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Book reviews

HELAINE SELIN (ed.) and UBIRATAN D'AMBROSIO (advisory ed.), **Mathematics across Cultures: The History of Non-Western Mathematics**. Science across Cultures: The History of Non-Western Science, 2. Dordrecht and London: Kluwer Academic Publishers, 2000. Pp. xx + 479. ISBN 0-7923-6481-3. £135.00, $217.00 (hardback).
DOI: 10.1017/S0007087404216685

This interesting but uneven collection tackles the daunting task of identifying and describing 'non-Western mathematics'. The term 'Western mathematics' is commonly used to refer to at least three related but distinct developments: i) Hellenistic Greek mathematics, particularly deductive geometry using rigorous proof, considered by many philosophers an ideal of epistemic certainty; ii) early modern European mathematics in Latin and vernaculars, derived from rediscovered classical texts, medieval philosophy and mathematical ideas and practices from Asia; and iii) modern 'universal' mathematics in all major languages, descended from ii) and spreading globally with Western technology and political dominance.

'Non-Western mathematics', one supposes, must then mean everything else. In practice, though, it seems to be most frequently used for one of the following: a) literate 'academic' mathematics in non-European languages, some of which influenced or were influenced by Western mathematics; b) quantification practices such as mensuration, number systems, game-scoring and computation in non-literate or partially literate cultures; c) other practices and artefacts employing more general concepts now often associated with mathematics, such as symmetry, mapping or classification.

Developments of type a), and some of type b), have been studied primarily in the discipline of history of mathematics, often with special regard to their relation to Western mathematics. Types b) and c) have typically been of more interest to anthropologists and teachers; nowadays they are often considered the province of 'ethnomathematics', dealing with, e.g., the history, anthropology and pedagogy of mathematical thinking in traditional societies. The reasons for studying all these various developments are equally diverse: some researchers incorporate them into mainstream history of science or anthropology; some seek to enrich modern mathematics pedagogy or to reform eurocentric attitudes.

Mathematics across Cultures excludes none of these perspectives or approaches, thus gaining in scope what it loses in coherence. Its twenty-one essays are divided into an introductory section on the historiography of non-Western mathematics and five regional sections dealing respectively with West Asia and North Africa, the Americas, the Pacific region, sub-Saharan Africa and East and South Asia. Within each regional section, individual essays treat distinct cultures or groups of cultures. Several of these are conventional investigations in the history of (academic) mathematics, rich in technical and textual detail; others are anthropological studies of non-literate mathematics, or discussions of ethnomathematical resources for mathematics teaching.

Certainly there is a pressing need for more accessible information about mathematics outside the European tradition(s) – although, as Walter Sizer astutely remarks in his article on Pacific cultures, that mathematics is still defined as the practices and ideas that modern scholars consider 'mathematical', whether they are specific quantification techniques or broader concepts like 'pattern' or 'variability'. Many subjects – particularly within non-Western 'academic' mathematics – are well served here by admirably lucid and informative historical surveys, e.g. Jean-Claude Martzloff's discussion of Chinese mathematical astronomy and Eleanor Robson's fascinating synthesis of textual and archaeological evidence to reconstruct early Mesopotamian mathematical culture. Some other articles, however, would have benefited from more reliable or up-to-date sources (e.g. that on Indian mathematics, which cites no publication more recent than 1979) or clearer exposition (such as the discussion of Australian kinship relations).

On a deeper level, some crucial issues are never directly addressed. For example, why should we lump so many different traditions, methodologies and cultures together as 'the study of

non-Western mathematics'? Considering that, say, some Arabic and Latin mathematical texts in the sixteenth century hardly differed more than some British and French ones in the eighteenth, is it meaningful to set Arabic mathematics in an intellectual domain that excludes Latin mathematics but includes non-literate mathematics in indigenous cultures? 'Non-Western' here takes on an almost purely political sense to mean cultures dominated or discounted by Western colonial expansion: 'colonialism led to a disparaging and belittling of the colonized cultures and their mathematical and scientific achievements' (p. xix). But, paradoxically, it is also applied to academic mathematics produced in the colonialist societies of early modern Portugal and Spain, on the grounds that those productions were neglected by some later historians of mathematics: 'We might say the Iberian science up to the Enlightenment fits into the characterization of "non-Western"' (p. 86). By this point, the term 'non-Western' has been reduced to meaning little more than 'belittled or disparaged'.

Moreover, why should we assume that the study of non-Western mathematics constitutes a tool for ethical advancement? The moral nature of the enterprise is implied by several contributors who assert that it is incumbent on us to 'extricate ourselves from the projects of European colonizing' (p. 72), to accept that 'survival depends on a global and holistic view of reality' (p. 85), to realize that 'the perspectives of all peoples, cultures and experiences are of value and deserve an equal voice' (p. 249), and even to create 'a new planetary order without inequality, arrogance or bigotry' (p. 89). But it is never explained why the moral desiderata of holism, equity, escape from colonialism and so forth are less ideologically suspect than our forebears' avowed goals of knowledge, rationality, progress, civilization and so forth. I know of no reason to think that we twenty-first-century multiculturalists are less swayed by self-laudatory, self-interested motives than were nineteenth-century imperialists, and future generations of critics will surely not spare our illusions. This prospect of mutually assured deconstruction unfortunately seldom deters us from taking our ethical justifications for granted. More reflection on fundamental questions of this sort would have made *Mathematics across Cultures* a more consistently satisfying book.

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H. T. Huang, **Science and Civilisation in China. Volume 6: Biology and Biological Technology. Part V: Fermentations and Food Science**. Joseph Needham: Science and Civilisation in China. Cambridge: Cambridge University Press, 2000. Pp. xviii + 741. ISBN 0-521-65270-7. £95.00 (hardback).
DOI: 10.1017/S0007087404226681

This volume, the twenty-first to appear in Joseph Needham's *Science and Civilisation in China*, is primarily concerned with the many Chinese food technologies which involve fermentation. These include the preparation of alcoholic drinks; soybean products such as bean curd and soy sauce; green, red and black tea; malt sugar; and a variety of preserves and sauces. It also considers briefly a number of related issues, including Chinese approaches to nutritional-deficiency diseases.

The context in which these technologies have been used is covered in a 100-page introduction to the food resources and culinary system of ancient China. This section is largely concerned with the classical period of Chinese history, up to the end of the Han Dynasty (206 BC–AD 220), and slights somewhat the changes which have come later. It is nevertheless a valuable concise introduction to the things the Chinese eat and the ways in which they prepare and eat them.

The section on alcohol production is also an introduction to what H. T. Huang in his conclusion calls 'the wonderful world of the grain moulds' (p. 592). This refers primarily to a range of remarkable products called *ch'ü*. The word is often inaccurately translated 'yeast'; Huang uses

the translation '*ferment*' (always italicized). These are made by the controlled exposure of cooked grain to organisms naturally present in the environment, and modern analyses indicate that *ferments* contain a wide variety of moulds, yeasts and bacteria (pp. 280, 592). In Chinese methods of preparing alcoholic drinks from grain ('rice wines'), a *ferment* provides both fungal enzymes for saccharification of starch, and yeasts to produce alcohol from sugar. It is possible that in early China the Western method of producing beer was used, with sprouted grain (malt) providing enzymes for saccharification and fruit yeasts to produce alcohol, but the use of *ferment* for these purposes has been standard in China since the late Han period.

Though it seems clear that the *ferments* were developed in the context of alcohol production, they have turned out to be useful in a wide variety of other food processes, especially preparation of the bewildering variety of soybean, vegetable, meat and fish preserves and sauces which are so important in Chinese cuisine. 'The enzymes present [in *ferments*] that are relevant to food processing include amylases that hydrolyse starch to sugars, proteinases that hydrolyse proteins to peptides and amino acids, pectinases that hydrolyse pectin to uronic acids, and lipases that hydrolyse fats to glycerol and fatty acids' (p. 593).

H. T. Huang is a distinguished biochemist as well as being deeply learned in the Chinese tradition, and he shares with Joseph Needham a penchant for 'brass tacks'. The sentence quoted just above is rather more abstruse than most of his technical explanations, but readers should be prepared for some tough biochemistry here and there. A good part of the book consists of long translations of Chinese recipes and descriptions of industrial processes, followed by technical exegesis. Translations are in any case rarely easy to read, and Huang's technical explanations are often demanding, but the reader who perseveres will find the book rewarding.

