ANNALS OF SCIENCE, 60 (October, 2003), 437–458

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Book Reviews

Life Sciences

JOHN C. GREENE, *Debating Darwin: Adventures of a Scholar*. Claremont, CA: Regina Books, 1999. 284 pp. $41.50 (hardcover). ISBN 0-941690-85-7.

How did the Darwin industry get going? That is one thing that future meta-historians of science will be asking. One of the places they will rummage will be in the works of John C. Greene. Greene, along with Michael Ghiselin, is one of the venerable first-generation Darwin scholars of the last century. Ghiselin, a trained biologist, looked at Darwin through the eyes of a practising population-biologist and 'rationally reconstructed' history in the light of the neo-Darwinist programme. Greene, a historian, moved within circles of the 'history of ideas'. Both made their marks on the study of evolutionism. Ghiselin's reconstruction of Darwin is still very controversial and provokes grand, often scholastic, philosophical disputes about the meaning of the Darwinian revolution and its entities. Greene inspired a whole generation of meticulous social historians, all aiming to put Darwin in his place (socially speaking). These progeny have been fruitful. Yet, Greene's own work has been eclipsed by their success. Such is the fate of the 'history of ideas' programme.

This book—consisting of a motley collection of odds, open ends, reminiscences, papers, and correspondence—traces those disputes in the history of the idea of evolution. Surprisingly, Greene is at odds with many of his progeny. For one thing, he thinks that the recent social turn is a wrong one—that it leaves out the very ideas around which social disputes revolve. For Greene, ideas and contexts are closely related, but not reducible one to the other.

In the first, autobiographical, chapter Greene shows this interrelationship in his own development. He reveals that he stumbled on his Darwin scholarship accidentally, carrying with him a weighty theological axe to grind. Greene, a devout Christian, began his career in the immediate pre-World War II period working on a doctoral thesis examining the pre-Darwinian controversies over geology and the Bible in America. Geology, as historians of ideas might say, 'set the stage' for the Darwinian controversy and Greene quickly progressed on to more unsettling matters. The result of his studies—his highly influential *The Death of Adam: Evolution and its Impact on Western Thought*—had the fortune to be published in the centenary year of the publication of Darwin's *Origin*. Greene's career as a Darwin scholar was launched.

From the very beginning, Greene staked his claim on what several historians and philosophers of science have since dubbed the 'received view': that the thrust of science is a 'value-free' examination of nature, its claims subject to independent, cold objective, 'scientific verification'. Some have called this the 'positivist' view, but Romanticists, hermeneuticists, Frankforters, dualists, existentialists and positivists alike shared it equally. For this school, the triumph of science is the triumph of 'value freedom'. As such, it appears difficult to have a science of personal experience and value, 'of the heart', to use Greene's term (p. 10). Max Weber presented us with our most lasting and unfortunate slogan about the 'disenchantment of nature'. For Greene, the triumph of Darwinism marked just such a disenchantment, a fall from grace. However, here Darwinism had wandered far beyond the limits of science. Greene discovered that there was much more about Darwinism than pure value-free discovery. Indeed, Darwin was a product of his time—a time of Victorian secularism and scientism. ('Scientism' is Greene's favourite term of abuse.) Thus Darwinism was a value-laden world view—laden with materialism and sentiments about 'progress' and 'creation'. However, for Greene, this meant that Darwinism had overstepped its ground. Neo-Darwinists were treading into the territory of religion and ethics—places where they do not belong. Here Greene had his critical cake and ate it too. Darwinism was denounced as an alienating, disenchanting, science and as a metaphysical world view masquerading as a legitimate science.

Debating Darwin includes interesting excerpts from a long correspondence between Greene

Annals of Science ISSN 0003-3790 print/ISSN 1464-505X online © 2003 Taylor & Francis Ltd
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and the patriarchs of neo-Darwinism, Theodosius Dobzhansky and Ernst Mayr, on the nature of Darwinism. Dobzhansky and Mayr were trained in that high-humanist style of European and Russian morphology, where metaphysics and fact meet in the same object. Greene's analysis of Darwin and neo-Darwinism puzzled Dobzhansky, who was both a Christian and a scientific progressivist and suffered no twinges of contradiction. Mayr is at great pains to show that culture and progressive movement can be explained naturally, by evolution. Greene was at pains to show the distinction—that evolution could not be progressive. Progress was a metaphysical and ethical judgement.

We might now judge Greene to be right about progress, but for all the wrong reasons. Evolution is not 'progressive', but not because science is unconcerned with metaphysics and progress is a metaphysical doctrine. Greene's students have shown how impossible it is to disentangle the metaphysical from the science. Following suit, philosophers have claimed science to be 'value laden' all the way down. Greene hesitates at his followers' total reduction of ideas to 'external' social forces. Science is about ideas too, he tells us. However, this works only if we refuse to take the received view of science, with its unfathomable dichotomy between science and the 'other', too seriously.

GORDON MCOUAT, *History of Science and Technology Programme and Contemporary Studies Programme, University of King's College, Halifax, Nova Scotia, Canada B3H 2A1*

Medicine and Health

LUCY HARTLEY, *Physiognomy and the Meaning of Expression in Nineteenth-Century Culture*. Cambridge Studies in Nineteenth-Century Literature and Culture 29. Cambridge: Cambridge University Press, 2001. xii + 242 pp. 20 plates. $54.95 (hardcover). ISBN 0-521-79272-X.

Lucy Hartley states that her study 'explores changing understandings of expression, primarily the expression of the emotions, and principally via the face, from the English publication of Lavater's *Essays on Physiognomy* (1789) to the publication of Francis Galton's *Hereditary Genius* (1892)'. That understanding constituted the nineteenth-century tradition of physiognomy, and Hartley explores its development through the writings of various theorists on physiognomic themes. The figures discussed include (for the earlier period) Charles Le Brun, David Hartley, and Johann Caspar Lavater, and for the Victorian era proper Charles Bell, Robert Haydon, Herbert Spencer, Alexander Walker, John MacVicar, Charles Darwin, and Frances Galton. Hartley also pursues physiognomic influence down fascinating side paths of Victorian culture, including the revolt of the Pre-Raphaelite Brotherhood against the Academy-based genre of history painting, the 'sensation fiction' of novelist Wilkie Collins, and various speculations on the nature of female beauty.

Despite the impressive collection of theorists whose views on physiognomy are sampled, Hartley is not out to analyse the physiognomic tradition systematically or to delineate its precise boundaries and contours. Indeed, her broad definition of physiognomy as 'a[ny] means of describing character through expression' characterizes the tradition too loosely to encourage such an approach. What emerges from her study is a series of fascinating if episodic glimpses into physiognomic thinking during the Victorian period, rather than an actual history of ideas about physiognomy or a social/cultural history of their role in the Victorian consciousness. Typical of this approach is the book's handling of physiognomy's relationship to other fields of Victorian natural philosophy. Hartley, like many commentators of the early nineteenth century, is intrigued by the claims made on behalf of physiognomy after Lavater to be a 'science'. Her introduction raises the notion of physiognomy as another kind of boundary science, comparable with phrenology or mesmerism, and she even speculates about its later role in stimulating the growth of psychology in England. However, subsequent chapters offer no general discussion of physiognomy's relationship to these other enterprises. Darwin's speculations on 'the expression of emotion' are examined in detail, for example, but the book has little to say about alleged physiognomic indications of race, criminality, or insanity, crucial as these were for nineteenth-century scientific and medical thought.

