Book Review

Editions and Selections

KLAAS VAN BERKEL, ALBERT VAN HELDEN and LODEWIJ P ALM, editors, A History of Science in The Netherlands: Survey, Themes and Reference. Leiden: Brill, 1999. xxviii + 659 pp. Illus. Index. €180.00/US$221.00 (cloth). ISBN 90-04-10006-7.

Virus, protein, vitamin, gene, galaxy, electrical condenser, pendulum clock, liquid helium—these are only some of the seminal notions elaborated by Dutch thinkers. A History of Science in The Netherlands provides a general and accessible reappraisal of innovations, institutions, personalities, ideologies, and sentiments over half a millennium. Serving as a handbook or reference work, this collective enterprise has the rare distinction of inviting sequential reading. Once several pages into the engaging general introduction, I found it hard to put the book down; a comparable attraction exerted by recent works in our discipline has been felt only with Cavendish by Christa Jungnickel and Russell McCormmach and Landscape and Memory by Simon Schama. It is as if colleagues from the Netherlands are speaking at an intimate colloquium in straightforward, Dutch-turned prose.

The first part of the volume is a revision of Klaas van Berkel’s fine survey, In het voetspoor van Steven: De geschiedenis van de natuurwetenschap in Nederland, 1580–1940 (1985), translated by Albert van Helden. The second part consists of thematic essays on early modern medicine by Frank Huisman, on preparation for university study by Pieter Boekholt, on the market for and the makers of scientific instruments by Peter de Clercq, on science and religion by Rienk Vermij, on science and philosophy by Michiel Wielema, and on mathematics and its relation with physics by Gerard Alberts, Eiso Atzema, and Jan van Maanen. The third part contains more than 200 pages of short biographies, with an indication of primary and secondary sources. The fourth part is a bibliography in two sections: in the first section, a selection by Van Berkel of more recent and important titles, arranged chronologically, and in the second section the discipline of history of science in the Netherlands, notably all scholars with the title of professor, compiled by Lodewijk Palm.

The heterogeneous contents notwithstanding, insight and evaluation radiate from nearly every page. Opening the volume at random to page 51, we see Burchardus de Volder introducing scientific instruments at the University of Leiden in 1675 and Newton’s Principia around 1687, also at Leiden; on page 202, the industrial laboratory of Gilles Holst at the Philips Company in Eindhoven, a pioneering site for fundamental research in physics; on page 280, the long tenure of the Latin schools as the cradle of university students; on page 343, the absence of a nineteenth-century clash between science and religion; on page 410, the first page of Van Berkel’s biography of Isaac Beekman, shorter and to my taste sweeter than the treatment by the masterful Reyer Hooykaas in the Dictionary of Scientific Biography; on page 577, the first page of Palm’s biography of Franciscus de Le Boère, Sylvius, again a discussion complementing the one in the DSB by the prolific historian of medicine Gerrit Arie Lindeboom.

Taken together, the discussions provide dignity to a national culture that remains under-appreciated in the English-speaking world. A reader will see here no case study of a small country, but rather a story in time and place that demands appreciation on its own merits. Because the book appears in English for international readers, the contributors avoid much of the wider, European background. Reading about Dutch scientific societies in the eighteenth century will naturally call to mind the rise of the Lit & Phils in Britain. The weak Dutch university culture early in the nineteenth century will readily be seen to mirror anaemic French universities. Johan Rudolph Thorbecke’s felicitous reinvigoration of practical learning in the secondary schools and the correlative rise of university research apparently owe much to German inspiration. (Thorbecke’s liberal notions drew inspiration from the German jurist Friedrich Karl von Savigny, whose young fiancée Thorbecke wooed away and married; Thorbecke’s letters to his wife, published in 1936, are enormously attractive billets-doux.)
During the Cold War, the Central Intelligence Agency promoted area studies in the academic realm. American political strategy required experts in the culture of contested parts of the world. Scholarship consequently divided between mainstream cultural studies—notably, the history of Europe and the United States—and marginal area studies. The division is also found in the discipline of history of science. Every historian of science, it seems, registers a passing acquaintance with London’s Royal Society in the seventeenth century, with Lavoisier’s chemistry, and with Darwin’s evolution. Topics such as Islamic or Latin American science pass under virtual silence for all except a small group of specialists. More than a hundred years ago, when the Anglo-German philosophical and entrepreneurial wonder John Theodore Merz set about writing his remarkable history of nineteenth-century science, he restricted himself to sources in French, English, and German—and effectively to people working in the national sectors dominated by those languages. Merz’s big three (with an aside to Renaissance and Mannerist Italy) still occupy centre stage in presentations of the history of science, although with the rise of American hegemony in the world, teachers and writers have increasingly plunged into the vast morass of science in the United States. The traditional focus excludes places and times where many significant themes may be examined at close range.

Wonder how path-breaking innovation may coexist with Pietist belief? Attracted to the way research depends on popular interest in science and scientific instruments? Want to see how brilliant laboratory work may be accomplished with slim means? Curious about why governmental authorities fund enterprises with little promise of lucrative pay-off? Like to know how, for nearly three generations, just the right scientists land in university chairs? Perhaps, in view of today’s accent on projecting education to remote locations, historians would care to examine spectacular developments in the Dutch East Indies. Maybe scientists would like to know how a small country, with no mountains of any kind, emerges in the twentieth century as one of the premier environments for astronomy. Possibly, with attention now being directed to bilingual education, educators are intrigued about how scientific excellence—in the Dutch language—relates to fluency in English, French, and German. In addition to providing access to all these themes, *A History of Science in The Netherlands* makes a splendid case for a fourth, major scientific culture in modern Europe.