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Owen Gingerich, **An Annotated Census of Copernicus' *De revolutionibus* (Nuremberg, 1543 and Basel, 1566)**. Studia Copernicana – Brill's Series, 2. Leiden, Boston, MA and Köln: Brill, 2002. Pp. xxxi + 402. ISBN 90-04-11466-1. $132.00, €113.00 (hardback).
DOI: 10.1017/S0007087404236688

The book under review is a labour of love, for the author has devoted some thirty years to tracking down the surviving copies of the first two editions of Copernicus's *De revolutionibus*. The result is a detailed description of 277 copies of the first edition and 324 of the second, some in private hands but most of them deposited in public collections. The reader might well ask what useful information could possibly justify such an enormous effort. The answer, which may come as a surprise, is that many copies are heavily annotated by distinguished scholars of the sixteenth century (and later), and some of these annotations appear in multiple copies. In effect, annotations were a form of scientific communication in the age between the invention of printing and the introduction of the scientific journal in the latter half of the seventeenth century. The most significant families of annotations stem from Erasmus Reinhold (d. 1553), a leading astronomer at Wittenberg in the generation after Copernicus (d. 1543); and from Paul Wittich of Wrocław (d. 1586), an itinerant scholar whose annotations are closely linked to those of Reinhold. The copy owned by Michael Maestlin (d. 1631), Kepler's teacher in Tübingen, is worthy of special notice, for his extensive annotations were written over a long period of time. Moreover, the copy owned by Kepler has many points of interest, including annotations by a previous owner, Jerome Schreiber of Nuremberg (d. 1547) which called attention to several important passages in Copernicus's *magnum opus*.

A subject that has generated much discussion, beginning at the time of the publication of the first edition of *De revolutionibus*, is the authorship of the anonymous preface. It was realized by many early readers that the preface expresses a very different view of astronomical models

(or 'hypotheses') than does Copernicus. Kepler's *Astronomia nova* (Heidelberg, 1609) (on reverse title page, and see the translation by William H. Donahue (Cambridge, 1992), p. 28) was the first to identify in print the anonymous author as Andreas Osiander (d. 1552), a leading Protestant theologian in Nuremberg and a contributor to several works published by Johannes Petreius, the printer of *De revolutionibus* in 1543. Osiander has often been vilified for what is taken to be an unauthorized intrusion into the work of a master but, as we shall see, the story is a bit more complicated. In this context the annotations have proved to be most useful. Maestlin's early annotations indicate that in the 1570s he did not yet know who the anonymous author was (Gingerich, p. 220); on the other hand, the annotations by Schreiber in Kepler's copy clearly indicate that Osiander was the author in question (p. 77) and this is the source explicitly cited by Kepler. But Kepler had another source of information which he mentions in a work that was not published until the nineteenth century, *A Defence of Tycho against Ursus* (written *ca*. 1600). This work is now available in Latin together with an English translation and notes in Nicholas Jardine's *The Birth of History and Philosophy of Science* (Cambridge, 1984). Kepler reports that he personally inspected Osiander's correspondence and quotes from a letter written by Osiander to Copernicus in 1541 in which we find some of the same language as in the anonymous preface. In the letter Osiander says, 'I have always been of the opinion that hypotheses are not articles of faith, but bases for computation, so that even if they are false it does not matter provided they yield the phenomena of motion exactly … So it would seem to be a good idea for you to say something on this matter in the preface' (Jardine, p. 152). There is no evidence that Copernicus responded, and Osiander may have misunderstood this silence as assent. It is hardly surprising that a Protestant theologian would not wish his name to be associated with a book dedicated to the Pope, and it is equally clear that a Catholic canon would not be pleased with a visible Protestant hand in the presentation of his *magnum opus*. It is also noteworthy that nowhere in his book does Copernicus mention the role of Georg Joachim Rheticus (d. 1574), a young Protestant astronomer who visited him in Frombork (Poland) for an extensive period of time and who published the first account of Copernicus's theory in the *Narratio prima* (Gdansk, 1540). Indeed, without the assistance of Rheticus, Copernicus's book would probably not have been published at all.

Although some of the information collected in this book has appeared previously, we now have a reliable and thorough investigation all in one volume on the fate of the early copies of this classic in the history of science and Owen Gingerich is to be congratulated for his achievement.

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Ladina Bezzola Lambert, **Imagining the Unimaginable: The Poetics of Early Modern Astronomy**. Internationale Forschungen zur allgemeinen und vergleichenden Literaturwissenschaft, 58. Amsterdam and New York: Rodopi, 2002. Pp. ix + 182. ISBN 90-420-1578-0. $34.50, €37.00 (paperback).
DOI: 10.1017/S0007087404246684

This work is an interesting examination of Copernicanism as a novel stimulus to visual imagination during the seventeenth century. A planetary Earth challenged Aristotelo-Christian beliefs concerning material distinctions between the Earth and the heavens, the spiritual character of the heavens, and the finite nature of a unique universe. In the early modern period an infinite universe and a plurality of inhabited worlds began to appear as themes in a number of works, both scientific and literary.

Lambert sees the use of metaphor as the means employed by the imagination, through its relationship to perception, thought and memory, as crucial to the creative process. After tracing theories of the nature of imagination from Plato to modern times, she continues with an analysis of the use of metaphor in Ariosto's *Orlando furioso*. Its protagonist travels to the Moon and finds

what he sees there as both complementary to the Earth and analogous to it. His experience on the Moon is capable of being woven into an infinite number of accounts, although the natural limits of imagination compel him to find analogies with familiar aspects of the Earth. Galileo was very fond of Ariosto's poem, as was Italo Calvino, whose novels are analysed in the concluding chapter of this work. For Calvino, no abstraction constructed by our imagination is an end in itself. The infinite number of complexities constituting the universe, the tangle of relationships among its components, and the innumerable ways they can be conceived, set limits to imagination and the use of words to render them with absolute certainty. Lambert's thesis is that the limitations of imagination and its relationship to creativity were similar in both literature and science in the early modern period.

The main part of her text is devoted to examples from the works of Galileo, Kepler and others, all of whom employed imagery familiar to their readers, and were concerned to reach a wider audience through the use of contemporary literary modes. After Galileo's discoveries with the telescope, the universe was no longer seen as a unity. The moons of Jupiter served to question the Earth's uniqueness, and the multitude of stars suggested an infinite universe. Galileo's *Sidereus nuncius* sought to have the visual image represent a physical world, to show Earth and Moon as similar, and to make telescopic images seem familiar. Although he observed many craters, Galileo initially chose to show only one, larger than any he had actually seen. The metaphor of the dead lunar face was altered to something undergoing change in the course of the Moon's motion as light passed over its dark spots. Building on the details of telescopic observation, Kepler's *Somnium* describes the imagined experience of a trip to the Moon, its astronomy and topography, and the lives of its inhabitants. Although many of the real Moon's characteristics are beyond imagination, Kepler's highly detailed account, a blend of his reading, memories and imagination, are a metaphor for the Earth's Copernican attributes.

In the latter part of the seventeenth century, several authors suggested that fixed stars are suns with their own planets similar to the Earth. Among the justifications offered were the uniformity of nature, the principle of plenitude and that the existence of other planets must have a purpose. Huygens's *Cosmotheoros* proposed that stars unseen by us would be visible to creatures of other planets. The inhabitants of other planets must be like us or close to it, since God does things in the best manner.

For Fontenelle, however, the principle of plenitude requires that there be diversity, but the details of it are limited by imagination's constraints. In Fontenelle's *Entretiens* the world is a theatre in which backstage arrangements are the means determining what is seen, and the laws of physics provide a foundation for the imagination. He sees Earth at the centre of the world as a transgression of the social order, and analogous to the Fall from the Garden of Eden in efforts to seek omniscience. The Sun at the centre, however, is analogous to the king's divine right to rule. Fontenelle also uses the microscopic world as analogy with the visible one with respect to the size of its organisms and the nature of their environment. He shifts the established macro–microcosm concept to one with the human as macro and the microscopic as micro. Fontenelle was aware that visual imagination cannot determine the reality behind the metaphor. The real world must be pursued in other ways.

Lambert's assertion that 'Galileo's discoveries … offered empirical evidence for Copernicus's cosmology' (p. 141) requires a brief comment. Empirical evidence was provided against Aristotelo-Ptolemaic cosmology, considerably weakening it and thereby lending force to Copernicanism, but empirical evidence for the Earth's motion was reserved for the future. Despite this, Lambert's work is rich in detail and provides a very useful supplement to recent explorations of the novel use of rhetoric and imagination during the Scientific Revolution.