Some of the ground covered by this book has already been ploughed by historians and scholars of literature and the arts, but Hartley brings to the material a particular thesis of her own. Physiognomy, she contends, presupposed an essentialist view of human nature that linked it closely to natural theology, a link that she explicates very clearly in the writings of

Johann Caspar Lavater and physiologist Charles Bell. However, later, Hartley argues, Darwin's naturalistic and evolutionary interpretations of physiognomic manifestations, and even more Francis Galton's insistence that human nature is susceptible to control and shaping through eugenics, undermined those essentialist assumptions and so led to the dissolution of popular interest in physiognomy. In the end, however, the book provides little evidence to show that Victorian interest in physiognomy actually did succumb to evolutionist critiques of its essentialist assumptions, nor does it indicate how such shifting levels of interest might be assessed or measured.

The scholarly conventions at work in this book correspond more closely to those of literature studies than to those of either history or cultural studies *per se*. Historians may find the descriptive material too spare or incomplete for their disciplinary taste, and think some of the theses to have been developed more by indication that by in-depth argumentation. Cultural studies specialists will find the theoretical framework thin or unconvincing. In sum, this is an interesting and intelligent essay on physiognomy in Victorian culture, but one decidedly uncertain about its audience and the story it wants to tell, and in need of some friendly but critical editing to help it to realize its full potential.

R. Steven Turner, *Department of History, University of New Brunswick, Fredericton, New Brunswick, Canada E3B 5A3*

Mathematics and Logic

Laurent Schwartz, *A Mathematician Grappling with his Century*. Translated from the French by Leila Schneps. Basel: Birkhäuser, 2001. viii + 490 pp. DM 76. ISBN 3-7643-6052-6.

This book is a number of books in one, just as the author has devoted himself to a number of independent activities: mathematics, educational reform, politics, human rights, languages, bird songs, butterflies, and biology in general. The reviewer knows nothing about butterflies and (certainly compared with the author) very little about all the other activities except languages; however, judging by the book, the author's creative and independent intellect has led to solid achievements in every area. The results are always interesting, even to a reader whose basic outlook is quite different from that of the author.

These manifold activities (which in deference to the author's nationality should perhaps be better described as a variety of activities) make for very different styles in the different chapters, each appropriate to its subject matter. The first chapter, which describes the author's childhood visits to the family estate Les Closeaux ('the small gardens'—the translator has put in parentheses its literal meaning, 'the closed waters'), is written in a leisurely, rambling style. To someone like the reviewer, who has never been to France, the author's description of the natural beauty and joy of this place bears an uncanny resemblance to Proust's description of his Combray, which is based on the village of Illiers and includes some features of Auteuil, the Parisian suburb where Proust lived. The author tells us that he used to ride from Paris to Autouillet, which the map shows to be quite close to Illiers; so perhaps the resemblance is not simply a coincidence. Of course, it is a virtual certainty that the author has himself read Proust and may be unconsciously imitating him. In any case, the style in the introductory chapter definitely seems Proustian to the reviewer.

This style extends to the first chapter, which details the author's education, and in which he reveals matter-of-factly that Jacques Hadamard was his great-uncle by marriage. The second chapter discusses the author's early work at the Ecole Normale Supérieure (ENS) and his courtship, which was severely threatened by his fiancée's tuberculosis, from which she eventually made a full recovery. His class at the ENS (1934) produced three outstanding scientists (Choquet, Schwartz, and Blanc-Lapierre) who became members of the Academy.

After these interesting, but not unusual reminiscences, the author shifts his focus in Chapter III to discuss his early days as a Trotskyist, having turned his back on the conservative ideals of his family. He tells us that it was the sudden confrontation of the abyss between the reality of World War I and what he had learned from French propaganda that led to this break. Here, as throughout the entire narrative, whether he is discussing French–German politics, the end of the French colonial empire, the American war in Viet Nam, the problems he and other French mathematicians had getting American visas, or the defence of persecuted intellectuals in the USSR and South America, the author exhibits a firm, principled stand,

motivated entirely by the desire for human freedom and dignity. Although his pride in the achievements of French intellectuals and his patriotism show through clearly, he does not, and did not, spare his criticism of his own country. Also, although he has been equally critical of some of the dubious enterprises of USA and the USSR, in neither case does he exhibit any rancour or emotionally based anti-American or anti-Soviet sentiment. He tried very hard to get some action to put an end to the genocide in Cambodia during the 1970s (as did a few political activists of both the right and the left in other countries). These political chapters make for exciting reading; they are not Proustian, but are written more in the style of the faster-moving chapters of *Les Misérables*.

Of course, most readers will buy this book to learn how the author made the discoveries that earned him worldwide fame and (along with Atle Selberg) the 1950 Fields Medal. They will not be disappointed. He gives a nice discussion of the distinction between mathematical discovery and mathematical invention. The ideal world of mathematics is inhabited by ideas that mathematicians have created (invented); however, it is a world ruled by logic, and once an object has been invented, the development of its properties is no longer a matter for invention, but rather for discovery. The author says that he had a long-standing habit of lying awake in his bed at night, all lights off, just thinking about mathematics. (Here come those echoes of Proust again.) The idea of using operators as generalized derivatives came to him one night in November 1944, a night he describes as 'the most beautiful night of my life'. The exposition here is clear and elegant, and a real pleasure to read.

The theory of distributions brought the author worldwide recognition and constant invitations to speak in public. He tells us that, after a while he began to hate the repetitiveness of these addresses, and would be constantly chanting to himself, 'God, he's boring!' while lecturing. Because of the visa problems occasioned by his Trotskyist views (overcome when he received the Fields Medal in Massachusetts by the personal intervention of President Truman), the author has spent comparatively little time in the USA. He remarks that most Americans do not like to read French, and hence that they read none of his works except the *Distributions*. This statement may possibly be true, although it does not ring true to the reviewer. What is true is that American mathematical research was indebted to Germany much more than France in the half-century following the American Civil War, so that German tended to be the foreign language best known to American mathematicians.

The author has known close up many of the galaxy of French mathematical stars during the twentieth century—Hadamard, Chevalley, Weil, Dieudonné, Cartan, Choquet, and Grothendieck (his student). These people appear from time to time in the narrative, affording the reader a glimpse of their personalities and adding to the interest of the book.

The author has not seen the French original, but the translation is certainly competent. Only a few minor points might be noted. For example, 'voices' in a legislative body are usually called 'votes' in English (p. 87). On page 42 the word 'finalist' should be 'teleological'. The 'fine theory of sets' should be 'descriptive set theory' (p. 64), and, on the same page, 'maximum of the module' should be 'maximum modulus'. Only one aspect of the translation is annoying, however, and that is the constant use of plural verbs whose subject is 'mathematics'. In English 'mathematics' is *singular*.

The reviewer has never had the privilege of meeting the author and has known him only through his works, chiefly the theory of distributions (thus perhaps bearing out the author's gibe, although the reviewer does read French literature, both fiction and non-fiction, with pleasure). For others in the reviewer's situation this book offers a generous amount of information about a very remarkable man. It is well worth the purchase price and the time required to read it.

ROGER COOKE, Department of Mathematics, University of Vermont, Burlington, VT 05401-1455, USA

Physical Sciences

LUDWIK KOSTRO, *Einstein and the Ether*. Foreword by MAX JAMMER. Montreal: Apeiron, 2000. iv + 242 pp. $25.00 (paper). ISBN 0-9683689-4-8.

This intriguing book is a study of Einstein's changing views on the existence and nature of an ether. Although it is widely believed that Einstein rejected the ether, Kostro notes that

'Einstein denied the existence of the ether for only 11 years—from 1905–1916. Thereafter, he recognized that his attitude was too radical and even regretted that his works published before 1916 had so definitely and absolutely rejected the existence of the ether' (pp. 1–2). Kostro documents his account with extended quotes translated into English, and provides the original German for all quoted passages in an appendix. I shall summarize Kostro's account of Einstein's changing views; 'E' in references indicates passages written by Einstein.