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I. McCalman, editor, *An Oxford Companion to the Romantic Age: British Culture 1776–1832*. Oxford: Oxford University Press, 2000. xiii + 780 pp. £85.00 (hbk), ISBN 0-19-812297-7; £20.00 (pbk), ISBN 0-19-924543-6.

Like the *Encyclopaedia Metropolitana* which Coleridge planned, this *Companion* begins with thematic essays, and then half-way through moves into an alphabetical sequence of short entries. The editors of the *Metropolitana* hoped that those reading the first thirteen enormous tomes would thereby acquire a good general education, getting detail from the subsequent entries; no doubt on its smaller scale that is the intention here. The planning, and the masterful editor’s introduction, come from Australia; so we might expect that we shall find the familiar world turned upside down, and some radically new perceptions. Distance can be a tyranny, but it can also lend enchantment.

We duly find essays on Domesticity (as well as on Women), on Poverty and Class, on Sensibility and Viewing and on Consumerism among the forty-one topics covered. Few will want to read them all one after the other, like an enormous repast of starters; some are very stimulating, many pack in a lot of information. Wordsworth turned away from contemporary poetic language to that of ordinary people: it is a pity that some of these essays are written in the obscurity of a learned tongue full of ‘discourses’ on this and that. There is an inevitable tension throughout between the need to write a lively essay which like a good lecture should arouse curiosity and enthusiasm and not be overloaded with detail, and the temptation to be thorough. There are too many lists, and sentences such as ‘Tom, Dick and Harry did likewise’, to make all the essays a good read.

It is strange for this period that Religion merits only a short essay, though there is another on Mythology; and indeed religion does keep cropping up in other places such as Enlightenment. Perhaps surprisingly in an Oxford Companion, while the Oxford Movement
gets in (though Keble’s Christian Year does not) the universities of Oxford and Cambridge are not in the index, though they are discussed briefly under Education. Richard Yeo on Natural Philosophy and Roy Porter on Medicine do an excellent job, which should ensure that the place of the sciences in the Romantic movement is duly appreciated. Historians of science in search of context will find many other essays useful, notably on Publishing, on Empire, on Women, on Industrialization and on Psychology. The last essay, on Literary Theory (which might well have made an outsider wince), is very good indeed. Exploration is given rather ideological treatment: we get little feeling of being alone on the wide wide sea, or of the sheer excitement travel narratives brought stay-at-homes. Foreigners such as Alexander von Humboldt or Goethe are barely our companions, with one mention each: it is difficult to draw frontiers, but the importance of such writings, and the business of translation (often by women) should not be forgotten.

What is completely clear is that there was in this period nothing like the ‘two cultures’ debate that C. P. Snow set off in the 1950s, but we might have got a little more about the sheer empirical aspects of Romanticism. Focussing upon the particular and the concrete, rather than on the abstract and general, was a feature of science as of poetry—and would have improved some of the essays here. We learn little about what people ate and drank, about meal times—when did breakfast parties happen, and how late was dinner?—about letter-writing, and the technicalities of postage and getting a frank. Transport might have been worth an essay, and, from an Australian perspective, transportation also—John Gascoigne on Empire is interesting, but we get a metropolitan view when we might have hoped for a colourful colonial perspective.

Though there were few alchemists literally speaking, electricity and chemistry seemed to be penetrating below the surface of things, and a new Newton was awaited who would reveal the simple laws of matter and force, while the imaginative alchemy that revealed potential gold in base metals was also crucial at this time. Science was a source of metaphors: even if exactly what Coleridge picked up and used from Davy’s lectures remains in doubt, we can with Yeo be sure that it promoted a dynamical view of the world. We can look up ‘chemistry’ (though not ‘dynamics’) in the second section of the book, cross-referenced to provide magnificent bedside reading full of serendipity as one thing leads to another. The names of people and places, such as Holland House, alternate with more abstract entries, on scientific societies, self-improvement and sermons, for example. There are also black and white illustrations scattered throughout the text. From these many and varied entries everyone can learn a great deal; and of course grumble about some of them, such as when ‘loyalism’ has no reference to the American colonies.

While one might regret that there were not fewer and longer essays, so that overlap would be reduced and there would be more room for detail and argument, one should not thus quibble when offered so splendid a compilation. Those of us teaching courses in which this period is an important component will find the book invaluable, and indeed the very multiplicity of voices a virtue. And for ordinary reference purposes and for thickening contexts, as well as at the bedside, the second part will be delightful as well as useful. The book is worth its considerable weight.

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Biographies

JOHN CAMPBELL, Rutherford: Scientist Supreme. Christchurch, New Zealand, AAS Publications, 1999. 506 pp. + index. NZ$50.00, £25.00, US$40.00. ISBN 0-473-05700-X.

Ernest Rutherford was a nice fellow. He had a sense of humour, he was unpretentious, down-to-earth, generous, and immensely successful. Born into a large rural New Zealand family, he advanced to become one of the great physicists of the twentieth century: Nobel Prize winner, Professor of the Cambridge University Cavendish Laboratory, Fellow of the Royal Society, discoverer of the theory of radioactive disintegration, splitter of the atom, and so on and so forth.
John Campbell, Rutherford's latest biographer and a physics professor at the University of Canterbury where Rutherford studied, also seems to be a nice fellow and he certainly has a sense of humour. His *Rutherford: Scientist Supreme* is an enjoyable and well-written celebration of New Zealand's great son. Campbell has two aims. One is to let Rutherford's example show young New Zealanders that it is possible to achieve great success coming from humble origins and without showing great promise early on. The other is to set the record straight on the many anecdotes circulating about Rutherford. I shall first discuss Campbell's biography on these, the author's own, terms before proceeding to issues that several historians of science have shown interest in, and that Campbell does not address or addresses only obliquely.