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RICHARD S. BROOKS and DAVID K. HIMROD, **Science and Religion in the English-Speaking World, 1600–1727: A Bibliographic Guide to the Secondary Literature**. American Theological Library Association Bibliography Series, 46. Lanham, MD and London: Scarecrow Press, 2001. Pp. xxxiv + 620. ISBN 0-8018-4011-1. $85.00 (hardback).
DOI: 10.1017/S0007087404256680

This is a curious bibliography. The subject is the historical relationship of science and religion – for practical purposes, science and Christianity – but with a set of unusual and significant limitations. Coverage is limited to publications concerned with this relationship over a period of 128 years (ending with the year of Newton's death). Subject matter is restricted to the English-speaking world – nothing here on Copernicus or Galileo or Descartes. The language in which the included books and articles are written is mainly English – among the first four hundred items, I counted only eighteen not in English (these eighteen in French, German or Italian). To be included an item must have been published no later than 1994. Finally, the bibliography contains exactly two thousand items, and the authors have read every one of them.

The authors' original purpose was to extend coverage to 1750, to complete this work in a few years, and then to proceed to later periods, but events intervened to reduce the scope of the project. Each item is annotated, usually in thirty to sixty words, but some get as many as a hundred words, and a few are in the two-hundred-word range. The authors reveal themselves to be careful and perceptive readers, whose purpose is to reveal the content of the book or article rather than to affirm or criticize its conclusions. Although they admit to selectivity on grounds of insufficient relevance, it appears to me that their reach was quite wide, as I find entries for which neither the title nor the annotation suggests religious relevance. For example, I wonder about the inclusion of a book (0145) on Newton as a theologian (written in German), for which the annotation reads as follows: 'Even in 1965, this book added nothing that was not already written in English. It relies on now outdated secondary sources'. And I cannot help but wonder about the relevance of an article (item 0525) on phalarism (inhuman cruelty), annotated as follows: '[The author] discusses the episode in the 1690's of the ancients versus moderns controversy'. But these are quibbles. All of the major, important sources seem to be present, accompanied by fair and informative annotations.

But there is more. All items are classified by topic, of which there are twelve: historiography; the magical, alchemical and *prisca* traditions; Protestantism and the rise of modern science; Christianity, social ideals, ideology and science; social institutions, science and Christianity; religion, technology, architecture and the environment; theology, philosophy and science; natural theology and natural philosophy; heretical Christianity, deism and atheism; science, the Bible and literature; religion and medicine; and Newtonian studies. The bibliography concludes with a topical index, an index of persons treated, and an author index.

This bibliography will not, of course, be of much use to scholars interested in continental developments, in science–religion encounters that occurred before 1600 or after 1727, or in scholarship published in the past decade. But given the task the authors set for themselves, one must be astonished at their energy and dedication. For scholars focused on developments that fall within the boundaries chosen by the authors, this volume should prove not only useful for bibliographical purposes but also a model of bibliographical dedication.

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AMIR R. ALEXANDER, **Geometrical Landscapes: The Voyages of Discovery and the Transformation of Mathematical Practice**. Writing Science. Stanford: Stanford University Press, 2002. Pp. xvii + 293. ISBN 0-80473-260-4. £46.95 (hardback).
DOI: 10.1017/S0007087404266687

In recent years historians of mathematics have moved increasingly towards studying the development of mathematics in its cultural context. Amir Alexander in *Geometrical Landscapes* goes farther than most, arguing that the early voyages of exploration influenced not only the kind of mathematics that was done, but also the nature of mathematical thought itself. He argues that sixteenth-century seafarers created a new mythology, the 'standard narrative of exploration', in which lands containing wondrous riches were protected by natural barriers through which the traveller must pass. The coastal maps of the period depict a maze of straits and islands protecting the treasures within, and thus, he argues, they were not just representations of a landscape but ideological constructions, visual images of the tale of discovery and conquest. This in turn, Alexander claims, influenced mathematicians to study matter, and even mathematical objects, as entities that could be broken down into small discrete parts, allowing the explorer to enter and conquer.

Alexander regards his exploration narrative as 'new', overlooking the fact that the vision of a promised land and the struggle to enter it is one of the founding mythologies of Western civilization, which perhaps explains, as he does not, why the myth was so readily accepted. Certainly, recognition of the importance of myth in shaping human experiences and intentions is a useful contribution to historical analysis. When he comes to discuss the relationship of the myth to mathematics, however, Alexander's thesis becomes weak, mainly because his knowledge of the mathematics of the period is sketchy. It is true that the voyages of geographical discovery became a natural metaphor for intellectual discovery, but metaphors need to be read with care. When Oughtred spoke of guiding his readers through a labyrinth, it was to lead them into not a new world but an old one, the world of Euclid and Apollonius. Oughtred, like so many of his contemporaries, including Harriot, was intensely concerned with the recovery of Classical mathematics, and it could be argued that it was this concern, rather than any new image of exploration, that drove contemporary mathematics forward.

Two chapters are devoted to the work of Thomas Harriot. Alexander's theme is perhaps best argued in his section on Harriot's optics, where he likens Harriot's image of light passing through the narrow spaces between atoms to the boats of the explorers working their way through the islands and sandbanks off the coast of Virginia. It becomes forced, however, in his discussion of Harriot's treatment of the 'continuum'. To suggest, as Alexander does, that Harriot was able to 'master the continuum and extract its secrets' (p. 169) is to place him almost four centuries ahead of his time. Such adulation has done Harriot no good in the past and does him no good now. Alexander makes much of a single phrase from Harriot's manuscripts: 'The truth when it is seen is knowne without other evidence' (p. 98). As he admits, philosophical discussion in Harriot's surviving manuscripts is very rare, and this isolated remark has no obvious context, but from it Alexander deduces for Harriot an entire world view. He claims, for example, that Harriot put personal experience above authority or logic, failing to observe how closely Harriot studied and adhered to Greek standards of argument. Where 'seeing the truth' was important for Harriot was in his mathematical exposition; through his use of space on the page and transparent symbolism, Harriot repeatedly presents his mathematics in a way that makes the underlying structure, whether of a mathematical object (like an equation) or of an entire argument, clear to the eye. To read more into Harriot's remark than this is to read more than can be justified.

Sweeping statements about mathematics and its history unfortunately mar this book throughout. Alexander claims, for example, that, in contrast to Harriot, Cavalieri ignored the difficulties of the infinite and that Wallis 'emphasized a loose, nonrigorous approach' (p. 136–7). The truth

is that Cavalieri made repeated efforts to ground his theory securely, while Wallis in his own mind was convinced that he had produced a sound and reliable method. Alexander also clings to a strange idea, apparently based on a single sentence from Clavius, that before the early modern period mathematics was based only on logical deduction from postulates, and he seems quite unable to comprehend the subtle and changing dynamic between logic and intuition that has pervaded mathematical thought throughout history. Thus he ends with the absurd and insupportable claim that by the end of the seventeenth century calculus had emerged as a logically coherent subject and brought to an end the era of mathematical exploration. At this point, if not long before, one cannot help feeling that Alexander has become as spellbound by his own 'exploration narrative' as, he claims, were the seventeenth-century practitioners themselves.

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L. W. B. BROCKLISS, **Calvet's Web: Enlightenment and the Republic of Letters in Eighteenth-Century France**. Oxford: Oxford University Press, 2002. Pp. xx + 471. ISBN 0-19-924748-X. £55.00 (hardback).
DOI: 10.1017/S0007087404276683

This is a deceptively simple-looking book. Lawrence Brockliss's study of the eighteenth-century Avignon *érudit*, Esprit-Claude François Calvet, is organized around his subject's areas of scholarly interest. In successive chapters Brockliss reconstructs the worlds of the eighteenth-century Provençal physician, antiquarian, naturalist and bibliophile. Each of these chapters painstakingly reconstructs the network of contacts and correspondents through which Calvet pursued his interests. In sharp contrast to trends in historical writing he takes pains not to assert the hithertofore unnoticed centrality of his subject to the intellectual history of these fields. Instead he insists that Calvet operated in a self-consciously provincial set of networks, that he avoided publication and that he was suspicious of every kind of intellectual novelty. Calvet was irredeemably obscure and his greatest honour, election to the Académie des inscriptions in 1763, was a reward for being the Comte de Caylus's antiquity collection agent in Provence rather than for any scholarly contribution of his own. In possibly the best symbol of Calvet's spear-carrying, in 1758 he surrendered his collection of inscriptions to Jean-François Séguier, who had just reconstructed the inscription of the Maison carré in Nîmes, because he was so obviously more talented. Thus began a twenty-five year relationship of subordination. Brockliss insists that Calvet made no innovations or any discoveries. Few subjects of historical biography can have been so consistently self-effacing.

