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Book reviews

R. G. A. Dolby, **Uncertain Knowledge: An Image of Science for a Changing World.** Cambridge: Cambridge University Press, 1996. Pp. xi + 365. ISBN 0-521-56004-7, £40.

In the USA, the expression 'waving a flag and kissing a baby' refers to a political stance to which no right-minded person could possibly object (especially when taken in isolation of other beliefs its proponents are likely to hold). Until shown otherwise, this is my instinctive response to a book that purports to defend a 'critical realist' philosophy of science. I am afraid that a thorough read of Dolby's clear but colourless prose does little to alter that initial impression. This is a pity, since the book contains a wealth of case material and considered judgements about marginal science that would be extremely useful in a classroom setting to test students' intuitions about what constitutes 'scientificity'. (I personally would like to have seen Dolby apply his Solomonic sensibilities to the General Semantics Movement, which tried to turn logical positivism into pop psychiatry.) To put the point in perspective: these days much of what publishers market as 'textbooks' are short books by famous authors that do little to address the needs of the student clientele. Dolby's book suffers from the reverse marketing problem of being most charitably read as a sophisticated science studies textbook (one that should equally suit the needs of historians, philosophers and sociologists), while it appears exclusively in hardback and advertised as a cutting-edge monograph. My advice to Cambridge University Press is to put out a cheap paperback edition (Canto Books?) that presents *Uncertain Knowledge* as a much better version of what Alan Chalmers' *What Is This Thing Called Science?* tried to do.

But how might Dolby have convinced his publisher that this was a cutting-edge monograph? In the first place, the book does contain some genuinely novel and interesting ideas. The one that Dolby puts to best use is that science cannot flourish if one's immediate reality is too congenial to the theories one is most likely to propose. On this basis, he criticizes pragmatism's failure to acknowledge that theories can work for all the wrong reasons. He also uses this idea to explain the failure of totalitarian science, especially Lysenkoist genetics. Here I am not so sure. At the risk of sounding too much the relativist, I would argue that Lysenko's failure is partly predicated on his inability to eliminate his opponents – especially those *outside* the Soviet Union who still set the terms by which Lysenko's agricultural reforms were evaluated. In other words, Dolby officially endorses only one solution to an overly congenial reality, namely to extend one's theories to uncongenial domains. But why cannot one simply try to make more of reality congenial, specifically by adapting the

criteria by which knowledge claims are evaluated to the claims one is likely to make? In that case, Lysenko's science failed because it was not totalitarian *enough*, in which case the status of Dolby's uncongenial reality becomes less a 'deep' metaphysical fact than a 'superficial' political one.

In the context of Dolby's metatheory of science, evolutionary epistemology, the above point acquires a special significance. When faced with an uncongenial reality, is there any principled reason to prefer altering one's theories to altering reality itself? After all, in literally biological terms, human beings have proved capable of populating the entire planet, not because the survivors have had bodies tailor-made to their respective climates, but because they have altered their immediate environments to overcome any natural maladaptiveness. Indeed, there is one version of this alternative that Dolby himself seems to have found important in the history of science.

In chapter 8, after a remarkably even-handed treatment of the various factors that have been offered to explain the Scientific Revolution, Dolby settles on Kenneth Boulding's concept of 'change multipliers' to account for its long-term effects. In other words, the new advances in science could spread so rapidly and irreversibly over much of Europe because the changes they brought to society were portrayed at the time as exactly what was anticipated and wanted, whereas in retrospect they can be seen as having altered European sensibilities about what constituted epistemic adequacy, the good society, etc. Paradoxically, the more that scientists have thought they were proposing theories that successively approximate reality, the more they had in fact 'shifted the goalpost' in the direction of the theories they were most likely to propose. From this standpoint, whereby evolutionary adaptation shades into what social psychologists call 'adaptive preference formation', the main difference between the cultural paradigm shift effected by the Scientific Revolution and Lysenko's ersatz totalitarian science is that the former managed to eliminate those who drew attention to the opportunistic convergence of expectation and desire that is pasted over by the word 'adaptation'.