Einstein's interest in the ether appears in his first known attempt at scientific writing (at age fifteen or sixteen), 'A Study of the State of the Ether in the Magnetic Field', in which he assumes the existence of an elastic ether that carries electromagnetic waves (pp. 12–15). Einstein confirmed his early belief in the ether in his 1922 Kyoto lecture on the genesis of relativity (p. 15). In this lecture Einstein described an experiment to measure the motion of the earth through the ether that he thought of during his student years in Zürich (1896–1900), although he never carried out this experiment, or other such experiments that he designed. During this period he also began to have doubts about the existence of the ether, at least when it is considered a privileged reference frame (pp. 16–18). These doubts came to a head with the special theory of relativity (STR).

In 'On the Electrodynamics of Moving Bodies' Einstein explicitly rejected the ether—but only in the sense of an absolute rest frame (pp. 29–32). He said nothing about a carrier of electromagnetic radiation, and did not mention the ether again until a 1907 paper in which he unified all results of STR thus far. Here he rejected the need for a carrier of electromagnetic radiation on the basis of a new argument: the equivalence of energy and mass shows that energy has inertia and is no more in need of a carrier medium than is any other item that has inertia (p. 33). Two years later Einstein returned to the subject and brought his light-quantum hypothesis into play: 'The electromagnetic fields that constitute light no longer appear as states of a hypothetical medium, but as autonomous forms that are emitted from light sources, just as in Newton's theory of light emission' (E, p. 37). Kostro comments that Einstein probably did not know Newton's full theory of light, which included a role for the ether. Einstein repeated his objections to the ether in other works until 1916. Meanwhile, beginning in 1907, Minkowski introduced his four-dimensional spacetime version of the STR. Initially Einstein was not enthusiastic: 'he found the use of tensors to be "superfluous erudition" ...' (p. 44). By 1912 Einstein began using tensors himself, and by 1916 he fully recognized the importance of Minkowski's work.

The general theory of relativity (GTR) brought Einstein back to a conception of the ether, although he first thought that this theory strengthened his objections because it makes the choice of spatio-temporal coordinates completely arbitrary: 'time and space are deprived of the last trace of objective reality' (E, p. 59). However, in a letter to Lorentz (16 June 1916) Einstein reconsidered. Einstein was responding to a letter (6 June 1916) in which Lorentz attempted to reconcile the stationary ether with the GTR. Einstein rejected this attempt, but indicated that the GTR is compatible with a new kind of ether that is identical with the metric structure of spacetime: 'I agree with you that the general theory of relativity is closer to the ether hypothesis than the special theory. This new ether theory, however, would not violate the principle of relativity, because the state of this $g_{\mu\nu}$=ether would not be that of a rigid body in an independent state of motion, but every state of motion would be a function of position determined by material processes' (E, p. 68). Kostro emphasizes that this was a limited concession: Einstein did not actually endorse this new ether, did not publish this view, and did not mention it in any publication for two years (p. 74). Then in 1918 Weyl published the idea that the metric tensor describes the ether, and Einstein wrote a positive review of the book. Kostro considers the possibility that this book played a role in moving Einstein to present his own version of the ether, but argues that the key event was an attack by Lenard who claimed that Einstein merely renamed the ether 'space' (pp. 74–75). Einstein published his new account of the ether in a response in November 1918. Einstein now argued that while in the STR any portion of space without matter or an electromagnetic field is empty, in the GTR such space has physical properties specified by the metric tensor—which also gives the gravitational potentials. 'This state of affairs can be easily understood by speaking about an ether, whose state varies continuously from point to point. One must only be careful not to attribute to this "ether" the properties of ordinary material bodies (e.g., a well defined velocity at every point)' (E, p. 76).

In January 1920 Einstein wrote a long article for *Nature* on the development of relativity, but *Nature* published only an abstract which did not include two sections on the new ether.

However, the manuscript has been preserved, and Kostro provides the two sections. Here Einstein described his earlier rejection of the ether as 'too radical' (E, p. 78), and sketched the new conception: we may introduce a medium that pervades space, with both matter and electromagnetic fields as states of this medium. However, this medium is not matter as understood in mechanics: it does not consist of individual particles that can be followed over time, and cannot be assigned a state of motion at each point. Still, empty space is now conceived of as having physical properties, and 'the concepts of ''space'' and ''ether'' merge together' (E, p. 78).

Passing over a debate between Einstein and Lenard at a conference in 1920 (pp. 85–88), the next major event is Einstein's Leiden lecture on the ether (17 October 1920). After reviewing the history of the ether from Newton to Lorentz and the STR, Einstein reconsidered the rejection of the ether that seemed appropriate at an earlier stage: 'More careful reflection teaches us, however, that denial of the existence of the ether is not demanded by the special principle of relativity' (E, p. 94). The STR allows for the existence of an ether that has no definite state of motion. Lorentz, Einstein argued, had attributed just one mechanical property to the ether—immobility—and this too must be eliminated. Such an ether might seem to have no physical significance, but this is a mistake because rejecting this ether amounts to denying that empty space has physical properties. Yet we have known that space has physical properties since Newton's discussion of rotation: 'According to Einstein, Newton might just as well have called his empty space the ether, because it possesses real properties that determine the behaviour of bodies' (p. 95). The GTR eliminates any notion that empty space lacks physical properties; its properties are conditioned by surrounding matter, and affect the motion of bodies. Einstein concludes that according to the GTR 'space without ether is unthinkable' (E, p. 98); such a space would eliminate the propagation of light, the rods and clocks needed for measurement, and even spacetime intervals.

Einstein's next publication on the new ether, 'On the Ether', appeared in 1924. Here Einstein stressed the active role of the ether in physical processes. He argues that Newton's absolute space is an ether because it determines the inertial behaviour of bodies, but this is a limited conception because bodies do not act on the ether. The STR is a step forward because its ether also determines the propagation of light and affects the geometry of bodies, but bodies still do not act on the ether. This limitation is overcome in the GTR. The ether now determines both the gravitational and the inertial behaviour of bodies (pp. 103–05). Kostro views this 1924 article as the end of a stage in Einstein's development of a new ether concept. Further developments will be tied to attempts to develop a unified theory of gravitation and electromagnetism.

Kostro interprets the aim of this new project as constructing a theory in which a single ether serves as the medium for both the gravitational and electromagnetic interactions (p. 116). After studying and rejecting attempts at this kind of unification by Eddington, Weyl, and Kaluza, Einstein began his own attempts in 1925. Einstein presented his new ether theory in several technical and popular lectures in 1929 and 1930; some of these lectures were subsequently published (pp. 121–22). Einstein now identified the relativistic ether with physical spacetime; electromagnetic and gravitational fields are treated as states of this continuum, rather than as entities in spacetime: 'Physical space and ether are only different terms for the same thing' (E, p. 123). However, Einstein's terminology is changing. Kostro notes that for some time Einstein had used the terms 'ether', 'physical space', and 'field' as synonyms. The term 'ether' was his preferred term between 1928 and 1934. Then 'physical space' began to predominate, and after 1938 Einstein stopped using 'ether' altogether. The preferred term is now 'total field', but, Kostro argues, Einstein did not lose interest in the underlying ether concept: Einstein 'published new editions of two significant works on the relativistic ether' (p. 150). The first of these was his 1920 Leiden lectures; these were unchanged. The second was a popular article originally published in 1934. In a new edition (1953), Einstein dropped references to a specific early unified field theory. On Kostro's interpretation, the result was that Einstein's identification of the ether and spacetime now referred to all of the proposed unified theories. This completes Kostro's account of Einstein's changing view of the ether. The book's final chapter includes some brief remarks on Einstein's methodological views, some material on contemporary views of the ether, and a review of much of the earlier material.