Brockliss uses the very humility and obscurity of his subject to advance an interesting claim. He inserts himself in a running debate on the relationship between the Republic of Letters, that self-conscious identity for the learned adopted in the seventeenth century, and the Enlightenment. Brockliss argues that confusion abounds on this topic, but that there is an almost unexamined assumption that the *philosophes* were a distinct intellectual movement, synthesizing scepticism and Newtonianism to generate *critique* as a form of cultural life. The life of the Republic of Letters, on the other hand, is understood to have been a socially and politically inert form of scholarship. He argues that this question is irresolvable because of the lack of empirical work on what members of the Republic of Letters actually did. His subject, Calvet, stood at the opposite pole from the ideal of the *philosophe* and so is a tremendous test case to understand the cultural work of the provincial *érudit*. On this reading Calvet's lack of distinction makes him more than a representative figure; he marks the limit of the Republic of Letters.

Brockliss concludes that the argument that the Republic of Letters and the Enlightenment were distinct social movements cannot be supported. He argues that even the most conservative, obscure member of the Republic of Letters, like Calvet, acquired the meritocratic, critical, progressive

values that we associate with the radical Enlightenment. Men like Calvet deplored the Revolution, because it destroyed the institutions, like universities and academies, that had structured their lives and ambitions. However, the revolutionary regime was eventually driven to use the expertise of local scholars to staff schools, administrations and eventually the new scientific societies. Just when it might have been most politically relevant, no distinction was drawn between *philosophe* and *savant*. In the final analysis, he argues, there was no difference between Enlightenment and Republic of Letters because there was no Enlightenment. Brockliss seeks to use his forgotten man to pose a challenge to those historians who try to find the genealogy of modernity in the cultural life of the eighteenth century. We must look elsewhere for the origins of our cultural concerns.

One might legitimately wonder if Esprit-Claude François Calvet was too lightweight a figure to bear the kind of load being placed upon him. The idea that the most obscure member of a world reveals its nature is fascinating and creative, but the problem of representivity will not go away. Take, for example, Calvet's correspondence network in the Midi. At first glance he seems very well connected to the major cultural figures of the region. However, it emerges that he had no correspondents in Montpellier, which is extraordinary since it was the centre of medical life and Calvet was a doctor. Calvet in fact acquired his degree from the far less prestigious faculty at Avignon and had only spent one year studying in Montpellier. Therefore he had not experienced the debates on vitalism or the struggles between the pro- and anti-Linnaeans that animated Languedocian intellectual life. In consequence Calvet had an antiquated idea of natural science and had only the most tenuous connection with the significant plant collectors centred on the botanical garden in Montpellier. Even in his own region Calvet was not only obscure, he was anachronistic.

Calvet certainly represented one way to be a provincial member of the Republic of Letters, but it was not the only way. His quiescent submission to every kind of authority – social, political or intellectual – seems more idiosyncratic than representative. Brockliss's polarity of metropolitan Enlightenment to continuous development and expansion of the Republic of Letters extrapolates too readily from Calvet's example. A Séguier or an Antoine Gouan, both thoroughly provincial, changed the nature of the Republic of Letters in the Midi, precisely because they found various kinds of authority to be incompatible with one another. Even in the provinces living in the world of the learned could and did raise disquieting questions. At the conclusion the reader is left with the suspicion that Calvet's web is analogous to a modern citation circle. The price of tranquillity and security was and remains isolation from the living currents of the epoch.

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Barbara T. Gates (ed.), **In Nature's Name: An Anthology of Women's Writing and Illustration, 1780–1930**. Chicago and London: University of Chicago Press, 2002. Pp. xxvi + 673. ISBN 0-226-28446-8. £17.50, $27.50 (paperback).
DOI: 10.1017/S000708740428668X

Intended as a companion text to *Kindred Nature: Victorian and Edwardian Women Embrace the Living World* (Chicago, 1998), Barbara T. Gates's *In Nature's Name* provides a wealth of primary resource material covering numerous literary as well as artistic genres. This anthology is clearly meant to form a multidisciplinary teaching aid. It ranges widely across different narrative forms, from complex technical papers to the more straightforward children's stories, and from the safety of domestic gardening to the perils of autobiographical adventure narratives. The editorial material is extensive and useful. Critical introductions to each section and biographical sketches position the extracts firmly within the author's (sometimes familiar, sometimes obscure) life and career, while a chronology places the texts culturally and historically alongside other literary and

artistic works and events of significance to British history and politics, to women's history and feminist politics and to the history of science, technology and medicine.

Structurally, the anthology is organized in seven major sections: 'Speaking out', 'Protecting', 'Domesticating', 'Adventuring', 'Appreciating', 'Popularizing science' and 'Amateurs or professionals?'. As the layout reveals, Gates offers a very welcome balance between the more domestic manifestations of interest in natural history and the public pursuit of science. *In Nature's Name* thus provides a selection of writings not only from authors who wrote popular works for specific audiences, but, intriguingly, from those who made adventurous forays into more esoteric realms (both geographically and scientifically). The extracts from the latter texts provide especially exciting opportunities for further critical examination of women's place in the history of science.

Alongside the well-known writings of popularizers like Arabella Buckley and Jane Marcet or the poetic appreciations of Emily Brontë and Christina Rossetti, Gates has included more obscure material. The most fascinating extract from the vitally important 'Speaking out' section is a speech, 'The physical and intellectual capacities of women equal to those of men', made in 1874 at the Anthropological Society by Emma Wallington, about whom Gates has been unable to find any information. Wallington's paper is a confident and vigorous defence of women's participation in the various sciences, illustrated by historical examples of successful female scientific practitioners. Following Wallington's impassioned speech, Gates reprints the original male-only 'Discussion', which painfully but also unintentionally and amusingly illustrates Wallington's point about society's treatment of the female intellect. A Mr Grazebrook suggests that for every remarkable woman, he could substitute fifty remarkable men, while Mr Churchill remarks that even Mary Somerville could only follow where men had led, and the President of the Society concludes with a comment that women simply do not possess the faculty of invention necessary to succeed in science. It is perhaps a Mr Lewis who, however, draws the clearest distinction between male and female intellect when he comments that 'to employ a woman to do a man's work was like using a chisel instead of a screwdriver, a practice, by the way, to which ladies were addicted' (p. 44).

It is precisely in this female stubbornness in forcing square pegs into round holes that this anthology excels. Alongside the caring and sensitive attitude towards the defenceless betrayed by the female anti-vivisectionists such as Frances Power Cobbe, Louisa Lind-af-Hageby and Liese Schartau, Gates presents extracts from the female exponents of the ultra-masculine huntin', shootin' and fishin' school – all in the interests of scientific discovery, of course. Mary Kingsley's rumbustious style when writing about fishing in West Africa (1897) is a delight, as is Isabel Savory's barely disguised excitement at her successful tiger hunting in India (1900). Even old favourites of vastly different women's writing such as Marie Stopes or Beatrix Potter are presented here in their other (and original) guises. Stopes appears in this anthology both as the author of *Married Love* (1918) and as a palaeobiologist, with a doctorate, the author of such papers as 'The "xerophytic" character of the gymnosperms'. Although known almost exclusively as a writer and illustrator of children's tales, Potter appears in this anthology as an experimental scientist – a position she always desired to attain – and which the Linnean Society, as Gates illustrates, has over a century later posthumously conferred upon her. While the scientific paper she wrote on fungi has been lost, her essay on 'Hedgehogs' reveals a more scrupulously scientific side to the creator of Mrs Tiggywinkle.

In Nature's Name is more than just a teaching aid. It is an enticing, often touching, collection of writings which reveal the pleasure taken not only by the editor in selecting the extracts, but by the women who were closely involved in and devoted to the development of science from the late eighteenth to the early twentieth century.

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RICHARD KEYNES, **Fossils, Finches and Fuegians: Charles Darwin's Adventures and Discoveries on the *Beagle*, 1832–1836**. London: HarperCollins, 2002. Pp. xix + 428. ISBN 0-00-710189-9. £25.00 (hardback).
DOI: 10.1017/S0007087404296686

Keynes's account of the already well-known *Beagle* voyage may not be strongly analytical and it may not present the journey in the broad cultural, social or even scientific context of European expeditions. However, if the reader can set aside its celebration of Darwin as a 'pioneer' or 'founding father' of particular approaches, this cleanly and accurately written book is great fun for the armchair traveller and scholar, and should be of keen interest to historians of natural history. Keynes also adds some fresh material to earlier descriptions of the famous nineteenth-century voyage, and he offers a valuable presentation of the ways in which Darwin's long excursion influenced his transmutation theory.

A great-grandson of Charles Darwin, Keynes has edited volumes that well qualify him for writing this book. These include his recent transcription of Darwin's zoology notes and specimen lists from the voyage (2000), his edition of Darwin's *Beagle* diary (1988) and his large edition of pictorial and textual selections from the voyage (1978). He incorporates bits of all these, including Darwin's drawings of specimens, shipmates' drawings and stunning plates of paintings, not often seen, by the *Beagle* artist Conrad Martens; these are all well keyed in to the text, as are the useful maps. Along with Darwin's publications and correspondence, Keynes also inserts observations from shipmates' memoirs and letters which often contrast interestingly, and sometimes conflict, with those of the *Beagle*'s well-liked 'Philosopher'.