Not being a young New Zealander I cannot vouch for Campbell's success in his first stated aim, but I imagine that his book is eminently suitable. Rutherford's engaging character emerges as clearly as can be desired of a narrative (although it must be said that the changes of character from a shy and diffident youth to a boisterous and choleric laboratory director are a little abrupt and fitful). Rutherford's life also makes for a nice and uplifting plot and it is told in vivid language. For example, Rutherford's London club, the Athenæum, is described as being a port bottle's throw away from the Royal Society. Rutherford was called Ern by his relatives, and the author assumes the same kind of intimacy by using this reference throughout the book. Because of Ern's jovial character this works just fine. (The same technique grates when John uses the nickname Willi for the austere German Professor Wilhelm Conrad Röntgen.) The book is nicely illustrated with many photographs and a useful map of Christchurch, where Ern spent his student days.

The biography is crammed with amusing anecdotes, half a dozen of which I just have to pass on to readers. A teacher teaching at both Ern's boarding school for boys and at a nearby girls' school had a semi-dress bell-top hat, in the lining of which the students used to send love notes (p. 54). At one of the ceremonies at Canterbury College where Ern received a diploma, the undergraduates had prepared to foil long-winded speakers. A banner declared that speakers had ten minutes. At the ten-minute mark all the students hid behind a deafening rustle of newspapers. If that did not work then time was called, followed by a ten second countdown and culminating with an eruption of alarm clocks (p. 138). As a not very successful lecturer, Ern had some trouble keeping his students quiet. Once, faced with a classroom disturbance, he declared in a determinedly dignified manner: 'I do not require an ass in this lecture room to keep me company.' Pause. 'Gentlemen, I guess the joke is on me' (p. 251). Ern's comment on a tennis double partner: 'stood at the back of the court where he was worth a good many points to the side by keeping up a running commentary on the looks and behaviour of the opponents' (p. 350). At one lecture Ern had some radium bromide passed gingerly around the audience. When a colleague later commented that it seemed rather large, Ern confessed that it had been a mixture of table salt and sand (p. 350). And finally, reacting to pomposity at a Trinity College high table: 'Anyone for the Marx Brothers?' (p. 445).

It remains to be seen whether Campbell has succeeded in his second aim, that of setting straight the record of the many little details of Rutherford's life. He (or perhaps the publishers?) has come to the unusual decision of not publishing the endnotes along with the rest of the book. Instead, the book contains a promise that a master copy of the references will be deposited at ten repositories with a Rutherford connection one year after the publication date. However, this has not occurred yet. An inquiry sent to the publishers in January 2001 elicited the following response: 'The master copy won't be finalised for release for another year due to various factors including paralysis in this country and delays in pinning down projects overseas.' So, the jury will have to remain out for a while yet.

I wish now to turn to the unfair part of the review in which the book is judged by standards other than those intended by the author. I feel obliged to do this because few readers of *Annals of Science* will be young New Zealanders looking for a role model, and a number of them will be historians of science with interests related to Rutherford's life and work but not centred upon them. Let me list three 'sins' committed by Campbell.

His biography is an unreconstructed and unabashed hagiography. The subtitle of the book is indeed *Scientist Supreme*. It borders on the tedious that praise for Rutherford, be this for his scientific genius or his human qualities, is always quoted as an end in itself. Campbell does give voice to opinions that Rutherford became domineering and intimidating,
but these opinions are confined to a very few of the 500 pages and even explained away as a mask hiding his shyness.

The biography is New Zealand-centric, going overboard in its compensation of the lack of New Zealand-related information in previous biographies. 43% of the pages are devoted to time spent in New Zealand. That is fine; John Campbell has unearthed much new material to justify this. But the book is also parochial in that everytime Rutherford encounters a New Zealander abroad the narrative takes a detour with a mini-biography.  

**Rutherford** presents uncritically Rutherford's view of the way science develops and of its role in society: pure science leads to truth that can then be applied. Hence more funding ought to be allocated to pure science. Maybe this third ‘sin’ will even discourage *Rutherford’s* deployment as a history of science textbook. Campbell seems to be unaware of the secondary literature in the history of science. He does not seem to have read T. J. Trenn’s classic analysis of Rutherford’s collaboration with Soddy; nor does he seem to be aware of Lawrence Badash's publications, such as the edition of Rutherford’s correspondence with the Yale chemist Bertram Boltwood. 2 It could at the very least have provided Campbell with further anecdotal nuggets. Campbell does not engage with Jeff Hughes’s analyses of both theory and instrumentation in the Cavendish Laboratory under Rutherford’s regime. 3 He also skirts over Rutherford’s early development of a contested radiation detection technique assuming that Rutherford’s evidence was self-evidently true. 4

These are petty historiographical disagreements. More significantly, professional historians of science can also use this book for pleasure or by mining it for telling details. One example: for many years I have wondered about the origin of Rutherford’s use of the graphical method, a technique that sets him apart from Cambridge University’s wrangler mathematics with its disdain of images. Much of Rutherford’s success in the study of radioactivity depended upon his deft application of the graphical method. I had previously speculated that Rutherford had learned telegraphy in colonial New Zealand, and that telegraphy engineers utilized the graphical method. Campbell makes it clear that Professor Bickerton at Christchurch College, who knew little physics, engineering, or telegraphy, inculcated his students with the utility of the graphical method. Only someone with a thorough knowledge of the New Zealand scene could have provided me with the answer to such an esoteric question.  **Rutherford** is a nice book.

