and regulatory interests: ballistic missiles and defence computers provided the market, while AT&T's worries about impending anti-trust actions prompted the sale of manufacturing rights at $25,000 a piece. Shockley's colleague Gordon Teal, an expert in the art of growing crystals, joined the tiny outfit Texas Instruments which produced silicon transistors under military contracts. Until 1954 the only civil commercial application of transistors was in the hearing-aids niche. However, searching for a new market with the end of the Korean War, TI began in a joint-venture manufacturing Regency TR1 transistor radios. Demand outstripped supply of these expensive devices, an opportunity snatched with alacrity by Masuru Ibuka and Akio Morita's fledgeling Japanese company Sony.

It is a shame that only five pages are devoted to Ibuka and Morita, a fact that underlines how *Crystal Fire* is not an account of the spread of semiconductors but a joint biography of Shockley, Bardeen and Brattain. All three shared the 1956 Nobel Prize in physics. Excepting such honours, Shockley's life after 1948 was a disappointment on both personal and professional levels. His attempt to strike it rich in California by exploiting the transistor began with high confidence ('after all, it is obvious I am smarter, more energetic and understand people better than most other folks', p. 232) but foundered when his research team mutinied and went off to form the successful Fairchild Semiconductor firm. (However, it is hard to feel sympathy for a man who started an affair while his wife was dying of cancer, and spent his emeritus Stanford years writing scientific racist tracts – aspects of his life that in half a page are raised and rapidly dropped by the authors.) John Bardeen switched research interests, picking up a second Nobel Prize in 1972 for work on superconductors. Walter Brattain stayed at Bell.

Crystal Fire is part of the public understanding of technology series funded by the Sloan Foundation, some of which have approached that ideal of general readability combined with academic seriousness. However, the results here are rather uneven: excellent and exciting passages are interspersed with ones more humdrum or too gushing (there must be a School of Popular Science Writing somewhere that dogmatically teaches that a chapter should begin with a scientist 'bounding' upstairs). Finally, there is a slight feeling that AT&T – who charge for the use of Bell Labs archives but waived some fees in this case – receive an overly deferential, even reverent, analysis. The 'swift invention of a semiconductor amplifier…proved the wisdom of Kelly's emphasis on basic research in solid-state physics' (p. 141). This eulogy to 'enlightened executives' continues in the epilogue (p. 282), where even 'multilayered complexity' is portrayed as an organizational advantage. Other authors might not have been so kind.

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Paul Thagard. **How Scientists Explain Disease.** Princeton: Princeton University Press, 1999. Pp. xviii + 263. ISBN 0-691-00261-4, £18.95 (hardback).

How Scientists Explain Disease is a strangely alluring book. Its author, the philosopher and cognitive scientist Paul Thagard, proposes to unify the often bitterly conflicting philosophical, sociological and psychological perspectives on the organization and activities of scientific communities by drawing on the theory of distributed artificial intelligence. According to this theory, the cognitive capacity of a modern expert system does not reside in any one component, but in the interactions of independent but interconnected centres of calculation. Yet there is something deeply unsettling or, more appropriately, stressful about Thagard's argument. While he draws a very close analogy between modern expert systems and the organization of scientific communities, he firmly rejects Harry Collins's view that 'what we are as individuals is but a symptom of the groups in which the irreducible quantum of knowledge is located'. Perhaps there is more than meets the eye in what Michael Ruse has described on the dust jacket of *How Scientists Explain Disease* as 'a delightful essay combining science, its history, philosophy and sociology'.

Thagard opens his promising argument by criticizing philosophers and sociologists of science for being overly reductionist. Scientific investigation is for him neither the abstract set of logical procedures once imagined by the former, nor is it the reflection of social interests imagined by the latter. It is instead an 'integrated cognitive-social' phenomenon whose exact characteristics are revealed by Thagard as he examines the development of the bacterial theory of peptic ulcers.