The now almost iconic observations (including those indicated in the title, of Darwin's once misrepresented finches – and mockingbirds), the South American fossils and the native people of Tierra del Fuego are conveyed thoughtfully and are sometimes nicely clarified. However, Keynes's familiarity with Darwin's zoology, and his own career as a physiologist, result in new descriptions of Darwin's lesser-known fascination with invertebrates on the voyage, including, for example, planaria, bryozoans and their movements, terrestrial flatworms or spiders. Darwin's examination of the many forms of life making their home in the kelp beds off the Falkland Islands presents an image not usually portrayed of the naturalist studying interconnections within a distinctive environment. But even with this insight into Darwin as a zoologist, Keynes does not fail to demonstrate that Darwin's primary preoccupations during the voyage were with geology and palaeontology. In the closely chronological account, Darwin's mounting evidence suggesting movements of the Earth's surface is clearly depicted. With his discoveries of sea shells at different elevations, his observations of coastal and river terraces, as well as his experience of a major earthquake, he wrote the following: 'Daily it is forced home on the mind of the geologist that nothing, not even the wind that blows, is so unstable as the level of the crust of this earth' (p. 281).

Impressions of the Fuegian people, written by Darwin, Robert Fitzroy and others, are compared, as are the equally anglocentric and often uninformed observations of native people of Argentina, Chile, Tahiti, New Zealand and Australia. Keynes often strays comfortably from Darwin's voyage itself to histories of a particular area or circumstance; in the case of Tierra del Fuego, he includes the story of Captain Fitzroy earlier transporting 'Jemmy Button' and three other Yahgans to England, as well as the 1859 murder of those on a ship of the Patagonian Mission Society. Keynes makes it clear that Darwin's unwavering curiosity extended to all people encountered throughout the voyage; these are absorbing inclusions, and Darwin's comments on indigenous people and Spanish and British colonists include references to class and manners, easily exposing him as the Victorian middle-class gentleman that he was.

The epilogue effectively draws together and summarizes Darwin's observations and thoughts during and after the voyage, tracing their possible contribution to his developing species theory.

References throughout the book to Darwin's religious faith during the voyage, based largely on the terms 'centres of creation' and 'the creator' appearing in his notes and journal, are less convincing. Darwin also used 'the creator' in the *Origin of Species* (1859) after his agnosticism had asserted itself; though he explained in an 1863 letter to Joseph Dalton Hooker that he had long regretted that he 'truckled to public opinion & used Pentateuchal term of creation', he did retain the term in later editions. Tracing precisely the initial extent and subsequent waning of Darwin's belief in an ordained creator or in natural design is not a task that is so straightforward, or perhaps even possible.

Keynes does best with more concrete elements of the story, especially when supplementing his nineteenth-century narrative with modern information, some of which tells us more about the geological past with which Darwin himself was so concerned. When describing his notable fossil finds of *Glyptodon* at Monte Hermosa, Argentina, Keynes provides photographs illustrating footprints, exposed by recent erosion, of *Megatherium*, *Glyptodon*, a palaeollama and other extinct animals. The footprints (now known to be sixteen thousand years old) beautifully demonstrate Darwin's conclusion that the animals, so similar to extant species, had lived fairly recently; Darwin's assessment further supported the young naturalist's growing and vital conviction that organic forms had not been immutable.

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Sofia Åkerberg, **Knowledge and Pleasure at Regent's Park: The Gardens of the Zoological Society of London during the Nineteenth Century**. Idéhistoriska skrifter, 36. Umeå: Department of Historical Studies, 2001. Pp. 254. ISBN 91-7305-147-0. No price given (paperback).
DOI: 10.1017/S0007087404306680

Founded by the East India Company official and collector Sir Stamford Raffles, the Gardens of the Zoological Society opened in Regent's Park in 1828. Supported in part by a landed elite with an interest in acclimatization, particularly of game animals, the gardens were also an expression of national pride for Peelite supporters, an institution to rival the Jardin des plantes in Paris. Struggling financially for many years, the gardens admitted a larger, fee-paying public by mid-century, and became a central London attraction for much of the second half-century. This study situates the gardens within the shows of London, the movements of rational recreation and sanitary reform, Victorian leisure culture and, finally, 'serious science'. The author draws on a wide range of sources to document its period of foundation, its day-to-day running, its representation in popular printed media, and its use by scientific practitioners and various publics.

The book is a Ph.D. thesis, apparently unrevised, and although highly readable it bears some of the stylistic and organizational marks of a dissertation. Several chapters are based on a single kind of source material (for example, guide books). Lengthy passages, quoted by way of illustration, and a substantial amount of original research, evident in the endnotes and bibliography, remain largely undigested, leaving the reader at the surface of issues of considerable interest. Some of the work's best insights are made in passing, while a number of important questions are raised in the Epilogue, such as the role of the picturesque tradition in the design of the gardens, and the status of the zoo animal in the history of biology, as exemplified in the intimate relations between the garden's collections and Richard Owen's comparative anatomy.

Critical of Harriet Rivto's exclusive attention to animals in the Victorian period as emblems of social power, the author's approach to rational recreation and amusement is largely unconcerned with the politics of natural historical learning and display. Perhaps more limiting is the book's persistent separation, both analytically and historically, of knowledge and pleasure. Thus a gleaning from the Society's *Proceedings* turns up little of what the author calls 'professional

hard-core science' (p. 196). While the gardens played the role of dissection warehouse for anatomically inclined Fellows, little interest was shown, according to the author, in field studies due to a general perception of the zoo as an inauthentic habitat. References to zoo animals in the work of Frank Buckland, William Youatt and Darwin are briefly mentioned. But the importance of such animals for behavioural studies, comparative osteologies and physiologies, and even statistical enquiries, is underestimated. The individual, 'anthropomorphized' animal, and the zoological anecdote, held central place in natural history for much of the century. If the zoo was not regarded as a natural setting, it was nevertheless a crucial site for exploring the processes of domestication and civilization, and the relations between animal and human culture.

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JOHN THACKRAY and BOB PRESS, **The Natural History Museum: Nature's Treasurehouse**. London: Natural History Museum, 2001. Pp. 144. ISBN 0-565-09164-6. £11.00 (paperback).
DOI: 10.1017/S0007087404316687

Peter Whitehead and Colin Keates ended their eponymous overview of *The British Museum (Natural History)* (London, 1981) with the closing of the Museum at the end of a day: 'Outside, its terracotta glowing in the floodlights, the façade looks every inch a storehouse for the "Wonders of Creation"' (p. 125). Perhaps fittingly for a zoologist author, this effectively celebrated both the form and function of the Natural History Museum. Appearing twenty years later, this small book by Thackray and Press is perhaps best seen as a successor to Whitehead and Keates. Whereas the latter focused principally upon the Museum as the institutional base for the systematic study of taxonomy, Thackray and Press's book is a more direct celebration of the history of the Natural History Museum.

Beautifully produced, this book renders history equivalent to the act of wandering through the splendours of a magnificent museum. Consequently, the central narrative of the history of the museum is punctuated by brief vignettes of selected specimens from the collections (Thomas Hawkins's sea dragons, paintings commissioned by John Reeves, *Archaeopteryx*), or of architectural details (the ceiling of the Central Hall, the Spirit Building, the Central Hall statues). With the text printed in sepia tone, these vignettes are like galleries jutting off from the Central Hall of the narrative, printed in black. And like a visitor to the museum, the reader realizes that this particular visit must be selective. After all, even prior to further additions, the Natural History Museum constituted approximately one mile of wall space and four acres of flooring; it now houses seventy million specimens of animals, plants, minerals, rocks and fossils.

Appointed in 1912, John Leonard became the museum's first guide lecturer. According to Thackray and Press, 'He seems to have had the knack of giving just the right amount of information for his particular group of visitors, and giving it in such a way as to make natural history seem the most fascinating subject in the world' (p. 103). This book follows in Leonard's tradition. It is meant to be more evocative than definitive. Nevertheless, it provides a well-informed account of the museum's history, from Sir Hans Sloane to the digital age. As the driving force behind the creation of a separate museum for natural history, Richard Owen deservedly receives a more favourable treatment in this account than is often his lot in the history of science. One of the final images of the book is a photograph taken from high in the ceiling of the Central Hall: looking down to the landing of the main staircase, you can see the solitary statue of Richard Owen.