It is worth reflecting here on the extent to which we are dealing with changing accounts of a specific entity, versus changes in the concept associated with a word. Certainly the

Aristotelian ether—an element out of which celestial objects are made and that exists only in the celestial realm—was rejected along with the two-part universe. However, the term was soon adopted for a quite different purpose: to refer to a variety of entities that pervade all of space, and that exist alongside—and even penetrate—other bodies. Thus Newton entertained the existence of an ether (or ethers) that carry heat, sound, and electric and magnetic influences, and play a role in the transmission of light (Queries 18–24). The nineteenth-century ether that carries electromagnetic radiation is a variation on this theme, but we are dealing with a different entity by the time we get to Lorentz's non-mechanical ether. In the case of the GTR the ether no longer pervades spacetime; it is spacetime. This is quite different from earlier concepts, and Einstein seems to recognize this: 'We may still use the word ether, but only to express the physical properties of space. The word ether has changed its meaning many times in the development of science. At the moment it no longer stands for a medium built up of particles. Its story, by no means finished, is continued by the relativity theory' (E, p. 143). It is worth considering whether this practice of preserving a word while the associated concepts change may not often generate more confusion than insight.

There is additional material in the book that I have passed over, including an account of the anti-Semitic attacks on Einstein and Einstein's changing attitude towards Mach's epistemology. There are occasional inaccuracies when Kostro comments on more general historical matters. At one point errant history leads him to misread Einstein. Discussing his distinction between 'box space' and 'relational space', Einstein says that Descartes reconciled these, and also says that 'the concept of space was enriched and complicated by Galileo and Newton ...' (E, p. 172). In Kostro's gloss, the two views of space 'were reconciled by Descartes after they had been enriched, to some extent, by Galileo and Newton' (p. 172). However, these are minor lapses that do not detract from the main subject or considerable value of the book. The book includes a substantial bibliography, although the index is limited to proper names. Footnotes are at the bottom of the pages.

Harold I. Brown, *Department of Philosophy, Northern Illinois University, DeKalb, IL 60115, USA*

Medicine and Health

K. J. Carpenter, *Beriberi, White Rice, and Vitamin B: A Disease, a Cause, and a Cure*. Berkeley, CA: University of California Press, 2000. xiv + 282 pp. 38 illustrations. 14 tables. $40.00/£27.95. ISBN 0-520-22053-6.

Kenneth Carpenter, a retired Professor of Nutrition, has established a reputation for himself in recent years as an historian of nutrition. His *History of Scurvy and Vitamin C* (Cambridge University Press, 1986) is an obvious precursor to the volume under review, and he has also written on the history of the controversy over the role of inorganic iron in the treatment of anaemia (*Journal of Nutrition*, 1990), and published *Protein and Energy: a Study of Changing Ideas in Nutrition* (Cambridge University Press, 1994).

Carpenter has chosen beriberi for his latest book, not only because it is a major nutritional deficiency disease that has been responsible for up to a million deaths but also because the story of tracing its cause and identifying a cure is an interesting one that shows the wider implications of experimentation and error in the tortuous process of scientific consensus building.

Beriberi is a word of uncertain etymology denoting an insufficiency of thiamine or vitamin B_1 in the diet. Symptoms include apathy, loss of appetite, painful legs, and, in extreme cases, paralysis, cardiac problems, asphyxial convulsions, and death. Patients may present with oedema (wet beriberi) or without (dry). The disease had been described by a Dutch physician in Java in 1673, a British official in Sri Lanka in 1803, and it was also well known in nineteenth-century Japan where it was called 'kakké'. Early explanations had included poisonous miasmas rising from the soil, sitting in the squatting position, and sexual excess. Kanehiro Takaki, a Japanese naval surgeon working in the 1880s, was convinced that the problem was rather a protein-deficient diet and he managed to persuade the authorities to add meat, condensed milk, bread, and vegetables to the traditional rice rations of sailors. The annual number of cases of kakké in the Japanese navy was reduced from 1000 per year to virtually zero and Takaki felt vindicated.

Also in the 1880s, August Hirsch published an important review of knowledge to that date. Although diet was discussed as a factor, Hirsch thought malnutrition was not a cause. Beriberi had been observed in wealthy, well-fed families in Brazil, and in parts of India where the diet was similar to other regions that were free of the disease. He also counselled against 'the modern craze for bacteria' but inevitably, with the fame of Pasteur and Koch spreading rapidly, there were workers willing to see beriberi as an infectious disease.

Dutch scientists played an important role in the early experimental work on beriberi. The Netherlands had a military motive because her weakened forces had been unable to conquer the whole of what is now Indonesia. In 1886 a medical commission was established to carry out research and its leader, Cornelis Pekelharing, concluded that he was dealing with an infectious disease. He thought that the causative bacteria were in the air of the buildings occupied by servicemen and that the buildings should therefore be disinfected.

A member of this commission, Christiaan Eijkman, then began experimenting with animals, at first with rabbits and later chickens, to try to infect them with beriberi. His early efforts did not bear fruit until by chance the birds' feed was changed to left-over, cooked rice. Very soon they developed a 'polyneuritis' that resembled beriberi in humans. Eijkman started a long series of trials that lasted for five years and which were the basis of the Nobel Prize that he received later. He realized that the way the rice was processed might be significant, especially the removal of the outer husk in order to leave a polished, white grain, but it was possible that the unprotected grain was vulnerable to some kind of infection, or that its starch was toxic. A crucial piece of additional evidence came when in 1897 the food of convicts in Java's prisons was analysed and a clear correlation found between the incidence of beriberi and the consumption of polished rice.

Kenneth Carpenter avoids a hagiography of the scientists involved in the beriberi story. He points out, for instance, that Eijkman was not especially well informed of the work of others. He did not know, for instance, of the work of Takaki because the library he used took only journals in Dutch and German, and he also undervalued the publications of another Dutch doctor, Van Leent, who in 1880 had very clearly stated that the disease was nutritional in origin. Eijkman himself suffered chronically with malaria and this meant that he was unable to work for long periods of time but this cannot explain why he was so uncertain about beriberi. At times he favoured a dietary model but he also published papers claiming that it was an infectious disease. Carpenter rightly observes that 'an honest work in the history of science must include such convolutions, rather than selecting only those contributions that give it the artificial appearance of a straight line'.

The book continues in this vein, with an account of Eijkman's successor in Java, Gerrit Grijns, who seems to have been more convinced of the nutritional explanation, and attributed beriberi to the lack of an essential trace nutrient. Grijns tried various dietary supplements to prevent or cure beriberi and found that mung beans were protective. The British were also active researchers from the early years of the twentieth century and their colonial authorities were more willing to respond to the lessons learned than were the Dutch. Leonard Braddon, a government medical officer in Malaya, noticed that villagers living on rice hand pounded each day or on imported, parboiled 'Bengal rice' were less likely to suffer from beriberi than Chinese labourers fed on machine milled 'Siam rice'. As a result parboiled rice was used in public institutions.

The Dutch and British tended to work without acknowledging each other; however, from about 1910 research on beriberi became more international in flavour. The first meeting of the Far Eastern Association of Tropical Medicine was held in Manila, with representatives from all of the major powers in the region, except France. This revealed that a variety of opinion still remained on the aetiology of the disease and the conference's final resolution implicating polished rice was only passed after several of the dissenting delegates had left.

From this point on the consensus did genuinely build around the nutrition explanation but the story of beriberi did not become simpler as a result. It transpired, for instance, that the disease was not confined to rice-eating cultures. Dr Cluny McPherson of the Mission Hospital for Deep Sea Fishermen in Labrador, Canada, in 1904 suggested that fishermen with paralysis of the legs, but who did not have fever, were suffering from beriberi. This diagnosis was greeted with derision: 'where do you think you are, Mac, in Japan or Malaya?' However, this was not an isolated observation. Similar symptoms were also reported from Brazil, Newfoundland, Europe, and America and, on close examination, the neuritis suffered by many alcoholics was identified as beriberi.