As Thagard points out, peptic ulcers were once attributed to some physiological anomaly, stress, which caused the overproduction of gastric acid, but from the mid-1990s they have been attributed increasingly to infections by the bacterium *Helicobacter pylori*. Thagard then describes the complex sequence of pathologists' serendipitous discoveries of *Campylobacter pylori* in ulcerated tissues, of pathologists' and gastroentorologists' redefinition of *Campylobacter* as *Helicobacter* as they articulated their new infectious theory, and the eventual trial of antibiotics to treat peptic ulcer. No more need then for Peptol, or for Zantac and Tagamet, 'two of the most lucrative drugs ever produced' (p. 93). Most of the actors that historians of modern biomedical science would expect to figure in such a revolutionary development, from professional bodies and pharmaceutical companies to scientific technologies, such as the electron microscope, appear in Thagard's narrative. There is of course one notable omission, namely the dyspeptic patient, but many of these historians would be equally at fault.

For Thagard, the development of the theory that peptic ulcer is due to a bacterial infection is an indisputably social phenomenon. However, he also argues against sociologists of science that this development was mediated by cognitive, or psychological, processes, such as moving from statistical correlations between infection and clinical symptoms to the causal linking of the two. He then claims that for all their disagreements, it is perfectly possible to integrate the perspectives of sociologists and philosophers of science more attuned to developments in psychology. The claim is difficult to believe, but Thagard takes heart from the observation that 'as Bloor pointed out in the second edition of one of the books that spawned the sociology of scientific knowledge ... sociologists would be "foolish" to deny the need for a background theory about individual cognitive processes' (p. 14). The problem, however, is that the complex sequence from discovery to therapy described by Thagard unfolds in a thoroughly rational manner, as opponents of the infectious theory, who believed that it was 'a totally crazy hypothesis' (p. 56), are convinced about its merits by an incontrovertible combination of microbiological and epidemiological data. Moreover, the final outcome, namely that *H. pylori* is the cause of peptic ulcers, is not a construction, social or philosophical, but a reflection of an underlying, independent material reality. In other words, Thagard is a scientific realist who believes that the convergence of his disparate actors on 'a totally crazy hypothesis' would have been inconceivable if it had not been for the existence of bacteria such as *H. pylori* 'for hundreds of millions of years before people and their societies came on the scene' (p. 219).

Thus, while calling for the integration of sociological, philosophical and psychological perspectives on the organization and activities of scientific communities, Thagard actually returns to Thomas Merton's version of sociology of science, by reducing its remit to the analysis of the context of production. Thagard is undoubtedly correct to view early versions of the more ambitious programme to produce a sociology of scientific knowledge as reductionist when they explained knowledge as nothing more than a reflection of conceptually prior social interests. However, the claim that constructionism, after Andrew Pickering or Bruno Latour, continues to deny any agency to events in the laboratory or under the microscope, is simply untenable. These sociologists, if one may still label them as such, may not pay as much attention to published papers as Thagard does, but they certainly pay much greater attention than Thagard to the details of scientific practice and the enormous amount of work needed to stabilize the meaning of statements such as '*H. pylori* causes peptic ulcers'. What actually divides Thagard and these sociologists is not reductionism, but the status of rationality. For Thagard, the rational evaluation of empirical data is not a socially contingent phenomenon but an innate human feature. Strangely, however, he explains at considerable length the cognitive reasons why adults and children are equally capable of moving from a correlation between two series of phenomena to a causal relationship. Then, when he returns to his case study, he discusses at similar length how a Consensus Conference organized by the National Institutes of Health had to be called upon to adjudicate the merits of treating peptic ulcers with antibiotics – because not everyone infected with *H. pylori* develops ulcers, and some develop ulcers without being infected. In other words, for the practising physician treating a peptic ulcer and the gastroenterologist convinced that *H. pylori* causes peptic ulcers, the causal link between bacteria and clinical symptoms was far from indisputable. Thagard escapes from this inconsistency by arguing that the logic of practising physicians, lacking access to advanced diagnostic equipment and badly informed about the most recent developments in biomedical science, is different from that of the gastroenterologist. Because he is not interested just in the description of, but also in establishing norms for, a more effective expert system, he then advocates the controversial notion of 'evidence-based medicine' beloved by insurance companies and national health services intent on cutting their costs. Needless to say, discussing why physicians could ever object to 'evidence-based medicine', which Thagard says 'initially struck me as internally redundant: what else could medicine be based on?' (p. 188), is outside Thagard's remit. A philosopher's refusal to discuss why evidence and truth might be in the eye of the beholder seems to me mighty strange.