One of the most potentially attractive features of *Uncertain Knowledge* is its explicit aim of using science studies as a resource to improve science. Despite their reputation for 'transgressing boundaries', today's historians and sociologists of science remain remarkably timid when it comes to taking the great counter-inductive leap from 'is' to 'ought'. Somewhat surprisingly, however, Dolby fails to make much use of the growing literature in social epistemology that does just this (and here I mean not only my own work but also that of Sandra Harding, Helen Longino and Joseph Rouse; I was especially surprised to find no mention of even Roy Bhaskar, considering that he is probably the premier 'critical realist' in British philosophy today). Moreover, the one social epistemologist with whom Dolby openly identifies, Philip Kitcher, would be taken aback by Dolby's liberal attitudes toward marginal science – even though they both justify science on attenuated realist grounds. This misguided choice of allies suggests that Dolby has not fully come to grips with the fact that traditional epistemological distinctions, such as realism (or rationalism) versus constructivism (or relativism), matter little in addressing the most important normative questions facing the role of science in society.

For readers of this journal, a good way to get at these equations is to conjure up the spectre of the 'Normatively Innocent Historian of Science' (NIHOS) perusing the pages of *Uncertain Knowledge*. NIHOS would be most struck by the fact that Dolby is full of advice about what we should think of this or that development in the history of science, and even about the future of science. (Aside: Dolby's crystal ball is clearest when discussing the normative implications of the increased use of computers in science. Thankfully, there is none of the *revanchiste* humanism that can be found in some science studies' discussion of this issue.) And while NIHOS would have to concede that Dolby is judicious in the opinions he offers, nevertheless a question would linger: why should Dolby be the one to decide these matters? Of course, like all sensible social epistemologists, Dolby says that his judgements are just as fallible as the science he judges, but NIHOS remains unconvinced of

Dolby's motives because, strange as it may seem, the personality of Karl Popper continues to cast a shadow over normative approaches to science. Popper's normative theory may have been fallibilism but his normative metatheory was infallibilism – that is, concerning his own judgement about the strengths and weaknesses of theories! I am sure that Dolby is personally much more reasonable than Popper, but he never offers any principled reason why his judgements should not be read just as dogmatically as Popper intended his to be read.

The solution to this problem is that the social epistemologist needs to be less the *connoiseur* and more the *constitutionalist*. It is intellectually impressive – as well as pedagogically useful – for Dolby to offer his judgements about the past, present and future of science. Since we live in times where 'serious' academics recoil from issuing such judgements, concrete exemplars are all the more needed. However, to avoid the charge of being a disguised elitist or tyrant, Dolby needs to create some conceptual distance between his personal judgements about scientific matters and the framework (or 'constitution') within which he would have final judgements on these matters be made. For example, a social epistemologist may believe (on what she regards as very good grounds) that Creationism should not be accorded the same status as evolutionary biology in science courses, while at the same time granting that the final decision should be taken by the local educational authorities, to whom the social epistemologist would argue her case as vigorously as possible but ultimately accept whatever judgement they reach. (Hopefully, under such a democratic regime, the social epistemologist would stipulate that any judgements reached by the local authorities should be in principle reversible at some later time, depending on the consequences.)

It is far from clear what realism (or its opponents, for that matter) has to contribute to a discussion of constitutionalism in science, aside from providing coded ways of conferring privilege on one or another party interested in the direction that science takes. Unfortunately, even contemporary social and political theory proves an unsteady guide, given its tendency (following Rawls) to advance constitutions designed to ensure that the theorist's preferred policies are the only ones that can issue from the regime. An unreflective social epistemology could easily go down this route, which is, after all, the one traditionally followed by philosophers of science who aspired to a normative theory that would account for only those episodes that are now deemed exemplary in the history of science. Given that Dolby's normative sensibility is a bit more finely grained than that of the average Lakatosian, he is unlikely to fall into that trap, once he takes the idea of a normative constitution for science seriously. In the meanwhile, *Uncertain Knowledge* would make a nice undergraduate textbook.

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Robert Fox (ed.), **Technological Change: Methods and Themes in the History of Technology.** Amsterdam: Harwood Academic Publishers, 1996. Pp. vii + 271. ISBN 3-7186-5792-9. £36.00, $54.00.