In the middle portion of his book, Kenneth Carpenter moves on from his account of the early detective work and attendant scientific bickering to an interesting description of 'the isolation and construction of a vitamin'. We are given a layperson's account of the chemistry of extracting the active substance in rice polishings, along with the invention of the term 'vitamin' (vital amine) by Casimir Funk in 1912. Funk had been prevented from using this word in previous publications by the Director of his laboratory, the Lister Institute in London, but slipped it into a book review, which he did not have to submit for vetting. The terminology proved to be vital for popularizing the concept, even though Funk's claim to have isolated a vitamin proved to be false. Crystals of vitamin B_1 were not definitively produced until the late 1920s/early 1930s. Since there is only a teaspoonful in every ton of rice polishings, this is not surprising. In addition there were difficult chemical hurdles to overcome.

Carpenter reveals some of the rivalry and bitterness surrounding the pioneering work on vitamins. Frederick Gowland Hopkins and Eijkman received the Nobel Prize in 1929 in recognition of their research but Elmer McCollum of the University of Wisconsin was marginalized, despite his important work on 'water-soluble B' as a growth factor for rats. Although this history is relatively well known I would have welcomed a greater emphasis on the characters and their alliances. How was it that Eijkman received the approbation when other workers on beriberi and what came to be called thiamine or vitamin B_1 were ignored? Bruno Latour's conceptualization of scientific influence in terms of an actor-network theory and his discussion of the 'construction' of scientific facts might have helped Professor Carpenter here. The lack of reference to the literature of the sociology of scientific knowledge is probably the major lacuna in this otherwise excellent book.

In the final third the author switches from historical to scientific mode. We learn about the difficulties encountered in identifying how much thiamine the body needs and this is no less interesting because it interfaces with the food policy implications of recommended daily allowances. Carpenter also discusses the knotty problem of food fortification, which with thiamine in flour began in Britain in 1940 and for rice in the Philippines in 1948. Finally, he cannot resist adding an appendix on the technicalities of thiamine chemistry.

This book is well written in a format and style that is accessible and sensitive to more than one disciplinary perspective. Most of the history is already on the record but the author has drawn numerous strands together in what he calls an 'integrated story', and this makes it a genuinely useful contribution to the growing literature on the history of nutrition.

P. J. Atkins, *University of Durham, Durham DH1 3LE, UK*

Natural History

James A. Secord, *Victorian Sensation: The Extraordinary Publication, Reception, and Secret Authorship of Vestiges of the Natural History of Creation*. Chicago, IL: University of Chicago Press, 2000. xviii + 624 pp. 155 halftones. $35.00/£22.50. ISBN 0-226-74410-8.

Students of the history of science usually first come across *Vestiges of the Natural History of Creation* (1844) in connection with Charles Darwin's *On the Origin of Species* (1859). *Vestiges* was seen by Darwin himself, as well as by later historians, as having drawn some of the anti-evolutionary fire prior to the publication of the *Origin* and as having been an important 'precursor' of that book. There is a common perception of the *Vestiges* episode as something that happened *Just Before Darwin* (the title of Milton Millhauser's 1959 book on the subject). James A. Secord's monumental *Victorian Sensation* rejects this anachronistic, Darwin-centred approach. Instead of trying to understand *Vestiges* by looking backwards from 1859, Secord immerses us in the world of readers of books, journals, and newspapers in 1840s Britain. From their perspective, Charles Darwin was not the author of the *Origin* but was merely an 'invalid geologist and author of a round-the-world travel book' (p. 21).

Victorian Sensation is a history of reading as much as it is a history of science. Indeed, Secord claims that his book offers 'the most comprehensive analysis of the reading of any book other than the Bible ever undertaken' (p. 2). The book in question, the anonymous *Vestiges*, was, according to Secord, the first of a new genre with which we have now become familiar—the 'evolutionary epic'. This was a genre aimed at a popular audience, in which the narrative form of Sir Walter Scott's historical *Waverley* novels was combined with the style

of science journalism. *Vestiges* told the story of the universe from its beginnings in 'Fire Mist' and swirling nebulae, through the geological history of the earth, right up to the development of terrestrial life and the emergence of human beings. All animals, humans included, were presented as having developed from entirely natural origins—this was no traditional 'creation' narrative. Although the author repeatedly asserted that this natural development was the outworking of a Divine will, the book was widely condemned as a work of religious infidelity. The ensuing public furore, the 'sensation' of Secord's title, had two focuses: the book's transmutationist doctrine of development and the identity of its author.

Speculation about the authorship of *Vestiges* was intense, and continued to fascinate for several years after the publication of the first edition. Among the most widely suggested potential authors in the first instance were Ada, Countess of Lovelace, and the aristocratic Member of Parliament Sir Richard Vyvyan. 'The two principal suspects in fashionable London could scarcely have been more different', Secord writes. 'One came from a well-known family of liberal Whigs, the other was a Tory of the most conservative kind; one had been brought up as a Unitarian, the other was an Anglican; one could not vote, the other was in Parliament.' (p. 186). This is an important observation, and one that helps to correct the impression sometimes given by Adrian Desmond's work, for example, that transmutationism was inevitably associated with radical politics and atheism in this period.

By 1847 several apparently well-informed sources were pointing the finger at the Edinburgh journalist and publisher Robert Chambers (although it was not until 1884 that the mystery was definitively cleared up, when the posthumous twelfth edition bore Chambers' name on the title page). Chambers, however, is not the central focus of *Victorian Sensation*. Secord rejects the Chambers-centred approach along with the Darwin-centred one. Among his reasons for rejecting both these approaches is the desire to recreate the experience of readers of *Vestiges* in the 1840s. Those contemporary readers were reading an anonymous work. To understand how they read the text and what it meant to them, Secord argues, we must first put the figure of Robert Chambers out of our minds. The absence of a named author in the case of *Vestiges* makes it particularly well suited to the approach, which Secord borrows from literary studies, of minimizing the importance of authorial intention when thinking about the meaning of a text. Such a heavy insistence on the primacy of readers in creating meaning might not be so well suited to all texts, however.

One of the most striking features of *Victorian Sensation* is the fine-grained detail it includes. The scene of the composition of Reverend Adam Sedgwick's venomous *Edinburgh Review* article on *Vestiges*, for instance, is brought wonderfully to life. We are told not only about the picture that would have been hanging on the wall of his Trinity College rooms in Cambridge—'The Fall of Babylon' by John Martin—but also what sort of frame it had. The picture itself is included as an illustration; another illustration in this section is a page from Sedgwick's own copy of *Vestiges* showing some of his annotations. However, my own favourite example of both the vivid detail of Secord's descriptions and the meticulousness with which they are referenced comes in an account of Charles Darwin reading *Vestiges* in 'the bustling, flea-infested British museum library' (p. 429). At the end of a lengthy footnote to this description we read, 'for the fleas, Miller, 1973'.

Victorian Sensation is not just full of empirical details, it is also historiographically innovative. It is, to start with, a marvellous advertisement for the virtues of social history of science. It presses the familiar buttons, showing how the creation and reception of scientific ideas are significantly shaped by economic, political, and religious interests. However, it goes much further than this in several ways. Secord is unusually good at explaining *both* the intellectual dimensions of philosophical, scientific, and religious debates themselves *and* their broader social functions (rather than focusing on the latter to the exclusion of the former). He is particularly painstaking and impressive in getting to grips with the theological debates of the period. Secondly, *Victorian Sensation* puts into practice an aspiration expressed by increasing numbers of historians of science to make extensive use of local histories. It includes formidable studies of urban life in London, Liverpool, Cambridge, and Edinburgh. These bring out very nicely how local circumstances affected the way *Vestiges* was read in different parts of Britain.