This said, Thagard is again correct to emphasize how too many sociologists and philosophers of science have paid little attention to why experiments fail. As he points out, the notion of reality as a philosophical construction or the outcome of negotiations between different social groups is irreconcilable with the failure, for example, to develop animal models of disease for *H. pylori*. Again, however, such failure only suggests that the pathologist and gastroenterologist are engaged with some external agent, and Latour would argue that it is out of the trial of strength between gastroenterologist and pathologists, on the one hand, and this agent, on the other, that something called *H. pylori* comes into being. That the resultant network of actants is so effective that antibiotics now successfully treat most peptic ulcers provides no evidence that science is the mirror of nature. As Jean Baudrillard pointed out, the crash some fifteen years ago of financial markets across the world illustrates brilliantly how an extended network can be highly productive and effective, and none the less be disconnected from its putative material counterpart.

Finally, Thagard writes at length about Collins's criticism of cognitive science as resting on some notion of individualism. He first dismisses the criticism as based on outdated conceptions of cognitive science, though he will not admit as much about his own understanding of science studies. He then argues that the truth that *H. pylori* causes peptic ulcers, like the truths of distributed artificial intelligence, rests on a network of disparate actors and not in any one centre of calculation. Not surprisingly, Thagard thinks that his approach shares much with Latour's actor network theory, but also that it has the advantage of not having to attribute to 'other non-human actants the same cognitive status as human scientists' (p. 224). It seems to me odd that someone as attuned to psychology as Thagard should conflate agents and actants. This perhaps is because ultimately Thagard understands the extended network that secures the existence of *H. pylori* as a prosthetic device for the agent of classical social theory. Unfortunately this problem is again far from peculiar to Thagard and has landed sociology of science in many a conceptual problem. Needless to say, Thagard has no time for cultural critics such as Paul Virilio, who has discussed at length the spectacular contemporary development of artificial intelligence with which Thagard concludes *How Scientists Explain Disease*, and who concludes that the human subject is no longer. Though Thagard describes quite neatly a world in which classical individualism is increasingly meaningless, his argument amounts to little more than the philosophy of science which he debunks at the outset, though now armoured with the newest innovations in cognitive psychology and computer science. We can now programme an expert system capable of reproducing the discovery that *H. pylori* causes peptic ulcers, or of revealing that we are automatons whose behaviour is determined by our genetic code. It matters little that for a while yet many patients will have antibiotics prescribed for them, and that they will do nothing for poor dyspeptic reviewers. I'll stick to cheap and cheerful Peptol … and the truth be damned!

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Isaac Newton. **The Principia: Mathematical Principles of Natural Philosophy, 3rd edition (1726). Newly translated by I. Bernard Cohen and Anne Whitman. With a supplement by I. Bernard Cohen**. Berkeley: University of California Press, 1999. Pp. 1025. ISBN 0-520-08816-6. £60.00, $75.00 (cloth); 0-520-08817-4, £24.95, $35.00 (paperback).

In the year that Newton's *Principia* has been acclaimed by one poll as second only to the Gutenberg printing press as the most influential achievement of the millennium, the publication of a new English translation is an auspicious and welcome event. The first English translation of the *Principia*, by Andrew Motte, was published in 1729, three years after the publication of the third Latin edition and two years after Newton's death in 1727. There have been a number of 'revisions' of Motte's translation, most notably that by Cajori in 1934, but the Cohen-Whitman edition is the first new translation in 270 years. In addition, the publication contains a 379-page guide to the *Principia*.