John Thackray began this book before his untimely death in 1999. According to Bob Press, who completed the task, John sought to convey the 'excitement' of the museum and its history. Although the book never loses its critical historical gaze, it is clearly an ebullient celebration of an institution where John Thackray worked for thirty years of his life. As a brief history of the

Natural History Museum, this book is an admirable achievement; as a posthumously published evocation of the wonders of an institution, it is a remarkable book.

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Marc Rothenberg, Kathleen W. Dorman and Frank R. Millikan (eds.), **The Papers of Joseph Henry. Volume 9. The Smithsonian Years, January 1854–December 1857**. With the assistance of Deborah Y. Jeffries. Canton, MA: Smithsonian Institution/Science History Publications, 2002. Pp. l + 516. ISBN 1-88135-363-9. $79.95 (hardback).
DOI: 10.1017/S0007087404326683

It is now thirty-two years since the first volume of Henry's papers was published. In the intervening years they have become a key resource for understanding the development of science not only in the United States but also elsewhere and particularly in Britain because of the close contacts Henry maintained there. The volumes have traced Henry's active career in teaching and scientific research (especially in electricity) from his beginnings in Albany through to his time at Princeton. Then in 1846 Henry abandoned both teaching and research to become an administrator when he was appointed the first secretary of the newly established Smithsonian Institution.

The early battles that Henry had to fight in order to shape the Smithsonian into the sort of institution he wanted and to determine the place it would occupy in American scientific and intellectual life were recounted in the previous two volumes. This ninth volume, which covers the middle four years of the 1850s, also charts another sustained period of struggle for Henry as he sought to maintain the Smithsonian in his chosen form. As with the earlier period, the argument centred round whether the Smithsonian should be the national library of the United States (which Henry was against) or whether it should pursue research (as he wished). Even research caused difficulty as Lorin Blodget, who coordinated the large number of meteorological observations made on behalf of the Smithsonian, did not, in Henry's view, sufficiently acknowledge the Smithsonian's role. Henry eventually sacked him by locking his office door and taking away the key. So far as its status as library was concerned, this was supported by the assistant secretary of the Smithsonian, Charles Jewett, and questions of who had authority over him thus also came into play. At a meeting on 8 July 1854, the Regents of the Smithsonian supported Henry both in his view of the library and also in his view that he had sole right to dismiss employees – and Jewett was dismissed two days later. The issue of the Smithsonian was discussed in both houses of Congress; Henry was vindicated and by the spring of 1855 he had firmly stamped his authority on the Smithsonian.

What is very curious is the well-known fact that Henry then accepted the collection of objects that had been acquired by the US Patent Office. It was clear that this would be the first step towards the Smithsonian becoming a museum, which, as with the library, Henry had resisted. No historian has satisfactorily explained Henry's change of mind, and the documents published here do not help much in explaining it. One must remember, however, that many of Henry's papers were destroyed in the 1865 Smithsonian fire.

Other issues treated in this, as usual, meticulously edited volume include Henry's provision of advice to the US Lighthouse Board, on extending the Capitol Building and on coinage. As secretary of the Smithsonian, Henry's views were evidently held in high regard by the various government agencies in Washington. The other major theme covered in this volume is Henry's involvement with the American Association for the Advancement of Science. There Henry and his old friend Alexander Bache sought to retain control of the association against a rising generation who wanted a more democratic organization. One cannot help feeling reading through the letters that Henry was still fighting earlier battles defending professional science from the charlatans, and

failing to recognize that times had changed. Towards the end of the period covered by this volume Henry became an advisor to the Dudley observatory in Albany, which was to result in considerable unhappiness. That, the editors promise, will be covered in the next and penultimate volume.

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GUILLERMO LUSA MONFORTE (ed.), **La creación de la Escuela Industrial Barcelonesa (1851): 1 de octubre de 1851–1 de octubre de 2001**. Documentos de la Escuela de Ingenieros Industriales de Barcelona, 11. Barcelona: Escola Tècnica Superior d'Enginyeria Industrial de Barcelona, Universitat Politècnica de Catalunya, 2001. Pp. 168. ISSN 1137-0238. No price given (hardback).
GUILLERMO LUSA MONFORTE, **Inquietudes y reformas de cambio de siglo: el proyecto de nueva Escuela Industrial (1899–1910)**. Documentos de la Escuela de Ingenieros Industriales de Barcelona, 12. Barcelona: Escola Tècnica Superior d'Enginyeria Industrial de Barcelona, Universitat Politècnica de Catalunya, 2002. Pp. 191. ISSN 1137-0238. No price given (paperback).
GUILLERMO LUSA MONFORTE, **El conflicto con la Diputación (1915). La plena incorporación de la Escuela al Estado (1917)**. Documentos de la Escuela de Ingenieros Industriales de Barcelona, 13. Barcelona: Escola Tècnica Superior d'Enginyeria Industrial de Barcelona, Universitat Politècnica de Catalunya, 2003. Pp. 191. ISSN 1137-0238. No price given (paperback).
DOI: 10.1017/S000708740433668X

For more than a decade, a treasure trove of documents concerning the early history of the Escola Tècnica Superior d'Enginyeria Industrial de Barcelona, now part of the Polytechnic University of Catalonia, has been accumulating in annual volumes published to coincide with the beginning of successive academic years. The volumes have contained manuscripts, printed sources and illustrations from the archives of the school, more recently with helpful introductions. The first ten of them (1991–2000) were devoted to the history of the school from its foundation as the Escuela Industrial Barcelonesa in 1851 until 1899. Volume 11, prepared to mark the institution's 150th anniversary in 2001, completes this cycle. Like other volumes, it offers a rich mix of sources and commentary: Guillermo Lusa Monforte's excellent account of the creation of the school (first published in another valuable publication, the journal *Quaderns d'História de l'Enginyeria* (1996) **1**), a facsimile reprint of the inaugural address of 1851 by the professor of agriculture, Jaime Llansó, an inventory of apparatus and books from 1851, and sixty-eight historic photographs of buildings, laboratories and events.

While the volumes are led by the nature of the documents selected rather than by an overarching interpretative theme, they reveal unambiguously the profound changes that affected Spanish society in the mid- and later nineteenth century. To a liberal bourgeoisie eager for political power and set on modernization, no career embodied the spirit of renewal more profoundly than that of the industrial engineer. And no city offered more hope of a passage from aspiration to realization than Barcelona. In fact, the passage was never easy in a country beset not only by economic problems but also, between 1868 and 1874, by the anarchy of full-blown revolution. Other industrial schools that were founded in Madrid, Seville and Vergara at the same time as the Barcelona school did not survive, and later foundations at Gijón and Valencia also succumbed. From 1867, in fact, the Escuela of Barcelona offered the country's only advanced preparation for technical careers in industry, a state of affairs that lasted until the creation of a similar school in Bilbao in 1899. Its history therefore is in many respects the history of a whole sector of Spanish industrial training at a crucial phase in the introduction of modern production techniques.

Beginning in 2002, the focus of what have been formalized since Volume 6 as the 'Documentos de la Escuela de Ingenieros Industriales de Barcelona' (incorporating the title by which the school has been most commonly known) turned to the twentieth century. In Volume 12, Lusa Monforte signals the new departure with a finely documented account of the school's place in a profound

rethinking of the role and nature of engineering education bred of a congeries of administrative reform (notably the creation of a national Ministry of Public Instruction in 1900), the growing self-consciousness of the engineering profession, continuing political unrest, and the demoralizing aftermath of the loss of Cuba in 1898. Volume 13 pursues the chronological course, with sources and another Introduction by Lusa Monforte concerning one of the recurring points of tension that set the Catalan regional authority (the Diputación, transformed in 1914 into the Mancomunicat de Cataluña) at odds with Madrid and culminated in the integration of the school in the state system in 1917.

The undiminished pace of the 'Documentos' project and the parallel vigour of the *Quaderns* (currently in its sixth volume) deserve warm praise. An apparently inexhaustible supply of source material, high scholarly standards and elegant production have already combined to make the Barcelona school one of the best-documented of all nineteenth-century institutions of technical education, and we can now look forward to an even more extensive coverage of the twentieth century.

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Jed Z. Buchwald and Andrew Warwick (eds.), **Histories of the Electron: The Birth of Microphysics**. Dibner Institute Studies in the History of Science and Technology. Cambridge, MA and London: MIT Press, 2001. Pp. xi + 514. ISBN 0-262-02494-2. £37.95 (hardback).
DOI: 10.1017/S0007087404346686

This impressive volume had its origin in two conferences held in 1997 to mark the centenary of an event that, if one is to believe some of its most persuasive contributors, never really occurred. Most textbook accounts tell us that J. J. Thomson discovered the electron in 1897 while experimenting on cathode rays at the Cavendish Laboratory in Cambridge. In the stream of current flowing through a rarefied gas, Thomson identified negatively charged 'corpuscles' that were, he said, substantially smaller than atoms. When he measured (very roughly) their ratio of charge to mass, he found a value that agrees (again, very roughly) with that now accepted for the electron.