The most important way this is a groundbreaking book, however, is that it is the first to look at the history of science primarily as a history of *reading*. This means shifting the historical focus from authors to readers. It also means making extensive use of the methods

of book history. Secord treats *Vestiges* not just as a collection of ideas but as a material commodity, made by men and machines, distributed by railway, sold in different editions, at different prices to different groups of readers. Thus the histories of the publishing industry, the book trade, and transmutationist ideas are interspersed. The more difficult task is to establish significant links between these histories. Secord tackles this problem head on, and makes some quite forceful claims—for instance that the binding cloth and type size of the book affected the intellectual meanings readers found in the text. Certainly the presentation of each edition indicated the readership for whom it was intended. Similarly, the price determined for whom it was affordable. However, even in those cases where readers were fully aware of how the price and binding of their edition determined its likely readership, it is hard to see how this would have shaped the meaning they attached to claims about physics, physiology, and phrenology. When it comes to these ambitious claims for the impact of the physical form of the book on the way the ideas were construed, the rhetoric sometimes outstrips the evidence.

Secord has, none the less, used the tools of book historians and literary critics to create a remarkable and different kind of history. The quantity of the evidence—every last flea is footnoted—and the quality of the analysis marshalled in support of this new approach make *Victorian Sensation* an imposing landmark in the history of science.

Thomas Dixon, *Faculty of Divinity, Cambridge CB3 9BS, UK*

Eastern and Oriental Science

Yung Sik Kim, *The Natural Philosophy of Chu Hsi (1130–1200)*. American Philosophical Society, Memoirs Series, Vol. 235. Philadelphia: American Philosophical Society, 2000. xii + 380 pp. $30.00. ISBN 0-87169-235-X.

Probably because it was the eight hundred years' anniversary of his death, 2000 was a year of harvest for scholarly works on Chu Hsi. Not to mention so many conferences on Chu Hsi held in China and Taiwan, we have seen two major works published in the English world in the year 2000. One is Julia Ching's *Chu Hsi's Religious Thought*, which is an excellent interpretation of Chu Hsi's religious thought, focusing on issues such as the Great Ultimate, spiritual beings, rituals, personal cultivation, and Chu Hsi's relation with Taoism and Buddhism. Another is Yung Sik Kim's *The Natural Philosophy of Chu Hsi*, which is a very comprehensive survey of Chu Hsi's natural philosophy and natural knowledge, including his basic concepts of natural philosophy, his general ideas about the natural world, and his natural knowledge in particular domains such as calendrical astronomy, harmonics and music, geography, divination and alchemy, technique of nourishing life, etc. Adding to all this, Kim writes a chapter comparing Chu Hsi's natural knowledge with Western scientific tradition, and ends up his book with a critical evaluation of Chu Hsi's natural knowledge, rather than his natural philosophy, and his methodology.

This book is relevant to history of science in the sense that it shows how Mediaeval China's knowledge of nature and its philosophical foundation could be synthesized in one of its greatest philosophers, Chu Hsi, a Neo-Confucian of Realist type. We should notice that Chu Hsi was earlier than Western Mediaeval scholars such as Roger Bacon (1210–92), Albert the Great (1200–80) and St Thomas of Aquinas (1225–74), etc. Also we should not judge Chu Hsi's natural knowledge through the looking glass of Western modern science and scientific methodology. Kim's book has great merit in his exploration of Chu Hsi's natural philosophy and natural knowledge based on exhaustive textual analysis of Chu Hsi's conversations and writings. However, when we come to the comparison with Western science and Kim's critical evaluation of Chu Hsi's natural knowledge, which characterizes Chu Hsi's natural knowledge as common sense and in lack of theoretical speculation about space, time, void, infinity, indivisibility, mixture, and so on, there might be some bias from the perspective of West which could be misleading for appreciating the consistency of Chu Hsi's own natural philosophy.

Yet I should say that Kim's book is very carefully organized and well documented on Chu Hsi's works and works on Chu Hsi. It analyses minutely Chu Hsi's texts, more in his *Classified Conversations* and less in his *Collected Writings*, to find out his basic concepts of

natural philosophy and his knowledge of the natural world. The numerous endnotes of each chapter are very helpful indeed for identifying all resources Kim refers to. It is also a merit of this book, among others, to supply readers with well-elaborated and helpful tables, resulting from hard work of research based on textual analysis and classification of Chu Hsi's sayings, for example the Yin–Yang Associations, the Five Phases Associations, the Common Trigram Associations, the Four Cosmic-Quality Associations, *Kuei-Shen* Associations, *Hun-p'o* Association, etc.

Here I want to point out a problem concerning Kim's primary sources. Most of his analysis of Chu Hsi's natural knowledge is based on the *Classified Conversations* (the *Yülei*), where we find different kind of dialogues and occasional talks Chu Hsi entertained with his students and other interlocutors. However, for me, the *Classified Conversations*, as recorded by his disciples and others, has less value than his own writings in the *Collected Writings*, which, as outcome from his own hands, he himself must be sure of and responsible for. Also, for pedagogical reasons, Chu Hsi's answers to students in the *Yülei* were more occasional and conjectural than of sure knowledge. We should say that the *Collected Writings*, in which all discourses and all wordings were well measured by Chu Hsi himself, should be of higher value for textual analysis than the *Classified Conversations*. I myself would hesitate to take *Classified Conversations* as an object of textual analysis in search for evidence of Chu Hsi's natural knowledge.

Besides, we have to distinguish between knowledge and guess or knowledge and belief. For example, Chu Hsi says in the *Yülei* that beyond heaven there lies a supreme empty void and that the shape of heaven and earth is like a bird's egg. In what sense can we call this Chu Hsi's natural knowledge? For me, these could at best be seen merely as guess or belief. However, when Chu Hsi proposed certain arguments and verifying examples, such as in the case when he said, 'mountains were formed by the elevation of sea bottom' (Kim, p. 146), then he proceeded to prove it by the presence of seashells on top of mountains. This kind of proposition might be seen as more than mere guess or belief.

Also we have to distinguish 'metaphorical reference to nature' and 'knowledge of nature'. For example, when Chu Hsi said, 'Two physical forms [grinding to each other] are things like drumsticks and drums. *Ch'i* grinding physical form are things like bamboo pipes. Physical forms grinding *ch'i* are things like [the sound of] feather fans and arrows.' (quoted in Kim, p. 32). Here Chu Hsi was not referring, as Kim would say, to tangible and visible corporeal objects such as drums, pipes, fans, and arrows, but rather in a metaphorical way to the relation between physical forms and *ch'i*. Also, when Chu Hsi said that studying is like burning of fire (p. 179), he was not discussing his knowledge of fire itself but used the metaphor of fire to illustrate the process of studying. I would not read Chu Hsi's knowledge of fire out of this kind of metaphorical use of language. Metaphor, which allows us to see X as Y, should not be considered as a kind of description. This should be always kept in mind when we read Chinese philosophical texts.

In Kim's book there are some figures that are very helpful for understanding what he is trying to say, but some other figures might suggest something incoherent with what he says. For example, Figure 9.1, 'The Dark Spot on the Moon' (p. 143), suggests a relation of moon, earth, and sun which is incoherent with what the author mentions in the previous page that the earth was 'floating on water ... whose boundaries touch heaven's vault' (Kim, p. 142), and in the following page, 'beyond these far regions are the oceans, which surround the earth and are extended outward to touch heaven's vault' (p. 145).