Most classic works (such as Plato's *Republic*) have multiple translations. Why then have we not had multiple translations of the *Principia* or, perhaps more to the point, why, if the Motte translation has sufficed for almost three centuries, do we need a new one now? Surely the mathematical principles have not changed. Cohen addresses a similar question in the second chapter of his guide to the *Principia*. After noting the obvious limitation of the archaic style, language and punctuation of Motte's 1729 edition (the limitations that prompted the 'revisions' by Cajori and others), Cohen speaks of a much more fundamental problem: some of the phrases and passages in Motte's translation are based on the second edition rather than the third edition, and moreover he has introduced some phrases that are not in the published text (p. 28).

Motte's intrusive phrases are intended to help the reader through difficult passages, but despite his good intentions they are misrepresented as being from Newton. Most of these insertions are not of 'tremendous significance', Cohen notes, but important exceptions do occur; for example, the words 'electric and elastic' are added by Motte in the General Scholium to qualify the word 'spirit':

> In his interleaved copy of the second edition containing corrections and emendations for a third edition, Newton qualified the word 'spirit' by the adjectives, 'electric and elastic'. This emendation did not find its way into the third edition, but it was communicated to Andrew Motte, who inserted these words into his English translation. Newton also indicated later his intention to cancel the whole final paragraph. He no longer considered it to be an accurate presentation of his ideas (p. 282).

In Motte, therefore, the final sentence of the *Principia* concludes with the phrase 'the laws by which this electric and elastic spirit operates', and it stands in contrast to the final sentence of the Cohen-Whitman translation 'the laws governing the action of this spirit' – a significant difference.

There are a few other revisions of the Latin second edition that do not appear in Motte's English translation of the Latin third edition. It may be that Motte used the second rather than the third edition as the primary source for his translation and then revised it. Cohen does not speculate on the reason for such a confusion; he simply demonstrates that it is so. Perhaps the answer lies in the commercial advantage for Motte of having the English translation published quickly following the much anticipated publication of the third Latin edition. Andrew Motte's brother Philip was the publisher of the translation, and perhaps he encouraged Andrew to get a head start by translating the second edition of 1713 and then revising it by comparison with the third edition when it appeared in 1726. In any event, it is clear that the reader of the Motte translation would be well advised to compare it to the Latin editions before making key comments upon specific phrases.

It would appear, however, that the Motte-Cajori revision of 1934 was made without regard for this qualification. Cohen demonstrates in detail that in this edition most revisions were made without reference to the Latin original, and it is this edition which is now most widely cited (a paperback edition was published in 1965). Moreover, Cohen reports that not only were some revisions made without consulting the 1726 Latin edition, but some revisions are not even consistent with the 1729 Motte translation itself. In notes 14 and 39, however, Cajori does make use of both Latin and English translations of multiple editions of the *Principia* to make his point. Why, then, would a scholar with a long and distinguished career not carry through in the rest of his work the same care evidenced in these early notes? The answer lies in the fact that Cajori died in 1930 with only a partially completed revision of the work. In a brief note from the editor, we find that R. T. Crawford was 'invited by the University of California Press to edit this work'. There is no indication of which revisions were made by Crawford and which by Cajori. In any event, Cohen provides ample evidence of the failure of the Motte-Cajori (Crawford?) edition to represent Newton's original text faithfully. Rather than make a parade of the faults of the Motte-Cajori edition, Cohen elects to discuss in some detail three examples: one on comets, one on projectiles and one on a philosophical position expressed in a scholium to the definitions (and he notes in conclusion that the title 'Mr Machin Astron. Prof. Gresh.' appears as 'Mr Machin and Professor Gresham' instead of 'Mr Machin, Gresham Professor of Astronomy').