As with many discovery stories, however, these sharp lines blur when we look more closely. The chapters by George Smith, Isobel Falconer, Graeme Gooday and Theodore Arabatzis all show that Thomson's early experimental results were not as clear cut or immediately persuasive as most later accounts suggest. Smith examines the sequence of papers Thomson published in the 1890s and concludes that 1899, rather than 1897, might better stand as the year his 'working hypothesis' about corpuscles solidified into a reasonably strong case for the existence of what we now call electrons. Gooday and Arabatzis go further, asserting that the existence of electrons was not widely accepted until later, and arguing that Thomson's experiments were only one of many threads that physicists eventually wove together to reach this conclusion. Gooday even suggests that to understand why Thomson came to be credited as the sole discoverer of the electron, we should look not so much at the experiments he did in the 1890s as at his former students' efforts in the 1920s and 1930s to bestow a suitable honorific on their old professor.

As in most edited volumes, the contributors here generally say their individual pieces without engaging very directly with one another. Mary Jo Nye and Kostas Gavroglu pass very close to each other in their respective accounts of how the electron was taken up by chemists in the 1920s and 1930s, and Ole Knudsen and Walter Kaiser shed light from different directions on the development of electron gas theory, particularly in the work of O. W. Richardson, but their points of actual contact are small. By far the most sustained and interesting exchange in the volume is that between Arabatzis and the philosopher Peter Achinstein on the seemingly simple question raised in Achinstein's title, 'Who really discovered the electron?' Arabatzis has delved as deeply as anyone in recent years into the early history of the electron, and one might expect him to be

well placed to answer Achinstein's question. As Arabatzis points out, however, even to raise the question presupposes that one has adequate answers to a series of knotty questions concerning our knowledge of objects, such as electrons, that we cannot directly observe. Arabatzis seems happy to leave these questions to philosophers; historians, he says, should leave aside the issue of who 'really' discovered what, and instead focus on what the scientists involved thought about it. When and how did they come to believe in electrons, and what evidence carried the most weight with them? Arabatzis proceeds to give an illuminating account of Pieter Zeeman's work in 1896 and 1897 on the splitting of spectral lines in a magnetic field and shows that Zeeman's experiments – many performed and published before Thomson's – contributed greatly to convincing scientists of the reality of electrons. Arabatzis makes it clear, however, that he is not trying simply to wrest the title of 'discoverer of the electron' away from Thomson so he can bestow it on Zeeman. On the contrary, he says, his aim is to show that 'the electron was not discovered by any particular scientist' (p. 188), and to suggest that the whole category of discrete 'discovery' breaks down when applied to this and similar cases.

Achinstein has little patience for all of this. He rejects what he calls Arabatzis's 'social constructivist' approach, as well as the attributional model of discovery that he regards as its corollary – the view that we ought to accept as the true discoverer of something whomever the relevant scientific community decides deserves the title. Achinstein does not much care when or how the community of physicists formed a consensus about the reality of electrons, or why they eventually settled the title of sole discoverer on Thomson; he instead wants to know who was really the first to produce good evidence that electrons exist and to recognize the implications of that evidence. One gets the impression that Achinstein went into this project expecting that some sharp philosophical analysis would suffice to cut through the social constructivist fog and rescue Thomson's claim to be counted as the discoverer of the electron. Along the way he manages to clarify some important points, but after examining the available historical evidence, he confesses that he now thinks the question of who really discovered the electron is 'complicated, much more so than when I first began to think about it' (p. 420). Even if one accepts Achinstein's straightforwardly realist philosophy, it is not at all clear that Thomson, or anyone else for that matter, deserves sole credit as the discoverer of the electron. Indeed, Achinstein's own criteria seem if anything to point towards giving the palm to Zeeman, or perhaps Philip Lenard or Emil Wiechert, rather than to Thomson.

The attributional model of discovery has many attractions for historians, but we ought to recognize the corrosive effects it could have if broadly applied. Although it enables historians to sidestep thorny and perhaps unanswerable philosophical questions about the nature of reality and our knowledge of it, the attributional model does not resolve those questions – as Arabatzis explicitly acknowledges. Moreover, if scientists themselves were to adopt an attributional view, the whole concept of scientific discovery, and with it the attributional model itself, would go up in smoke. Besides, even historians might like to think that they occasionally make a discovery or two while digging through archives or bending over their books, rather than simply having these attributed to them later by others. Who, for example, was the first to discover that J. J. Thomson did not discover the electron in 1897?

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Harry M. Marks, **The Progress of Experiment: Science and Therapeutic Reform in the United States, 1900–1990**. Cambridge History of Medicine. Cambridge: Cambridge University Press, 2000. Pp. xii + 258. ISBN 0-521-78561-8. £14.95, $19.95 (paperback).
DOI: 10.1017/S0007087404356682

The multi-centre, randomized controlled clinical trial has turned into something like a gold standard in clinical science in recent decades. Most medical commentators assume that, naturally,

a drug or therapeutic practice will not be worth much if its value cannot be demonstrated in this way. The inherent problems associated with the organization of such trials, however, which rely on the cooperation of large numbers of people in different institutions and locations, are rarely discussed in detail, and neither are the historical reasons for their success in spite of such problems. *The Progress of Experiment*, originally published in 1997 and now issued in paperback, deals with the history of such cooperative clinical research in the United States.

Marks traces the origins of the current faith in clinical experiments to the therapeutic reformers of the early twentieth century and their attempts to rationalize medicine by drawing on the laboratory and new statistical methods. The reformers intended to turn medicine into a republic of science, governed by reason rather than the idols of the marketplace. He identifies two main strategies that they embraced to achieve their goals. In the first half of the twentieth century they focused their attention on institutions such as the American Medical Association's Council on Pharmacy and Chemistry (launched in 1905). The plan was to establish effective laboratory controls for the evaluation of claims by manufacturers and researchers. In the 1930s and 1940s officials at the US Food and Drug Administration adopted the Council's approaches to judging the risks and benefits of therapies in their attempts to regulate the safety of new drugs.

After the Second World War, embracing a slightly different strategy, reformers increasingly concentrated their energies on establishing methodological standards, such as new statistical methods and the randomized controlled trial, rather than institutions as a means of securing a rational therapeutics. Marks provides us with a careful analysis of the debates over two iconic studies: the National Heart Institute's Diet Heart Study and the University Group Diabetes Program Study. The Diet Heart Study was intended to show to what extent the American diet was responsible for the notable increase in heart disease in the twentieth century. Due to seemingly insurmountable practical problems as well as differences over both the right scientific tools for the job and the potential consequences of its results, the study never went beyond the preparatory stage. Marks's analysis of the University Group Diabetes Program Study demonstrates that far from solving the controversy over the risks and benefits of oral hypoglycemic drugs it was designed to tackle, the trial merely shifted the focus of the controversy to issues of study design.

The Progress of Experiment is a book rich in empirical detail, all meticulously documented in long footnotes. That said, this book clearly aims at answering important theoretical questions regarding the scientificity of medicine and the often conflicting interests of various groups of historical actors. 'When reasonable people disagree', asks Marks (p. 233), 'where do the boundaries of unreasonable behaviour begin?'. The author applies approaches from the sociology of scientific knowledge to the history of therapeutic research, which seven or so years after the book's original publication date may sound slightly dated to some, but nevertheless remains valid. We cannot be reminded often enough of the political nature of science and the limited power of experiments to resolve controversies. A central argument of the book is that clinical research is intrinsically a social process, 'an activity conducted in a manner similar to politics, by groups of individuals with differing beliefs and interests, who must somehow persuade one another to enter into a temporary and partial alliance' (p. 243). In the light of ongoing attempts in the US and Europe to base medical practice more firmly on scientific evidence (whatever this may mean in detail), this book makes for interesting reading for anybody interested in arriving at a historically based judgement on the potential scope and limits of evidence-based medicine. With its detailed, well-researched case studies it will also provide good course reading. It is laudable that the paperback edition makes this book available to a wider group of potential readers.