This volume and the 1993 conference in Oxford from which the papers are drawn were conceived as a sequel to the meeting on 'The Structure of Scientific Change' held over thirty-five years ago in the same city. Since there cannot be very many participants at the latter gathering who are still professionally active today, it is perhaps worth saying that one of the contributors was Thomas Kuhn, speaking just before the publication of *The Structure of Scientific Revolutions* (Chicago, 1962). In retrospect then it is easy enough to identify the early 1960s as a period in which historians of science – at least in the English-speaking world – started to come to grips with the theoretical concerns of the social sciences. Will this collection of fourteen essays (selected from over a hundred delivered at the conference) be seen as marking a similarly profound moment in the process of bridge-building between historians of technology and colleagues in the social sciences?

Probably not, partly because, as Robert Fox points out in his introduction, many historians of technical change have long been interested in

economic theory, econometrics and their historiographical counterpart, economic history. But I recall that for many the most keenly anticipated debates over methodology at the 1993 meeting were those with advocates of the various forms of social constructivism that, even then, had been on the agenda for over a decade. Often inspired by earlier work in the sociology of science, these approaches feature in this collection's opening section on theoretical models (to which should really be added the final essay in the book, by Donald MacKenzie). But there are also several other sources of inspiration for historians looking for a theoretically informed 'big picture' of technical change.

MacKenzie's and Trevor Pinch's essays draw most obviously on social scientific analyses of the natural sciences. Clearly and engagingly expressed, the basic themes will be familiar to anyone who has paid any attention at all to the dialogue between sociologists and historians of technology: sociology's distinctive contribution to the analysis of technical change is an insistence on showing how interested social groups negotiate just what it means for an artefact to be considered viable. By contrast, Antoine Picon's and John V. Pickstone's stimulating and complementary essays tread less familiar ground. Picon connects the changing representation of nature and society in eighteenth-century France with the evolution of what he calls the 'collective mental frames' of historical actors involved in the production of technologies. Pickstone too focuses on the eighteenth and early nineteenth centuries, drawing widely on the earlier writings of Foucault and recent historiography of science, technology and medicine to identify two 'ways of knowing' – the savant and the analytical – which, he argues, formed contrasting approaches to the understanding and improvement of artefacts. There is, I think, a bridge to be built here between Picon's 'engineering rationalities' and Pickstone's 'ways of knowing', on the one hand, and the work of the historical sociologists Wiebe Bijker and Paul Rosen on '(socio-)-technical frames', on the other. Finally, Joel Mokyr draws upon Darwinian evolutionary biology to outline what he terms 'the evolutionary method in technological change'. He variously calls the relationship between the two domains of inquiry a metaphor, an isomorphism and an analogy, before finally characterizing it 'as another application of a Darwinian logic that transcends the world of living beings' (p. 64). No doubt historians of science will come to their own conclusions.

The remaining nine papers offer some illuminating if necessarily highly selective snapshots of certain lines of substantive inquiry. The historiography of the pre-modern era is marked by just two papers, Bert Hall's and Richard Holt's contrasting judgements on Lynn White's *Medieval Technology and Social Change* (Oxford, 1962). The next three papers treat aspects of the first industrial revolution, for the most part in the British context. John Harris examines the effectiveness of legal attempts by the English in the eighteenth century to prevent the transfer of industrial skills and machinery to foreign powers: he includes some passing comments on the significance – or more strictly, the lack – of scientific culture in explaining English technological superiority. Christine MacLeod is also concerned with the appropriation of technologies, but her focus is on debates in Britain during the third quarter of the nineteenth century over the definition of what we would now call intellectual property rights. She neatly demonstrates how quarrels over whether invention was an act of irreducibly individualistic insight or a product of wider social and economic circumstances were directly related to attempts to abolish the patent system. The third of this trio of papers engages a little more explicitly with the theoretical concerns of the volume's first section, rejecting economic models of technical change and social constructivism of the Anglo-American Dutch variety in favour of Bourdieu's notion of habitus. But perhaps the most interesting aspect of Patrick O'Brien, Trevor Griffiths and Philip Hunt's study of the evolution of the British textile industry is the suggestion that the rapid pace of technical innovation up to the middle of the nineteenth century can be understood only in terms of the political economy of British trade overseas.