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1 Thaddeus J. Trenn, *The Self-Splitting Atom: The History of the Rutherford–Soddy Collaboration* (London: Taylor & Francis, 1977).
2 Lawrence Badash (ed.), *Rutherford and Boltwood: Letters on Radioactivity* (New Haven: Yale University Press, 1969).
3 Jeff Hughes, ‘Plasticine and valves: industry, instrumentation and the emergence of nuclear physics’, *Invisible Industrialist: Manufactures and the Production of Knowledge*, edited by Jean-Paul Gaudillière and Ilana Löwy (Houndsmill: Macmillan), 58–101; ‘“Modernists with a vengeance”: Changing cultures of theory in nuclear science, 1920–1930’, *Studies in the History and Philosophy of Modern Physics*, 29 (1998), 339–67.
4 Arne Hessenbruch, ‘Rutherford’s 1901 experiment on radiation energy and his creation of a stable detector’, *Archives for the History of the Exact Sciences*, 54 (2000), 403–20.

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**Physical Sciences**

**Iwan Rhys Morus, Frankenstein’s Children: Electricity, Exhibition, and Experiment in Early-Nineteenth-Century London. Princeton: Princeton University Press, 1998. xiv + 324 pp. US$52.50/£35.00 (cloth), ISBN 0-691-05952-7.**

This book seeks to understand the study of electrical phenomena in London during the first half of the nineteenth century. There are a number of problems with the way Morus approaches this task. First, there is an unstated assumption that electricity was the same phenomenon whatever the social context in which it was studied. We have therefore the rather curious sight of someone claiming to be an adherent of social constructionism, assuming the objectivity of a ‘natural’ phenomenon. Thus electricity is looked at from the point of view of
what Morus terms, somewhat ahistorically and without justification, fashionable science, popular science, practical science and the uses to which it could be put in communications and medicine. Second, it is not entirely clear why Morus chose to concentrate on London. In this period at least electricity could be studied in a wide variety of geographical locations. For example Joseph Henry in upstate New York was able to independently discover electromagnetic induction. The large resources available in London were not necessary for the study of electricity (useful though they were) and Morus seems to be completely oblivious of this. There is thus a serious failure to place work in London in a broad international context in which it could be properly understood.

But these problems are merely effects of the fundamental flaw with the book which is an over close adherence to currently fashionable notions. This has resulted in some bizarre interpretations of the evidence discussed and the omission of some evidence which does not easily fit the theory. Furthermore texts, such as Shapin’s and Schaffer’s *Leviathan and the Air-Pump*, are used in discussing the nineteenth century as if it was unproblematical whether the lessons so derived from them for the seventeenth century were applicable. They may or may not be, but the question should have at least been asked.

To take one example, Morus makes a good deal of the notion that there were two groups (one elite, the other practical) of electrical practitioners and that there was an impermeable barrier between them which Morus predicates on their respective social locations. A consequence of this, Morus argues, is that the practical group were transparent in presenting their experimental demonstrations, while the elite blackboxed their work. By paying proper attention to the evidence, Morus would have found that both these propositions were oversimplifications to say the least. One of the features of the ‘elite’ Royal Institution was that people were frequently invited to witness experiments in the laboratory and of this Morus seems unaware.

In addition Morus makes a large number of purely factual errors: Faraday was not hired at the Royal Institution as Davy’s assistant (p. 16 and p. 23); Faraday was paid 21s per week, not 25s (p. 17); Davy did not resign at the Royal Institution to go to the Continent (p. 18); Faraday was never offered a knighthood and therefore could not decline it (p. 32); Faraday’s first series of ‘Experimental Researches in Electricity’ was not so numbered (p. 36); Faraday did not spend his childhood in the East of London, but the West (p. 43); the East India Company’s Military Academy at Addiscombe was not Royal (p. 46); Sturgeon went to Manchester in 1840, not 1838 (p. 46); Joseph Banks was President of the Royal Society for forty-two years not twenty (p. 99) and so on.

Such careless errors together with Morus’ contentious arguments, make this an unsatisfactory book, the more so coming from a supposedly reputable university press.

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Technology and Engineering

PETER GALISON and ALEX ROLAND, editors, *Atmospheric Flight in the Twentieth Century* (Archimedes New Studies in the History and Philosophy of Science and Technology, Volume 3). Dordrecht: Kluwer Academic Publishers, 2000. xvi + 383 pp. Illustrations. Index. US$180.00/£112.00 (hbk), ISBN 0-7923-6037-0; US$49.00/£34.00 (pbk), ISBN 0-7923-6742-1.