As an example of the problems that can arise from a faulty 'correction', consider the Motte-Cajori version of Lemma 11 of Book 1. My colleague Bruce Pourciau was working on a paper concerning the mathematical lemmas of Section 1, Book 1, and how these lemmas reveal Newton's command of the limit concept. In this paper Pourciau opposed the widely held view that Newton had no clear understanding of the basic limit process, but he found one lemma where Newton did in fact appear to be confused. In Lemma 11, the curvature lemma, Newton demonstrates that the ratio of the squares of the chords of two circles drawn tangent to a given curve is equal to the ratio of the product of their diameters and subtenses. When this result was 'modernized' in the Motte-Cajori edition, the numerators of the two ratios were made equal, as were the two denominators. This result goes beyond what Newton had actually demonstrated in the body of the proof of the lemma. Fortunately, Pourciau decided to change the references in his article from the 1934 Motte-Cajori translation to the 1999 Cohen-Whitman translation. In that edition, however, the ratios were equated correctly and a subsequent check with the 1729 Motte edition proved also to be correct. The error had been introduced by the Cajori (Crawford) revisions of the Motte translation, and with this correction Newton's strange failure with limits disappeared.

The first 370 pages of the work constitute a guide by Cohen, which he intends as 'a kind of road map through the sometimes labyrinthine passageways of the *Principia* (p. xiv). The first five of ten chapters serve as a background, while the final five chapters analyse the structure of the work itself. In the first five chapters one finds a brief history of the *Principia*, a commentary on earlier translations, some remarks on the general aspects (from Newton's philosophical goals to his methods of analysis), a review of some fundamental dynamic concepts and a discussion of the axioms set out in opening section of the *Principia*. In the last five chapters one finds an outline of the structure of Books 1, 2, and 3, a discussion of the General Scholium, and a concluding chapter entitled 'How to Read the *Principia*', which concludes with nine examples of specific solutions to problems generated by Newton.

Cohen is generous in his attribution of credit to the work of contemporary colleagues and earlier eighteenth- and nineteenth-century scholars. The translation itself is dedicated to Tom Whiteside, and the influence of his notes and commentaries in the eight volumes of *The Mathematical Papers of Isaac Newton* is evident throughout the guide, as is the work of Sam Westfall in his scientific biography of Newton, *Never at Rest*, and the work of other colleagues such as Rupert Hall and Curtis Wilson. The year 1995 gave rise to five new books, each serving in some fashion to provide guidance to the *Principia* for the interested reader, and they are acknowledged. Cohen is also aware of and sensitive to the work of scholars who are relatively new to the field. In particular, the guide includes a section written by Michael Nauenberg on the new findings concerning Newton's curvature measure of force and sections by George Smith on an alternate view of Book 2 and a discussion of lunar motion in Book 3. It is the scholarship of Cohen, a leader in Newtonian studies for over half a century, that provides the unique vantage point for this incomparable overview of the background and contents of the *Principia*.

The jewel in the crown, however, is the translation itself. Cohen reports that in 1972, following the publication with Alexandre Koyré of the variorum edition of Newton's third Latin edition, he considered making a revision of the Motte translation. He even had an interleaved copy of a facsimile edition of Motte produced to serve as a working text. He tells us, however, that colleagues and scholarly friends urged him to take on the much larger task of an entirely new translation. He finally decided to accept the challenge, and with the collaboration of Anne Whitman, a Latin scholar, he began the work. Tom Whiteside advised them not to consult existing translations, even when confronted by vexing problems, until they had completed a full draft of their own translation. It was sound advice, and however difficult it was to resist the temptation, they did not consult other translations on specific points until the final two rounds of their multiple revisions. After their translation was completed and checked several times against the Latin original, it was compared to the Motte translation. This comparison revealed many similarities in language, particularly in the mathematical expressions, and it also brought to light a number of variations from the Latin introduced by Motte, variations that might not have been noted had they carried through with the original plan simply to revise the Motte edition. They then considerately made, in my opinion, a very generous decision:

> Taking into account, however, that Motte's phrasing represents the prose of Newton's own day and that in various forms his rendition has been the standard for the English-reading world for almost three centuries, we decided that we would maintain some continuity with this tradition by making our phrasing conform to some degree to Motte's (p. xii).

The translation also benefits from Cohen's earlier work with Alexandre Koyré on the variorum edition of Newton's third Latin edition. Cohen's abundant footnotes in the translation itself point out the important revisions made by Newton in the successive Latin editions of 1687, 1713, and 1726. Beyond these editorial comments, the footnotes contain discussions of particular choices made in translation, expansions of a point in question in the text and references to work by other scholars relative to the section. These notes go beyond the commentaries of the opening guide because they provide an insight for the reader to the evolution of Newton's thought, and they do so in the context of the work itself. Only a scholar of Cohen's broad experience and deep devotion could supply such a running commentary.