The final four papers (if we exclude MacKenzie's) deal with technology, politics and national cultures. Ian Inkster contributes to a well-established vein of inquiry, reminding us

that attempts to transfer technologies from one country to another fail as often as not. He illustrates some of the reasons for this historically through a fine comparative study of attempts to force industrialization in Russia and Japan by means of the importation of foreign technologies and personnel in the period 1870–1914. This paper, with its emphasis on the messy complexity of such transfers and the need to transform social institutions and cultural milieu if there is to be any chance of success, can profitably be read with Morris Low's look at modern attempts to enhance Japan's international political standing by raising barriers to collaboration on technological matters. As Low remarks (p. 209), in today's global economy, technology transfer 'no longer follows a simple linear model'; to which we might add, it never did. By way of contrast, Yves Cohen analyses the engagement of politics and industrial production in France and the USSR during the 1930s, concluding that in many sectors of the economy the emphasis of Stalinist politics on manufacturing gave less impressive results than in France. Finally, Thomas P. Hughes argues convincingly for a greater emphasis on the study of the managerial organization of the very large scale and highly complex technological systems that have grown up since the Second World War.

The editor warns in his introduction that the collection is not intended as a definitive guide to the current historiography of technical change (let alone that of technology *per se*), and as long as the essays are read with this in mind historians of science will find much that is rewarding. That said, I do feel that, individually excellent though many of them are, taken as a body the empirical studies gives a slightly misleading impression of what is going on in the historiography of technical change. The dialogue between sociologists and historians of technology has produced some fine studies recently, including one or two monographs, but one does not really get a true sense of the richness of this work here. And it is a great shame that – aside from the odd remark in the more explicitly methodological papers – issues of gender have been excluded: all more the pity given that by far and away the liveliest and most rewarding session that I attended at the 1993 meeting was on the gendering of technologies of domesticity, leisure and pleasure. Still, almost without exception these studies are marked by what Robert Fox characterizes as a rich and 'flexible eclecticism' of analytical approach. And that of course is as much one of the joys of working in the history of technology as it is in the history of science.

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J. L. Berggren and R. S. D. Thomas. **Euclid's Phaenomena: A Translation and Study of a Hellenistic Treatise in Spherical Astronomy.** Sources and Studies in the History and Philosophy of Classical Science, 4. New York: Garland Publishing Inc., 1996. Pp. xi + 132. ISBN 0-8153-0493-5. $36.00.

The spherical earth was surrounded by a huge celestial sphere that revolved daily about it. This sphere had on it, together with other circles (for example the arctic and tropic circles), three particularly important great circles (or, more strictly, their circumferences): the equator, perpendicular to its axis of rotation; the ecliptic, along which the sun travelled annually; and the horizon, which depended on the earthly position of the observer and was stationary with respect to the earth rather than the celestial sphere. The ecliptic was divided into twelve equal parts, the twelve signs of the zodiac, but these did not rise over the horizon in equal times, and their appearances were also dependent on the observer's terrestrial location. These factors accounted for such phenomena as the differing lengths of daylight at different times of the year and different parts of the world.

There are those (including the present reviewer) who find it much more difficult to think geometrically in three dimensions than in two, and find it hard to imagine even the relatively simple relations between these three great circles. For such, a step-by-step rigorous treatment was especially valuable, and in Antiquity one was provided by Euclid's *Phaenomena*, a work which (like his *Elements*) probably superseded various earlier writings in the genre. In their particularly fine volume Berggren and Thomas have for the first time publicly rendered it into English, and

they add copious helpful annotation and explication.

As with most Greek mathematical texts there are problems of transmission, and two main manuscript traditions are extant. One (*b*) was used by David Gregory in his 1703 edition of Euclid's works; his version of the *Phaenomena* remained standard until Menge's edition of 1916, which was able to take into account an earlier tradition (*a*). Berggren and Thomas argue plausibly that Pappus of Alexandria (around the end of the third century AD) had a version very close to Euclid's own, and that this was soon afterwards fleshed out (perhaps by Theon of Alexandria) to form tradition *a*, which itself was later reworked into *b*. My only regret about the volume under review is that the Greek text could not have been provided on facing pages, but this would no doubt have increased the price unacceptably, and the lack is partially remedied by the provision of very useful English and Greek glossaries of technical terms.