The editors of this collection of fourteen essays, Harvard University’s Peter Galison and Duke University’s Alex Roland, call the development of flight a ‘defining technology’ of the twentieth century, one that has transformed society and shaped the way people understand their relationship to the physical world. As we come closer and closer to the centennial anniversary of the Wright brothers’ historic achievement of 17 December 1903, to be celebrated internationally in 2003, the truth of this label will be made ever clearer by scholars and aviation enthusiasts all around the world. In how many ways has the flying machine changed us? As French pilot and author Antoine de Saint-Exupery wrote in 1939, it has ‘unveiled the true face of the Earth’. It has brought people together. It has changed our economy. It has added an unprecedented new dimension to warfare. It has affected government, public adminis-
tration, international relations, international policies, manufacturing, marketing, mining, cities, real estate, media, railroads, ocean shipping, agriculture, forestry, and much more. The flying machine has also affected the family, religion, health, recreation, education, crime, and even sex. And, as with everything else, it has not been all to the 'good'.

The concept of a 'defining technology' (first developed by J. David Bolter in his influential 1984 book *Turing's Man: Western Culture in the Computer Age*) is a useful one to keep in mind as we approach the Wright centennial. It is one that should inspire scholars to take both a closer look at, and a wider view of, the deeper socio-political, cultural, and intellectual meanings of flight as it evolved over the past one hundred years. The incredible progress of aviation from Kitty Hawk to the present gives us little reason to doubt that aviation has been one of the quintessential forces of our age. Certainly it provided many of the 'defining moments': Blériot's crossing of the English Channel in 1909; Lindbergh's non-stop solo flight over the Atlantic in 1927; the B-29 Enola Gay dropping the atomic bomb on Hiroshima in 1945; X-1's breaking the mythical sound barrier in 1947; the spacecraft Eagle with astronauts Armstrong and Aldrin aboard landing on the Moon in 1969. None of the essays in this impressive volume (not as impressive as its exorbitant price tag, however) specifically detail any of these paramount moments, but their authors do shed light on a number of critical ways in which aviation came to define twentieth-century Western society.

This volume is essentially the proceedings of a meeting held in April 1997 at the Dibner Institute for the History of Science and Technology, on the campus of the Massachusetts Institute of Technology (MIT). The meeting's inspiration came from Alex Roland, an historian of technology with considerable expertise in aerospace history. His idea was to stimulate a discussion about the history of flight but to convene a group of scholars that went beyond the conventional experts. Such an unusual summit might provoke interdisciplinary insights that specialists in aeronautical history had been missing. For his co-chair, Roland picked Peter Galison, a distinguished historian of science. Galison had never written about flight, which was what Roland wanted, but as a licensed private pilot he was naturally interested in the subject.

Roland's idea panned out, but in unexpected ways. Of the fifteen speakers at the conference (one paper was co-authored), only four actually qualified as coming from outside the scholarly mainstream involved with the history of flight. Leading the 'outsider' contingent was Galison himself; his essay, 'An Accident of History', opened the conference with a philosophical and historical analysis of society's treatment of aircraft accidents and its assessing of responsibility. But joining him were only three other alleged outsiders: Frederick Suppe, a University of Maryland philosopher of science who explored 'The Changing Nature of Flight and Ground Test Instrumentation and Data: 1940–1969'; George E. Smith, a Tufts University philosopher of science whose paper (co-authored with David Mindell, a young MIT historian of technology) concerned 'The Emergence of the Turboprop Engine'; and David Bloor, a Scottish sociologist of science, who presented the conference's closing discussion paper. But like Galison, two of these three were not truly outsiders to the aviation enterprise. Suppe spent the early part of his career with General Electric involved in the computerization of flight-test instrumentation and data analysis, and Smith worked with General Electric and Pratt Whitney during the 1950s and 1960s in jet engine design.

Still, this quartet definitely represented a minority. All the other papers at the conference were presented by scholars whose body of work belongs to the mainstream of academic aeronautics history. Their contributions at Dibner and elsewhere testify not so much to the special interdisciplinary insights that can be provided by outsiders but to the increasingly solid and insightful analysis that has been brought to bear on aeronautics history by leading scholars working very much inside the field over the past twenty years and more: Peter L. Jakab on early flight research at the US army's McCook Field; Walter G. Vincenti on the design of wings at supersonic speeds, 1946–8; Eric Schatzberg on the transition from wooden to metal aircraft; Roger E. Bilstein on the early transference of aeronautical technology from Europe to America; Takehiko Hashimoto on the wind tunnel and the emergence of aeronautical research in Britain; John D. Anderson Jr on whether the evolution of aerodynamics in the twentieth century represented a development in science or in engineering; Robert G. Ferguson on the exchange of engineering knowledge within the US airframe manufacturing industry; Tom D. Crouch on the impact of the Wright patent suits on the growth of American aeronautics; Deborah G. Douglas on what went into the design of the 'modern' American
commercial airport; and Alex Roland on the role of patents in the development of American aeronautics. True, the Dibner programme included papers by two aeronautic engineers—Anderson and Vincenti—but neither qualifies as an outsider given their active participation in the serious academic study of aeronautical history over the past two decades. Given the great overall strength of the work presented by the insiders, one might wonder about Roland’s premise for the conference in the first place. On the other hand, there can be no doubt that non-specialists have much to offer to aeronautical history ‘in the wider view’. Perhaps a better idea, then, might have been to solicit contributions from many more outsiders, and have these invitees give all the talks, with insiders making up the audience and finding inspiration in the different approaches and novel ideas.