Concerning Chu Hsi's knowledge of Man, Kim seems to suggest that Chu Hsi accepted Mencius' theory in attributing moral qualities to *ch'i*, the latter's ideas of 'magnanimous *ch'i*' and *ch'i* mind interaction (pp. 216–19). However, when related to Chu Hsi's concepts of *li* and human nature, we should add to all these that he is different from Mencius in that Mencius understood the four virtues of humanity, righteousness, propriety, and wisdom as the full unfolding of the four beginnings, that is the mind/heart/feelings of commiseration, of right and wrong, etc. However, Chu Hsi held that humanity, righteousness, propriety, and wisdom belong to the *li* of human nature, whereas those mind/heart/feelings of commiseration, right and wrong, etc. were but the expression of *li*. As to his concept of Mind, Chu Hsi followed Chang Tsai in saying that mind embraces both human nature and human emotions in its unity. Also he followed the line of thought of the Cheng brothers that mind, as a unity of quiescent substance and dynamic function, is on the one hand closer to the original human

nature and, on the other hand, in tension between its being one with reason and its divergence from reason in the emotional life; the battle thus created could be resolved only by virtue of persistent reverence.

Generally speaking, Kim's translation of Chinese terms into English is quite correct and reasonable. Still, there are some noticeable problems. For example, Kim's translation of *kan-ying* as 'stimulus–response' (p. 122) could be misleading, because the term *kan* means receptive affection rather than stimulus. Therefore it is better to translate this term as 'affection–response'. Also, the term *ti-li* in traditional Chinese culture should be understood and translated as 'geomancy' rather than as 'geography' (p. 246).

VINCENT SHEN, *Department of Philosophy, University of Toronto, Toronto, Ontario, M5S 1A2, Canada)*

Philosophical Aspects of Science

J. CRARY, *Suspensions of Perception: Attention, Spectacle, and Modern Culture*. Cambridge, MA: MIT Press, 1999. ix + 397 pp. $39.95. ISBN 0-262-03265-0.

Attention is en vogue. Contemporary media experts, information technologists, and Internet traders understand very well that it is not sufficient to count on the curiosity of consumers with good will. Since the spectrum of visual stimuli and entertainment, material and non-material goods has become so vast and diverse, curiosity and fascination with the new are no longer at stake, if the problem is how to acquire and manage more and more information in shorter and shorter periods of time. In this situation, attention has become a central focus of interest. Attention is so precious and expensive because it cannot be increased at one's discretion and because it is the target for anyone who wants to 'sell' goods, ideas, knowledge, ideology, etc. Authors such as Georg Franck speak of an 'economy of attention' and argue that it stands with equal rights, analogous to an economy of money. We are confronted with a situation in which it is more and more complicated to decide how to invest one's own attention and how to evoke the attention of others. Consequently Franck calls for a new 'ethics of attention'.[1] It is a truism that this condition of the (post)-modern individual is inseparably linked to the conditions of information technology and media that surround us. The length of items on television has regulated our visual attention, the permanent threat of mobile phones has affected our capacity for concentration in various social situations, and the use of computers inevitably trains us to bring our own attention and speed of response to correspond to the commands and functions of the machine.

Jonathan Crary is fully aware of the current importance of attention. In a brief and illuminating reflection on the so-called 'attention deficit disorder', for example, he argues that it is nonsense to pathologize certain forms of behaviour 'in a culture that is so relentlessly founded on a short attention span, on the logic of nonsequitur, on perceptual overload' (p. 36). Crary's exhaustive and admirably erudite history of attention in modernity is—although it only covers the relatively short period of time between the 1870s and the 1910s—in fact a long argument that current patterns and mechanisms of attention are to be understood as a consequence of modern transformations of perception and of attention in the nineteenth century. These transformations are inseparably interwoven with scientific, technological, economic, and social changes, but the world of modernist painting is the main stage on which Crary displays and exemplifies his argument.

Theorists of modernity such as Walter Benjamin, Siegfried Kracauer, and Georg Simmel have described the difficult situation of the modern subject in the techno-industrial world as a biased relationship. Simmel, for example, described a rapid growth of the 'objective mind' in law, technology, science, art, and everyday life to which the individuals reacted with a decrease of culture, in particular with hindsight to cultivation, attention, and sensibility. The 'acceleration of nervous life' ('Steigerung des Nervenlebens'), which results from the permanent change of inner and outer impressions, has led to 'blaséness' ('Blasiertheit') and distraction, so that the differences between things and phenomena are no longer perceived.[2] Similarly, Benjamin argued that attention and distraction are two opposite poles and that distraction is the appropriate reaction of the modern urban individual.

This modern legacy becomes the starting point for Crary in two respects, firstly because it implies a historiographical and methodological point and secondly because the historicity

of attention itself is at stake. First, Crary follows Simmel's claim, according to which historical transformations occur more or less at the same time in the arts and sciences, in technology and everyday life. By juxtaposing various historical events that seem to be quite distant from one another at first sight, a panorama of an epoch emerges. In this scenario, it is not necessary to prove specific relations between these fields with the aid of philology. To be sure, this is not a post-modern invention. Benjamin's fragmentary 'Arcades Project' and Dolf Sternberger's 'Panorama des 19. Jahrhunderts' from 1937 are composed exactly in this way. Crary does not organize his book around Parisian arcades or the panorama but around multiple historical facets of attention. After a more general introduction on the historical development of attention, he focuses on three important paintings, to which he devotes one large chapter each: Edouard Manet's *In the Conservatory* from 1879, Georges Seurat's *Parade de cirque* from 1888, and Paul Cézanne's *Pines and Rocks* from 1900. I feel unable to summarize these chapters even roughly, since they all start from a careful description of the paintings and then embark on extensive intellectual excursions which one might characterize as endless chains of associations. Crary describes his method as assuming a 'simultaneous but autonomous coexistence of disparate cultural artifacts, outside of mechanical or biographical notions of influence and worn-out distinctions between "high" and "low" culture' (p. 9). Like Foucault's notion of *episteme*, the juxtaposition of these 'cultural artifacts' results in a big picture that may not always appear plausible or coherent. On the other hand, the loose connection of events and the avoidance of giving only one authentic meaning to paintings, experiments, scientific theories, etc. lead to an interpretative freedom and to new insights. Apparently well-known episodes from cultural history and from the history of science seem quite different when observed through the looking-glass of attention.

The second aspect hints at the historical notion of attention itself. In contrast to theorists such as Simmel, Benjamin, and others, who had proposed a fundamental duality between attention and distraction, Crary argues that 'modern distraction was not a disruption of stable or "natural" kinds of sustained, value-laden perception [. . .], but was an effect [. . .] of attempts to produce attentiveness in human subjects. If distraction emerges as a problem in the late nineteenth century, it is inseparable from the parallel construction of an attentive observer in various domains.' (p. 49). Here the history of science becomes crucial, because Crary gives overwhelming evidence that physiology, psychology, and medicine played a decisive role in the attempts to create and manage new regimes of attention. His emphasis on sensory physiological experimentation, instruments, models, and theories is known from his first book *Techniques of the Observer* (1990). There Crary argued that the discovery of subjectivity in early nineteenth-century physiological optics made vision into a process in which the perception of the world was not a given but the result of a physiological construction of the observer. Perception was thus conceived not as a passive, but as an active process. In consequence, empirical investigations of perception, motion, cognition, and pathological deviations led to 'powerful narrative models of subjectivity' (pp. 96–97).