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Robert Boyle, **A Free Enquiry into the Vulgarly Received Notion of Nature,** edited by Edward B. Davis and Michael Hunter. Cambridge Texts in the History of Philosophy. Cambridge: Cambridge University Press, 1996. Pp. xxxvi + 171. ISBN 0-521-56100-0, £37.50, $54.95 (hardback); 0-521-56796-3 (paperback), £13.95, $18.95.

It is doubtful whether a more complete statement can be found to exemplify the outlook of one of the leading exponents of the mechanical philosophy, Robert Boyle, than in his composition *A Free Enquiry into the Vulgarly Received Notion of Nature*, which finally saw publication in 1686 after going through various drafts for more than twenty years. In this dense work we find Boyle, as the editors point out, displaying 'philosophical acumen, theological learning and experimental expertise' (p. x). Boyle's purpose was to bring about not only a convergence of mechanistic science with the biblical doctrine of creation, but also to combat opposing conceptions of the physical world, particularly those of an Aristotelian and Galenic character. The intelligibility of the mechanical conception of matter and motion expressed for Boyle a corresponding denial of 'Nature' possessing any purposive wisdom or thinking faculty of her own. This in turn suggested for Boyle a proper understanding of God's absolute sovereign power over the created order that threw into sharp relief any other impersonal semi-deity held to be immanent in the world. In Boyle's wide-ranging attacks on 'vulgar' conceptions of nature, an impressive list of able scholars, including J. E. McGuire, Francis Oakley, Margaret Osler and John Henry, have long discerned a lengthy list of opponents in Boyle's sights. Familiar enemies found in some of Boyle's earlier writings of the 1650s, such as Epicurean atomists who were taken to task for advocating random chance as the emergent principle of the universe, were soon to be joined at the Restoration by other materialists, such as Thomas Hobbes, who denied the air-pump's capacity to establish experimentally a vacuum. These particular threats of 'atheism' required to be met, in Boyle's view, with an improved characterization of science's relationship to religion, which might also underline the limitations of knowledge claims about nature. *Free Enquiry* famously expresses this latter point in revealing Boyle's strong unease with those who suggested matter obeyed 'laws of nature' in some literal sense. The discourse, as Davis and Hunter emphasize, equally shows Boyle's unhappiness with Galenic overestimates of 'nature' as a wise healing agent, and the mediating role of 'plastic power' used by the Cambridge Platonists, More and Cudworth, to resolve the theological conundrum of God as both beyond and within nature.

Out of the forty or so books that Boyle published, the editors have chosen this particular work as worthy of wider exposure beyond the confines of mainly historians of science. The text used is both based on the first edition and collated with the 1687 Latin edition, as well as being more complete than Birch's modernized eighteenth-century text. Some readers may note some irony in the fact that the text employed here is itself modernized to conform to the editorial policy of Cambridge Texts in the History of Philosophy. There is a useful glossary

of unfamiliar terms used by Boyle and a short guide to further reading. The guide is necessary, not least because the complex nature of Boyle's corpuscularianism and its relationship to his theological positions can only be alluded to in an edition of this kind.

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Francois de Gandt, **Force and Geometry in Newton's Principia,** translated by Curtis Wilson. Princeton: Princeton University Press, 1995. Pp. xiv + 296. ISBN 0-691-03367-6. $49.50.

In this extremely valuable book, de Gandt brings the wealth of a long career to bear on the fundamental physical ideas behind Newton's *Principia* and on the mathematical methods employed to express them. We are indebted to Curtis Wilson for providing a clear and flowing translation. In the translator's introduction we are told that the method employed throughout the text is that of *explication de texte*, which combines close reading of texts with historically informed commentary. Wilson claims that in applying this method to selected writings both of Newton and of his contemporaries and predecessors, de Gandt has given us a deeply original mediation on the sources and meaning of Newton's *Principia*. This reviewer is in full agreement.