As an artefact in itself, the most interesting aspect of this conference volume concerns what the outsiders had to say about the insider presentations. This can be gleaned chiefly in sociologist David Bloor’s discussion paper, which closed the meeting with a careful and thought-provoking review of all the talks presented. Early on, Bloor notes that ‘most contributors have chosen to keep a low profile when it comes to questions of methodology or theory.’ But instead of criticizing the empirical approach of the historians, Bloor calls it a ‘wise strategy’, because ‘Concrete examples often speak louder than explicit theorizing.’ Still, given his own expertise in such theorizing and rigorous analysis of method, it was natural for Bloor in his commentary to explore ‘the more significant sociological themes that are explicit, or implicit, in what has been said.’ Also not surprisingly, Bloor communicates a bias in favour of papers that examine relationships between ‘the social and the technical’ over those looking more strictly at the inner workings of the technologies themselves. Readers may find it ironic that three of the papers identified by Bloor for being so narrowly focused were those authored by Smith and Suppe, two of the outsiders, and by Vincenti and Anderson, the two engineers-turned-historians in the group. Clearly, the work of greatest interest to Bloor came from the historians of technology speaking at the Dibner meeting. In his view it was they who most clearly offered studies that were ‘rich and many-faceted’ enough to prompt ‘major, and exciting, revisions in the history of culture—as well as in the theory of scientific and technical knowledge.’

In other words, what Bloor seems personally to have gotten out of the Dibner meeting turned Roland’s initiating premise right on its head. Instead of outsiders shedding new light on the history of aeronautics, what impressed Bloor the most was the extent to which the presentations made by the mainstream historians of aeronautical technology offered sociologists and the philosophers of science and technology important new ways of coming to terms with major issues within their own disciplines.

Throughout the presentations one of the most interesting issues addressed the question of whether or not flight has functioned in its multivarious historical contexts in the same way other technologies have functioned in theirs. No simple yes or no answer sprung from the meeting, but a number of provocative insights took shape along the way. If there was a thesis to the entire proceedings, it was that the historiography of flight remains too parochial and nationalistic. As we approach the centennial anniversary of powered flight, much more still needs to be done to enhance our understanding of flight’s development in comparative and international perspective.

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ERIC HIGGS, ANDREW LIGHT AND DAVID STRONG, editors, Technology and the Good Life? Chicago: University of Chicago Press, 2000. 392 pp. US$65.00 (cloth), ISBN 0-226-33386-8; US$25.00 (pbk), ISBN 0-226-33387-6.

Albert Borgmann is clearly one of the foremost American philosophers of technology. His book, Technology and the Character of Contemporary Life (TCCL) (University of Chicago Press, 1984) began what has turned out to be a trilogy of influential books on the impact and importance of technology upon daily life. It was followed by Crossing the Postmodern Divide (University of Chicago Press, 1992) and most recently by Holding onto Reality: The Nature of Information at the Turn of the Millennium (University of Chicago Press, 1999).
This book, Technology and the Good Life? was conceived when two of its editors realized in 1994 that it had been a decade since TCCL had been published, and they decided to form a conference on Borgmann’s work and, in particular, upon the distinction between what Borgmann called the ‘device paradigm’ and ‘focal things’ or actions, the distinction which had drawn the most discussion and controversy. Perhaps fortunately, by the time the conference was held in 1995 and the publication date, the two later books could also be discussed in this collection as well. Originally invited to the conference, but prevented by timing, I feel fortunate that now, in this review, I can enter the discussion at least indirectly.

The book consists of seventeen chapters presumably focused upon the noted distinction, but fortunately for readers also upon the role of technology beyond the normative questions that lie embedded in the device/focal difference. The authors range from students and devotees of Borgmann, outwards to appreciators and even critics, thus the work is not simply adulation and application. In the original TCCL setting, two versions of household heating technologies set the stage for the distinction. Automated heating systems are background technologies—they are hidden, perform their work without or with minimal intervention, remain beyond the direct actions of humans and are a type of consumer object. In contrast, a wood burning stove calls for the preparation of the wood, the direct maintaining of its heating (and cooking) capacities, and thus engages the family with different individuals playing different but focal roles. It ‘gathers’ its family and is a focal thing that calls for focal activities.

Those who have read the works of Martin Heidegger will recognize a related but updated version of this approach to technologies and Borgmann is clearly identified with this tradition of approach to technology. But Borgmann is more flexible, more pluralistic, and more democratic than his classical predecessor. Yet, his emphasis upon family, church, disciplined, morality, and, yes, simpler technologies remains within kinship lines. His respondents take a variety of positions with respect to this lineage: David Strong and Eric Higgs perhaps remain the closest to Borgmann, extending his notions into roles related to the preservation of wilderness in the form of national parks and the Crazy Mountains forest regions. Others attempt to extend, even stretch, the notion of the focal into much more complex but basic areas—Paul Thompson’s discussion of the complex of farming is one such re-working: Jesse Tatum takes the extension into the area of design; and Phillip Fandonzi deliberately takes the notion of focal things into cinema, a technology more likely to be taken as a device than most. These applications and extensions also hold a mild critique that addresses the ambiguity inherent in a binary difference.

Then there are essays as well from authors who come from non-heideggerian traditions. Larry Hickman introduces an alternative scheme from pragmatism, in particular from John Dewey, which nuances the distinction and pushes it towards more social phenomena. Two critical theorists (Frankfurt School), Doug kellner and Andrew feenberg, are more critical. Both see much more positive roles in contemporary technologies. Kellner through cyberspace phenomena such as the internet and media, Feenberg sees the social modification for uninten-ded but more democratic developments of what are clearly initially ‘devices’ in both some manufacturing processes, but also the French Teletel system. Here what originally appear to be industrially or militarly founded technologies, become through adaptation to users, more human friendly and democratic. Similarly, Diane Michelfelder shows that women’s uses of telephones play positive social roles in the maintenance and upkeep of care within communities.