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Roy Porter, **Madness: A Brief History**. Oxford: Oxford University Press, 2002. Pp. xii + 241. ISBN 0-19-280266-6. £11.99, $22.00 (hardback).
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To find a book that is similar to the late Roy Porter's *Madness: A Brief History*, one has to go back to Erwin Ackerknecht's *Kurze Geschichte der Psychiatrie*, originally published in 1957. Both were written by great medical historians, and both present very short introductory overviews of their respective subject. Similarities, however, stop here. Ackerknecht's work was the most narrowly conceived history of the psychiatric theories and concepts of great doctors, which is somewhat disappointing when seen as the work of the author of *Medicine at the Paris Hospital* (Baltimore and London, 1967) and the proponent of the behavioural approach to history of medicine. In contrast, Porter's is a typically wide-ranging survey of the cultural and social history of madness, as well as the intellectual history of the medicine that treated the disorder. The striking difference between the two works is an eloquent testimony of the momentous transformation which history of medicine underwent in the last couple of decades. Porter's work is thus an epitome of the dynamic multidisciplinary enterprise which modern history of medicine has become. As such, this book will be the best single volume in an undergraduate introductory course for history of medicine and science. It presents the most compact, readable and brilliant survey of a medical condition, showing new history of medicine at its best.

Indeed, Porter's work might be better entitled 'A Brief History of Western Civilization Seen through the Window of Madness'. Porter keeps big issues in the picture, such as science and religion, mind and body, normal and pathological, and liberty and restraint. Although many of these issues were rarely discussed in depth, one is constantly reminded why history of psychiatry matters. The organization of the book befits this overall goal. After an Introduction that addresses the fundamental question 'what is madness?', Porter proceeds with a mixture of thematic and chronological organization. This somewhat loose structure allows him to move freely across historical periods and geographical boundaries, and to highlight gradual shifts, the survival of age-old problems, the coexistence of different paradigms and the continuing tension of conflicting attitudes. This sense of continuity and complexity is achieved also through a mixture of broad-brush general pictures and well-crafted cameos in narratives of individual cases, excavated by Porter himself and other historians of psychiatry. The familiar stories of Edward Jorden's bewitched girl in sixteenth-century London, George Trosse's auditory hallucinations of religious damnation, James Tilly Matthews's delusions of tortuous factories, and many other gems, are told again. These individual case histories create a sense of continuity through time and demonstrate complex nuances that accompanied each incidence of mental disease at local and private levels. Porter's book is thus an antidote against the scholarly compartmentalization of the history of psychiatry. Despite – or because of – its enormous sophistication in the last couple of decades, evidence-based history of psychiatry now faces the danger of becoming a parochial sub-discipline with myopic concentration on details, losing the intellectual dynamism and relevance prominent at the time of anti-psychiatry, early feminist assault and Michel Foucault. Porter himself has been responsible for reclaiming history of psychiatry from dogmatic theorists, but his history of psychiatry has never exhibited an antipathy against tackling big issues.

As a book which was perhaps written very quickly, like many of Porter's works, this one has many faults. Apart from problems inherent in this kind of undertaking (covering more than two millennia of intellectual, cultural and social history of madness and psychiatry in a small book is no easy task), one major flaw is its treatment of the twentieth century. While Porter integrates his observations culled from medieval England and twentieth-century America, Greek drama and the Enlightenment *philosophes*, he has left the twentieth century on its own. The twentieth century is segregated into a separate chapter, in which Porter too briskly enumerates Kraepelin,

Freud, shock therapies and drug therapies, with little attempt to trace their continuities and discontinuities with developments in previous centuries.

This does not, however, significantly decrease the value of the book. This small book will remain the best entry point for students and teachers of history of medicine for years to come.

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STEVE FULLER, **Thomas Kuhn: A Philosophical History for Our Times**. Chicago and London: University of Chicago Press, 2002. Pp. xvii + 472. ISBN 0-226-26896-9. £14.50, $22.50 (paperback).
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The received view of twentieth-century philosophy of science portrays Kuhn as a revolutionary thinker whose major result was to undermine the philosophical tradition of logical positivism. In the past few years, however, a number of scholars have distanced themselves from this reading, deeming it reductive and, at best, partial; from many, and often fundamental, points of view Kuhn did not manage to break entirely with the preceding philosophical tradition.

By contending that Kuhn held a profoundly conservative view of science and of how one ought to study its history, Steve Fuller adds new material to the controversy. He offers a sociohistorical reconstruction of Kuhn's progress, beginning with his graduate career at Harvard, under the influence of James Bryant Conant. In his capacity as president of the university, Conant had developed an educational programme intended to help deflect the Cold War's unease over the future of science by focusing on its illustrious past. Fuller argues that this rhetoric made its way into *The Structure of Scientific Revolutions* – Conant's General Education in Science programme, in which Kuhn's major work was conceived and elaborated, was an attempt to promulgate noble lies about the autonomy of science and the importance of basic research. And although Kuhn himself was unaware of the sociohistorical factors that were shaping the vision of science he presented in *The Structure of Scientific Revolutions*, he shared the tendency in Western culture to conceal possible negative effects of new knowledge from the general public.

A fundamental premise of Fuller's work here is that 'the overall impact of [the] book has been to dull the critical sensibility of the academy' (p. 7). Addressing the 'paradigm converts who displayed, in most pronounced form, the historical amnesia and political inertia that Kuhn held to be conducive of "normal science"' (p. xvi), Fuller holds Kuhn responsible for the dangerous weakening of the critical attitude which many writers, scientists and philosophers took to be the very engine of scientific research and progress. In Fuller's view, the effect of *The Structure of Scientific Revolutions* was that philosophers of science relinquished their critical attitudes towards science and turned from treating science prescriptively to treating it merely descriptively, thus becoming merely 'underlabourers' (pp. 260–5).

Fuller targets two key features of Kuhn's philosophy – namely his view of the history of science as a developmental sequence that does not lead in any particular direction, and the increased specialization of disciplinary research agendas as the surest historical measure of scientific progress. This, Fuller argues, is a dangerous legacy which threatens the search for a more unified understanding of reality, as understood throughout most of the Western philosophical and scientific tradition. Indeed, such a fragmentation has permeated in structure and content the past quarter-century of philosophy of science, which has exfoliated into philosoph*ies* of 'special' sciences. If, on the one hand, this split has served to curb the excesses of philosophers inclined to dismiss entire fields of enquiry for their failure to live up to the standards of another such field (usually physics), then on the other, Fuller stresses, it has 'removed any public space for discussing the overall ends of science. In content, post-Kuhnian philosophical defenses of scientific progress have been disconnected from any substantive ends that science might be presently pursuing'

(p. 14). By blurring its normative task, Kuhn has deprived philosophy of science of one of its fundamental characteristics. His influence has been permeating the past quarter-century of philosophy of science both in structure and in content – allowing, on the one hand, the fragmentation of the field and, on the other, the disconnection of the notion of scientific progress from any substantive ends science may be seen as pursuing.

Already in his challenging 'Philosophy of science: a subject with a great past' (in Roger H. Stuewer (ed.), *Historical and Philosophical Perspectives of Science* (Minneapolis, 1970), pp. 172–83) Paul Feyerabend denounced what he thought was the sorry state of contemporary philosophy of science, especially the very many epistemologies that had been erected 'in a spirit of *conformism*' (p. 172, his emphasis) and that had lost any chances of making an effective contribution to our knowledge of the world. In fact, in the past years the academic world has witnessed an ever-increasing proliferation of disciplines and subdisciplines. More and more often, specialists tend to lose sight of what is going on outside their restricted field of research, out of lack of interest or actual acquaintance with the technical knowledge required to follow the ongoing debates in one discipline or another.

Of course, increasing specialization is not wrong in itself; proliferations of problems and their studies are all to the good. But there may be a danger here. For, on the one hand, disciplines tend to propagate, rather than progress; the issues discussed no longer address wide-ranging problems but focus on details, consistently avoiding generalization. On the other, practitioners of disciplines and subdisciplines found ever more specialized journals, meet at narrowly focused conferences and speak their technical languages – that is, they tend to isolate themselves from others and evade interdisciplinary confrontation. In so doing, each discipline risks becoming an independent 'form of life' *à la* Wittgenstein, each alleged to have its own standards, or principles, or 'logic', which need not conform or be reducible to any other standards and which it is the task of the specialized philosopher to describe and clarify – not in the least to judge, defend or criticize. There is no more arguing or judging among disciplines; criticism, evaluation and explanation would no longer be proper philosophical aims. Knowledge is essentially divided, and description is all that remains to the philosopher. Philosophy loses its unifying perspective and philosophical critique is no longer of content, but of criteria application. In Feyerabend's words, all that is left are 'consolations for the specialists'.

There are many reasons for this tendency, of course. But certainly Kuhn's philosophy, with its emphasis on the proliferation of allegedly incommensurable specialities and subspecialities as the only measure of scientific and philosophical progress, may be deemed largely responsible for it. Kuhn-style proliferation is evasive, especially of controversy, and may lead to intellectual stagnation. A way to avoid that, I suggest, is resistance to the taboo of going beyond one's specialization – after all, they stagnate who have no intent to move forward. And specialism, in this respect, is the worst antidote.

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