The aim of the work, as expressed by the author, is to employ the geometrical treatment of force to provide an introduction to the reading of Newton's *Principia*. The geometrization of force is set forth initially in the first chapter, in which de Gandt presents a translation of and a detailed commentary on the *De motu* of 1684. This manuscript contains the demonstration of four theorems and the solutions to seven problems that were sent to Halley in response to his initial request for a solution to the problem of planetary motion. The first four theorems and the first four problems provide the foundation for the first seventeen propositions of Book One of the first edition of the *Principia*: the section identified by Newton as critical to any reading of his extended work. In a preamble, de Gandt's delightful sense of humour shows through as he introduces the reader to Halley's famous visit to Newton and the quest for Wren's 'forty-shilling book' in the format of a 'typical British detective story', in which Newton is seen as the Sherlock Holmes of the affair. The text of *De motu*, which contains Newton's geometrization of force, is the result of Halley's request and the explication of this text is the charge of the first chapter.

The second chapter places the *Principia* in the intellectual context of the seventeenth century by setting forth the essential features of Newton's contributions against the background of other seventeenth-century scientists, such as Kepler, Galileo and Huygens. With respect to Kepler, the emphasis is on his concern with physical cause in addition to the more obvious concern with archetypal cause. In particular, the analysis centres on Kepler's geometric laws of the diffusion of the solar magnetic force. There is also an extended discussion of Newton's interpretation of Galileo's contributions to the two laws of motion and to what the author describes as Newton's implicit fundamental hypothesis: 'force acts in time; it is regulated according to the time'. All are set in an extended discussion of seventeenth-century mechanics. The chapter concludes with consideration of the relationship between gravity and centrifugal force with emphasis on the contributions of Descartes and Huygens. In the context of 'the richness of the texts' of other authors, de Gandt sees Newton's *Principia* appearing as an exceptional creation.

The third and final chapter is exclusively devoted to the discussion of the mathematical procedures of the mid-seventeenth century that relate to those employed by Newton in the *Principia*. In particular, the author points to 'the difficult problem of the distinction between a differential or fluxional calculus and the modes of reasoning of the *Principia*'. The chapter begins with a review of the background to the method of indivisibles, moves on to consideration of the kinematics of curves and the method of fluxions, and concludes with a discussion of two distinct mathematical methods employed in the *Principia*: the method employed in the solution of direct problems (Book One, propositions immediately following Proposition 6) and the method employed in the solution of inverse problems (Book One, propositions immediately

following Proposition 39). The direct problem seeks the nature of the force, given the path and force centre, and the inverse problem seeks the path, given the nature, of the force. The work concludes with a short overview of the notions of force, and of their relationship to mathematics and to matter.

Most reviewers are not satisfied, however, until they express some reservation as well as acclamation for a work, and I am no exception. Among the topics discussed in chapter 2 on the background of Newton's dynamics, is the role played by Robert Hooke, especially in the famous correspondence of 1679 in which Hooke brought up the question of planetary motion and put forth his suggestion that the analysis should rest on the tangential and inward radial components of motion. De Gandt claims that 'it is very probable that Hooke's intervention was decisive in Newton's intellectual evolution, leading him to a new conception of curvilinear motion' (p. 147). In partial defence of that claim, de Gandt attempts an analysis of the curve Newton sent to Hooke after receiving Hooke's 'correction' of Newton's first response. This figure has been the subject of much debate and de Gandt joins a long and impressive list of scholars who have contributed to that debate. Without attempting to provide the details of the analysis, let me point to the original figure and to de Gandt's reconstruction of it (see Figure 1). The critical point is that the reconstruction is a 'closed' figure: it starts at point A and the path moves through points F, G, H, I, K, L, and then joins point A, to reproduce the curve once again. In the original figure, Newton terminates the diagram after point L: he does not produce a closed figure. The critical nature of this distinction is made clear in a recent article in which details of the original figure are obtained using a numerical technique (M. Nauenberg, 'Newton's early computational method for dynamics', *Archive for History of Exact Sciences* (1994), **46**, 221–52). Central to the numerical technique is the concept of curvature, in which a portion of a curve is represented by a portion of the circle of curvature that best approximates the curve at that point. This technique was alluded to by Newton in an early (1664 or 1665) cryptic note in his *Waste Book* and an alternative version was