Thus it turns out that what seems a narrow focus upon the device/focal distinction serves a much wider purpose. Had I been at the conference, I would have argued that technologies are multistable; that each holds the possibilities of multiple trajectories, not so much dependent upon either individual or small group innovations, but upon the practices that grow out of uses as the humans use, play, and displace the original technologies. Yet, if one result is a softening of the device/focal distinction, another is the holding consensus that Borgmann plays something of the role of technological conscience for many of us. Few philosophers display so deep a passion to seek and to find the good life. Technology and the Good Life? is a good read within this changeable field.

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Instruments and Measurement

CHRISTOPH MEINEL, editor, Instrument – Experiment: Historische Studien. Berlin: Verlag für Geschichte der Naturwissenschaften und Technik, 2000. 423 pp. 66.50 DM. ISBN 3-928186-51-5.

This book presents a collection of some thirty-seven papers in German and English, revised from the 1997 meeting of the Deutsche Gesellschaft für Geschichte der Medizin, Naturwissenschaft und Technik in Regensburg. Its sections cover a wide spectrum. The editor has refrained from organizing this volume according to discipline, preferring rather to look at instruments as ‘material intermediaries between scientific cultures’, and attributing an active, shaping role to them in social and cultural practice, beyond the bare function of transmitting scientific results. The book begins with articles on historiography and methodology, followed by sections on the function of instruments in producing reality, on the establishment of new experimental procedures, on the relation between the instrumental mediation of vision and scientific knowledge, on the social practice of instrumentation, and on the materiality of instruments.

This volume’s rich and disparate contributions offer a valuable introduction to a large and lively field of science studies. Clearly, Ian Hacking’s famous dictum that ‘Experimentation has a life of its own’ can now be extended to the history of experimentation itself, which has come to life, evidenced in the proliferation of new catchwords; for example, ‘new experimentalism’, ‘practices of science’, ‘material culture’, ‘microhistory’, ‘laboratory studies’, ‘replication’, and ‘technoscience’.

The survey article by Klaus Hentschel succeeds in bringing some order to the often confusing uses of these terms, clarifying debates, and pointing out the limitations of various approaches. But Hentschel is more ambitious: he intends to go beyond simple survey, and uses this occasion to advocate a programme for future studies in the history and philosophy of experimental science and practice. The impetus is laudable and reflects a topic currently all too familiar in discussions among historians of science: how are we going to get beyond detailed microstudies to achieve a more coherent and general picture of experimental science? In short, where is the big picture? Hentschel is looking for patterns emerging from a multitude of existing microstudies. Order, he argues, requires terminological unification and taxonomies of criteria (for types of arguments and what constitutes a good experiment) in the first place. Only a ‘grid of terminological distinctions’ will allow for comparability among case studies and make the search for ‘typical sequences’ possible (pp. 30–31). As he admits, however, there is still a long way to go.

Hentschel regrets that popular chronicles of science currently do not take into account recent work in the history of experiments and instruments. This also means that the results of studies of experiments do not lend themselves to the kind of concrete simplification required by that genre. The question remains: are microstudies fuzzy because they go into so much detail or are the very objects of observation fuzzy? In short, is it the historians of science or the real world that produce blur?

In the second article of this collection Hans-Jörg Rheinberger argues for the latter. Doing experiments in itself, he says, seems to be a messy enterprise. The historian is thus only reproducing chaotic patterns of scientific exploration. Rheinberger’s article provides a nice contrast to Hentschel’s; historians’ attention to detail can indeed lead to very different results. Where Rheinberger sees happy chaos, playfulness, tinkering, and ‘wild thinking’, out of which some kind of order emerges over time, Hentschel sees systematic patterns beyond the chaos that first meets the eye. For Rheinberger, microstudies do reveal the essential character of experimental work—flux and fuzziness—whereas for Hentschel they merely provide building blocks with the necessary degree of fine-grained resolution to build a general descriptive edifice of experimental work.

How to interpret these very different propositions? Do the two authors come to their interpretations because they each think they have captured something real and universal, or is it because they in fact deal with different objects? Here it may after all prove necessary, I fear, to talk about disciplines and the ways they constitute their research objects, as it is striking how far both Hentschel and Rheinberger build upon the metaphors and values of their respective disciplines of study, physics and biology.
Historian of biology Rheinberger describes ‘fertilisation’ between tinkering and logic. He proposes some playful processes of ‘selection’ from among a multitude of choices, leaving some space for randomness. (Thus he argues that instrumental accuracy often far surpasses the precision of the objects under consideration.) His big picture is clearly an evolutionary one, what he calls a ‘non-trivial interplay in time and space, in the course of which [epistemic objects and experimental conditions] shift among themselves and can exchange roles’ (pp. 53–54).

Historian of physics Hentschel, in contrast, hopes for real, or at least persistent, patterns that inform scientific investigation. Hentschel’s approach is not unlike that of the particle physicists who look for the ultimate composition of matter. For him experimental science is composed of ‘building blocks’ and there is a possibility of using accurate descriptions to ‘screen’ experimental investigation.

Nevertheless, even if both are informed and influenced by their respective objects of research, does this exclude the search for common ground among all disciplines in studies on experimental practice a priori? The two introductory articles provide a useful lens—or epistemic object, perhaps—through which to read the remaining thirty-five articles.