Anne Whitman died in 1984, when the final version of the translation was about to be completed after two decades. Cohen continued to prepare it for publication with the help of Julia Budenz. The result is a superb translation, clear and concise. It will serve the scholarly world for at least another three centuries and will stand as a memorial to I. Bernard Cohen's dedication to scholarship.

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Andrea Carlino (Translated by John Tedeschi and Anne C. Tedeschi). **Books of the Body: Anatomical Ritual and Renaissance Learning**. Chicago: University of Chicago Press, 1999.

Published in 1994 as *La fabbrica del corpo: Libri e dissezione nel Rinascimento*, this is a welcome translation of Andrea Carlino's exploration of the 'intellectual inertia' that characterized the teaching of anatomy in Renaissance Italy. Beginning with iconographic representations of the quodlibetan model of dissection (comprising *sector*/dissector, *ostensor*/demonstrator, *lector*/reader, and *disputatio*, the discussion following practical demonstration), Carlino traces the pedagogical shift from textual to bodily authority, taking early representations as his starting point and ending with the changes implicit in the title pages of Vesalius' *De humani corporis fabrica*. Noting that the didactic and investigative Vesalian model had also been adopted by a number of physician-anatomists who were active in Italy between the end of the fifteenth and the beginning of the sixteenth centuries, Carlino asks why anatomy was not freed earlier 'from the authority of Galen and the monopoly of the Galenists'.

He finds psychological, sociological and anthropological answers to this question by placing the practice of anatomy within the cultural context of Renaissance Rome: significant as the capital of the Christian world, and as the seat of a university where dissection as an academic activity was 'shrewdly filtered through the religious authorities and controlled by political and judicial institutions'. The psychological and sociological factors explored by Carlino include the continued reliance upon and reverence for the authority of the ancients, and the difficulties, inherent in the new anatomical method, associated with the redefining of socioprofessional and didactic status.

The anthropological factors are discussed in considerable detail, and are predicated upon such cultural determinants as 'the revulsion generated by contact with cadavers', funerary practices, the status of the body chosen for dissection, forms of execution and the judicial and religious practices surrounding the individual destined for the dissection table. Carlino shows that these 'seemingly marginal [anthropological] aspects' are of central importance, and are detectable in ancient, medieval and Renaissance writings. Through this cultural continuum Carlino traces 'a common fear of contamination by proximity to the impure… [which] affected the physician and…student alike…[and] effectively obstructed the possibility of conducting dissections and the means of acquiring knowledge through so doing'. By contextualizing the practice of dissection within academic, religious and judicial ritual, Carlino argues, we can better understand the persistence and continuity of such qualms and how they were confronted and accommodated. Academic ritual entailed the solemn performance of the public anatomy lesson: an act that reinforced the theoretical and philosophical nature of dissection and further emphasized the socioprofessional distinctions between the learned university trained physician and the mass of other healers. Religious and judicial ritual was formulated to offset the enduring anthropological revulsion for dissection. Carlino's integration of factors such as the legal criteria for procuring cadavers, the concern for the spiritual comfort of the condemned and his or her family, and the legal and religious procedures prescribed for the removal of the body from the gallows, highlight the enduring necessity of addressing the problems of distaste surrounding the practice of dissection.

Through his investigation of the textual evidence, Carlino finds that even after the anatomical reforms of the sixteenth century promoted the legitimacy and utility of anatomical dissection, contact with 'the dead and with blood [and] the comparison of the anatomist with an executioner' militated against the perception of dissection as little other than 'wicked…unworthy…cruel…and vile'. Combining the 'conflict between scientific exigencies and anthropological resistance' reminds us that 'we are dealing with acts, choices and behaviour, with which…human beings attempt to restrain death by means of ritual'.