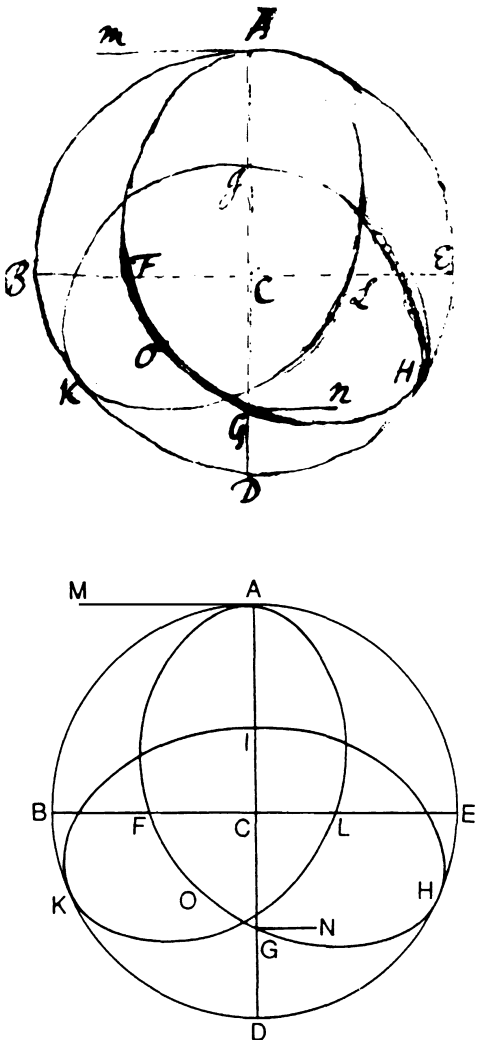

Figure 1.

introduced into the revised editions of the *Principia*. The central role of curvature in Newton's dynamics is to be found in the early commentaries on Newton's work as well as extensive references in D. T. Whiteside's notes in *Mathematical Papers of Isaac Newton* (in particular, volume 6). There is no discussion of the role played by curvature in de Gandt's work. The book, nevertheless, is an extremely valuable contribution to the world of Newtonian scholar-

ship in terms of the breadth of what it does discuss.

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Chloe Chard and Helen Langdon (eds.), **Transports. Travel, Pleasure, and Imaginative Geography, 1600–1830.** Studies in British Art, 3. Published for the Paul Mellon Center for Studies in British Art and the Yale Center for British Art. New Haven: Yale University Press, 1996. Pp. viii + 341, illus. ISBN 0-300-06382-2. £35.00.

Travel has been a significant spur to the scientific imagination, the experience of Charles Darwin and Alexander von Humboldt providing the most celebrated examples. Anthropology, natural history and geology have all been shaped by exotic experience and the imaginative transports of travel. Crossing boundaries and the expansion of experience are themes that link the history of science to art history and literary history. The present volume, which has its origins in a series of seminars in London in 1992–93, has its focus on art history and literature, but two papers in particular address topics of more direct concern to historians of science. Richard Hamblyn's essay on depictions of volcanoes in the eighteenth century is concerned to illuminate the formation of a new discourse, the emergence of geology as a science from an antiquarian vision of the landscape. Ken Arnold's account of travel and exotic curiosities in the seventeenth century examines the role of collecting in the early Royal Society, focusing on problems of cultural interpretation and assimilation. The volume includes an essay by Nicholas Thomas on social life in New Zealand as reported by the Forsters during Cook's second voyage; and much else to enjoy, in studies by Elinor Shaffer, Roy Porter and others.

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Michael Shortland (ed.), **Hugh Miller and the Controversies of Victorian Science.** Oxford: Oxford University Press, 1996, Pp. x + 401. ISBN 0-19-8540531-1. £49.50.