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Earth Sciences

NAOMI ORESKES, The Rejection of Continental Drift: Theory and Method in American Earth Science. New York and Oxford: Oxford University Press, 1999. x + 420 pp. £49.50/US$55.00 (cloth), ISBN 0-19-511732-8; £21.50/US$29.95 (pbk), ISBN 0-19-511733-6.

The history of the revolution in the earth sciences with the advent of plate tectonics has been documented extensively; examples include Anthony Hallam’s A Revolution in the Earth Sciences: From Continental Drift to Plate Tectonics (Oxford University Press, 1973) and William Glen’s The Road to Jaramillo: Critical Years of the Revolution in Earth Science (Stanford University Press, 1982). Histories and textbooks have of course included the episode in the early part of the twentieth century when American earth scientists rejected the theory of continental drift formulated by Alfred Wegener (1880–1930). Naomi Oreskes’s fascinating new book, The Rejection of Continental Drift, is the first to examine closely just why these Americans so vehemently rejected the idea of continental drift. Carefully explaining not only the history but also the science under discussion, she makes a compelling case that the reasons were not so much for a supposed lack of an adequate mechanism for a mobile crust. The clash of ideas about how to ‘do science’ was equally important, hence the book’s subtitle Theory and Method in American Earth Science. Her experience both as a working geologist and a trained historian shows clearly, as this book combines the best of both worlds.

It is impossible to do this book justice in a short review. The list of published sources in the extensive bibliography (pp. 371–403) is prodigious, but the wealth of detail gleaned from the author’s delving into sixteen archival sources is this book’s real strength. The archival sources include the papers of, among others, Charles Schuchert (1858–1942), William Bowie (1872–1940), Bailey Willis (1857–1949), John Joly (1857–1933), Reginald A. Daly (1871–1957), and Alexander du Toit (1878–1948), all of whom were prominent in the controversy.

Beginning with an introduction entitled ‘The Instability of Scientific Truth’, Oreskes points out a fact well known to science historians—namely, that scientific ‘truth’ changes with new information—and one that scientists gloss over at their peril. The introduction is also an excellent summary of the entire book and, for this reason, bears repeated reading. The epilogue ‘Utility and Truth’ is another section worth reading more than once.

The book is divided into three parts. Part I (‘Not the Mechanism’) consists of four chapters. The accepted story of the rejection of continental drift has been that there were no plausible mechanisms proposed to allow for a mobile crust. Not so. Chapter 4 is devoted entirely to drift mechanisms under active discussion in the 1920s, particularly the theory of convection currents of Arthur Holmes (1890–1965). By the end of the nineteenth century, many Europeans were comfortable with the idea of a constantly shifting earth, while Americans clearly were not.
Part II (‘Theory and Method’) contains three chapters. Here the contrasting ways of doing science between Americans in general and Wegener in particular are described. One scientific method puts the gathering of data first and derives theories from them; the other begins with theory and then devises experiments to test it, either to verify or falsify. American geologists, largely influenced by the multiple working hypotheses of T. C. Chamberlain (1843–1929), considered theories as ground to be cautiously treaded. In particular, Americans tended to distance themselves rhetorically from their personal theories. Wegener, by contrast, proudly acknowledged his theory of continental drift and said he had come upon it by accident (p. 154)! American geologists were shocked by this; in their view this was simply not the way to do science. The biggest problem, though, was American adherence to an extremely strict understanding of uniformitarianism.

Part III (‘A Revolution in Acceptance’) also contains three chapters. Here is documented the steps whereby the acceptance of continental mobility came to be accepted. Chapter 10 (‘The Depersonalization of Geology’) contains what I consider to be the best statement of the author’s thesis: ‘The rejection of continental drift was an implicit rejection of observer-dependent evidence despite the strength of that evidence. The acceptance of plate tectonics was an affirmation of a depersonalized alternative’ (p. 304).

The Notes (pp. 319–69) supplement wonderfully the book’s meticulous documentation; many of them are as worthy of extended discussion as any part of the main text. The epilogue’s Note 13 is a personal favourite: ‘Yet scientists are notoriously oblivious, if not hostile, to their own history. Science looks to the future, not the past. Why should one be interested in a history of mistakes? As a student of mine once put it, ‘‘Why should we learn old ideas that aren’t even true, anyway?’” Why, indeed’ (p. 369).

I found little to criticize in this book. Illustrations are mostly black-and-white drawings and reproductions from published works, with a few photographs; their quality is sharp overall. Their captions are often detailed, which will help anyone who is not completely familiar with the science. Readers should be cautioned that both the US Coast and Geodetic Survey and the US Geological Survey are referred to informally as ‘the Survey’. However, these very different agencies are mentioned in the same paragraph only once that I noticed (p. 133), so it will be clear which agency is being referred to.

There is also a puzzling omission in the index, where the ‘USGS’ (US Geological Survey) is referred to on only one page. This is misleading at best, because the USGS is referred to many more times throughout the book, almost always as the ‘US Geological Survey’ as opposed to its acronym. Other agencies, notably the US Coast and Geodetic Survey and the Office of Naval Research, are indexed by both their acronyms and their spelled-out titles. It is not clear why this was not also done for the US Geological Survey.

Aside from this small detail, The Rejection of Continental Drift is a monumental work of scholarship. For anyone, scientist or science historian, who is interested in the heretofore scantily documented history of the early twentieth century arguments against the continental drift theory by American earth scientists, I highly recommend this book.

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