By providing an explanation for the persistence of the Galenic anatomical model which goes beyond the usual ones of reverential adherence to the authority of ancient texts and the limitations of the humoral theory of disease, Carlino's detailed study adds a further important and convincing facet to the history of Renaissance dissection and is a welcome addition to the historiography. Generously illustrated, one source of disappointment is that there are no translations of the many citations from original Latin or Italian texts.

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DEEPAK KUMAR. **Science and the Raj, 1857–1905.** Delhi: Oxford University Press, 1997. Pp. xv + 273. ISBN 0-19-564194-9. £4.50, $10.95.

Science has become an increasingly enigmatic area of British imperialism. On the one hand, English education and institutions did not treat the sciences as a high priority. On the other, the work of outstanding individuals and major societies contributed notably to the science of imperialism in the late eighteenth and nineteenth centuries. Scots constituted a vital source of scientific endeavour throughout the empire, particularly in the natural and biological sciences. Moreover, we are becoming increasingly aware of the significance of contrasting levels of influence that seem to cut across the supposedly 'normal' imperial flows. In many places, indigenous knowledge was extremely important in informing westerners, particularly in the natural sciences. So was international activity in science. The apparent nationalistic thrust of imperialism was in some ways subverted by international exchanges. Environmental knowledge and theory were very much in the international domain. So was work on astronomy, botany, foresty, medicine and, from the end of the nineteenth century, bacteriological studies. The British employed Europeans, notably French, German and Austrian scientists, botanists and foresters, throughout the empire when it suited them. By the beginning of the twentieth century, the cross-cutting of imperialism and internationalism in science had become complex and highly significant, and is as yet not fully understood.

Some of these enigmas do surface in Deepak Kumar's work. Kumar has been one of the pioneers of the social and administrative history of science in India. Here he covers a broader span than the dates in the title suggest and offers a valuable fivefold periodization of the British scientific encounter with India: initial exploration and encounters with the Indian environment in the widest sense; the organizational imperatives of the administration of a colonial science, particularly in botany, geology, survey and forestry; the beginnings of a scientific education; the emergence of scientific research within India; and the responses and resistance of Indians themselves. Kumar's approach is essentially empirical and is often repetitive, particularly when he repeats quotations from one chapter to another, but the research is very well grounded in primary sources and reflects the author's inclusive approach to the sciences. He charts the shifts from 'amateur' to 'professional' science, from 'science as avocation', as he puts it, to 'science as enterprise'. He accepts some of the British evaluations of Indian science (for example dubbing Jai Singh's eighteenth-century astronomy as being essentially medieval), but notes the areas in which India was more advanced than Europe, in dye-making for instance. But, despite the hostility to the sciences shown by key figures such as Sir William Jones and Lord Macaulay, somehow scientific education did stumble into existence in India in the nineteenth century, permitting Indians to start to collaborate in Western science.

Kumar opens the book with a consideration of the various theories of diffusion and exchange which have been proposed for imperial science, but his empirical work is not geared to the modification or advance of such theoretical insights. Although he mentions the possibility of a negotiated colonial relationship through science, he does little to develop such an idea. By the end he seems to accept that in the Indian case the development of Western science was primarily a one-way transfer, a dependent mimicry rather than a two-way exchange, even if there were some 'moments of autonomy'. Certainly the British saw their science and technology as a marker of progress and civilization, both the distinguishing characteristic of and, almost, the justification for their rule. They also created a sort of hierarchy of shared status. Thus, although the British Association for the Advancement of Science had meetings in Canada, South Africa and Australia in the late nineteenth and early twentieth centuries, India, for all its importance, was excluded from such a vision of a peripatetic imperial science. Yet the negotiation of knowledge, at least in the natural sciences, was unquestionably geographically specific. Indians were involved, albeit in racially determined lower levels, in surveys of all sorts, in botany, forestry, hydrology and agronomy, as well as in veterinary, medical and bacteriological research. This list could perhaps be extended. It remains for other Indian historians to offer insights into such activity as well as a further contribution to science as a realm of exchange as well as a tool in the patterns of dominance.

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