Hugh Miller, as this collection of essays emphasizes, was 'a man of many parts'. Several of his books, notably the *Old Red Sandstone* (1841) and the autobiographical *My Schools and Schoolmasters* (1854) were best-selling classics of Victorian non-fiction prose. His *Footprints of the Creator* (1849) may have sold more copies than Robert Chambers' anonymous *Vestiges of the Natural History of Creation* (1844), the work it was intended to refute. As editor of the *Witness* newspaper, Miller became a key figure in Scottish religious and literary life during the early Victorian period. When Miller shot himself in 1856, Charles Dickens mourned the death 'of a delightful writer, an accomplished follower of science, and an upright and good man'. It was, as Thomas Carlyle said, 'the world's great loss'.

Michael Shortland has assembled an interdisciplinary group of contributors to analyse Miller's remarkable career. Although growing up in circumstances that were effectively middle class, Miller chose to become a stonemason; and even after turning to journalism he posed for the pioneering photographers David Octavius Hill and Robert Adamson as a 'bonneted mechanic'. Leading themes of the collection, highlighted in Shortland's long and occasionally rambling opening chapter, are individual self-fashioning and self-presentation. Although Shortland engages in more extensive theoretical reflection that the occasion might be thought to warrant, he offers subtle readings of texts and images, and perspectives that could be applied to other scientific figures in the nineteenth century.

Later chapters by David Robb and David Vincent continue the theme of self-fashioning with revealing discussions of Miller's autobiographical writings. Several other essays – particularly a fascinating piece on 'Miller's madness' by Roy Porter – touch upon his self-destruction. There are good accounts of Miller's religious views (John Brooke and John Henry) and his role in forming the breakaway Free Church of Scotland (Donald Macleod). Two of the best chapters look at Miller's work in relation

to Scottish traditions of antiquarianism, local history and folklore study (David Alston and James Paradis). David Oldroyd bravely tackles a task central to any understanding of Miller, by providing the first modern survey of his work in geology and palaeontology. The book is generally well produced, although there are annoying repetitions of basic information that should have been spotted. For example, a story about Miller being attacked by fairies in his trousers is recounted at some length three times (pp. 42, 223, 270). (The fairies, alas, were probably ants.)

The volume concludes with a very useful eighty-four page inventory of Miller's publications. Shortland acknowledges that it is by no means complete, particularly in relation to juvenilia, anonymous writings, foreign editions and later printings. To provide a full bibliography would be, as Shortland says, a 'massive' task (although perhaps not quite so 'senseless' as he suggests). The list is most notable for including Miller's contributions to the *Witness* newspaper from its foundation in 1840 to his death in 1856. Unfortunately, Shortland never says how he has identified Miller's authorship of otherwise anonymous articles, stating simply that he compiled the list 'by examining original copies of the *Witness*'. This is in itself a huge job – but as editorial material in the newspaper was almost always unsigned, we are left to infer that judgements about inclusion or exclusion have been made on the (inevitably insecure) basis of literary style. Although Miller probably did write most of these anonymous articles and editorials, the attributions need to be used with caution.

Edited volumes focused on individuals used to be justified on the grounds that their work could be treated 'definitively' only by a team of specialists. In contrast, Shortland believes that a variety of perspectives is valuable in its own right, as a way of avoiding the false sense of coherence necessitated by traditional biography. Certainly the viewpoints offered here are very diverse. They range from psychoanalytically informed deconstruction to retrospective evaluation of Miller's science in light of modern knowledge. Perhaps the edited volume, with all its opportunities for displaying a range of unresolvable analyses, will become the genre of choice for academic postmodernists? All the same, for Miller to become accessible to a wider public, the 'full biography' that Shortland initially aspired to write remains a desideratum. Despite obvious pitfalls, there are many examples to suggest that biography can be both popular and innovative.

This is not an introductory book, although it will surely become the starting point for serious research on its subject. Readers who know nothing of Miller might be best advised to begin with some of his essays, with *My Schools and Schoolmasters*, or with the autobiography he wrote in 1829 while still a stonemason. Shortland has recently edited and published this remarkable document for the first time as *Hugh Miller's Memoir* (Edinburgh, 1995). With the appearance of the present volume, anyone who wants to learn more about this fascinating figure will be doubly in Shortland's debt.

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