While the topic of Crary's first book was the demise of the anchored classical observer in the first half of the nineteenth century, this book deals with the emergence of the 'unstable attentive subject' (p. 148), which on the one hand copes with the 'subjective limitations of vision and makes perception its own' and 'becomes open to control and annexation by external agencies' (p. 5). The central point is that until the mid-nineteenth century attention had been understood as the guarantor for the coherence, stability, and unity of mental life. It was not until the 1860s that attention was fundamentally reconfigured in the experiments of Helmholtz, Mach, Fechner, and other psychophysiologists. This understanding of attention as an experimental object had broad consequences. The reassuring bourgeois idea of attention as making us the masters of ourselves was replaced by the idea that attention is a motor act that is partly responsible for the shaping of perception itself. If attention was until then a virtue, typical of an educated and disciplined individual, it now became 'a continuum of variation, a temporal modulation, and it was repeatedly described as having a rhythmic or wavelike character' (p. 65). This new understanding was exactly the result of Fechner's (and others') experiments. Consequently, for the French psychologist Théodule Ribot attention was 'an exceptional, abnormal state, which cannot last a long time, for the reason that it is in contradiction to the basic condition of psychic life, namely change' (p. 64).

The destabilization of attention was not restricted to the laboratory. It was part and parcel of a broad tendency in society and culture. Attention became 'a fundamentally new object

within the modernization of subjectivity in the second half of the nineteenth century' (p. 17). A great deal of modern technology was established to manipulate attention in two directions. The goal was either to control the observer's subjective experience (e.g. with the tachistoscope and reaction time experiments) or to use attention as a dynamic system in order to enhance the capitalist world of goods, spectacle, and consumption. Crary collects numerous examples for the demonstration of this historical oscillation of attention between free-flow and control, (self)-disciplinary technologies and distraction. The three paintings by Manet, Seurat, and Cézanne each stand for a crucial aspect of this destabilization and re-synthesis of attention.

Since I am not in the position to judge Crary's art historical expertise and his analysis of the three paintings, I restrict myself to the question of which role the psychophysiological sciences play in this approach. Since the late 1980s or so—catalysed by Crary's first book and other influential studies by Christoph Asendorf, Barbara Maria Stafford, and Anson Rabinbach—we have been witnessing an ongoing fascination by cultural historians with the world of experimentation, instruments, and technologies including the phenomena they produced: images, graphs, diagrams, optical illusions, measurements, etc. Much of this work has contributed heavily to a more refined understanding of scientific practice: scientific images often follow (and sometimes shape) aesthetic conventions; self-experimentation and sensory physiology have had an impact on the construction of the modern subject; psychophysiological measurements in an industrial context are a central aspect in the history of the modern body. What is the result for a broader understanding of scientific developments in Crary's book? Such a question does not seem unfair, since he subscribes to Deleuze's proposal that 'philosophy, art and science come into relations of mutual resonance and exchange, but always for internal reasons' (p. 9). I fully agree with this claim, but the question of whether these 'internal reasons' are sufficiently and plausibly analysed remains tricky.

Crary's excursions into the history of science are undoubtedly based on profound research and an admirable knowledge of secondary literature. He gives fair and well-informed descriptions of John Hughlings Jackson's neurology, Helmholtz's theory of unconscious inferences, and Charles Sherrington's neurophysiological theory of integration. Nevertheless, the author leaves us with a hemianoptic picture when he emphasizes the holistic element in Sherrington's theory without mentioning that neurologists such as Kurt Goldstein, Viktor von Weizsäcker, and many others regarded reflexology as an important neurological doctrine, but at the same time criticized it as mechanistic and thus insufficient for a holistic conception of the human organism. Crary states that Henri Bergson criticized Helmholtz's 'unconscious inferences' as 'making perception into something mechanical and automatic' (p. 322), but he does not say that at the same time Ernst Mach attacked Helmholtz for exactly the opposite reason, namely that the 'unconscious inferences' were an irritating relapse into idealism. My point here is that the historical existence of a cultural artefact like the 'unconscious inferences' and reflexology is so complicated and sometimes contradictory that it is not so easy to take it as one coherent discursive field as Crary seems to suggest. This is ironic, since Crary has explicitly formulated his aim to liberate some modern key paintings from their interpretative chains and—quoting Roland Barthes—'"to remain attentive to the plural" of these paintings' (p. 9). It is a high price, if the multiplication of the meanings of one artefact is bound to the reduction of the meanings of another. I do not think that this is an unavoidable nemesis of any comparative cultural history, but Crary's extraordinarily rich study displays the possibilities and the dangers of this approach.

All in all, I would like to understand *Suspensions of Perception* as a contribution that fulfils Walter Benjamin's proposition about the use of history. This was the motto for Crary's first book: 'For the materialist historian, every epoch with which he occupies himself is only a fore-history of that which really concerns him.' This book is an admirable and earnest attempt to re-emphasize the importance of what Aleida Assmann has recently called 'transcending attention'.[3] This form of attention is deep and focused rather than superficial and widespread, resting and hesitating rather than free-floating, and serving for self-education and knowledge rather than for amusement and spectacle. Crary is not a conservative scepticist like George Steiner or Harold Bloom. He does not entirely cast off short-term attention and spectacle, because he knows that after having eaten the apple from the tree of the *condition moderne* there is no way back to a status quo ante. The alternative model is to use various forms of attention strategically so that information technologies are not the only manipulative masters of attention.

In his discussion of Cézanne, which is centred around the question of 'how the discontinuities and disjunctions [of attention] became the basis for new models of synthesis and perceptual organisation' (p. 330), Crary discovers the technologies of stasis as an antidote against the overwhelming flows of information. Instead of 'sweeping the eye back and forth over the visual field', he suggests 'patiently looking in a fixed way at local areas of the field'. Only thus 'does one begin to see its unknown texture, its strangeness, the unfathomable relations of one part of it to another, the uncertainty of how these local elements interact as a dynamic field' (p. 298). I cannot decide whether this is a correct interpretation of Cézanne, but it is certainly the technology of attention that Crary patronizes. In a brief epilogue, Crary regards Freud as having developed 'one of the most formidable techniques of attention to emerge in the twentieth century' (p. 367). In September 1907, Freud reported to his childen about his amusement among thousands of people in the Piazza Colonna in Rome. For Freud, suspension of attention was not only the right behaviour to enjoy that warm summer evening; in his *Recommendations to Physicians Practicing Psychoanalysis*, Freud claimed that suspended attention when listening to patients was a crucial diagnostic tool. Freud's emphasis on a technology of attention was certainly original; nevertheless, he relied on much older technologies and practices that had been developed in a medical and psychological context. As early as the late eighteenth century, the Berlin psychologist Karl Philipp Moritz programmatically called for careful introspection. He asked that we suspendedly observe the daily tide and flood of ideas and images in ourselves.[4] Thus, suspended perception for both the apparently important and less important aspects of ourselves was part of bourgeois self-experience from the very beginning, and it is no coincidence that Freud transferred this practice to the domain of psychoanalysis. Unfortunately, Crary's Foucaultian approach of assuming a radical rupture and discontinuity in the 1870s is inattentive to these diachronic continuities from the eighteenth to the twentieth centuries. Despite this flaw, it is a great merit of this complex book to have shown that technologies of attention had a fascinating history in modernity and are still relevant today.

[1] Georg Franck, *Ökonomie der Aufmerksamkeit* (Munich, 1998).

[2] Georg Simmel, 'The Metropolis and Mental Life', in *On Individuality and Social Forms* (Chicago, IL, 1971), pp. 324–39.

[3] Aleida Assmann, 'Einleitung', in *Aufmerksamkeiten*, ed. by Aleida Assmann and Jan Assmann (Munich, 2001), pp. 11–23 (p. 21).

[4] See Karl Philipp Moritz, 'Vorschlag zu einem Magazin der Erfahrungs-Seelenkunde. An alle Verehrer und Beförderer gemeinnütziger Kenntnisse und Wissenschaften, und an alle Beobachter des menschlichen Herzens, welche in jedem Stande, und in jeglichem Verhältniß, Wahrheit und Glückseligkeit unter den Menschen thätig zu befördern wünschen', in *Karl Philipp Moritz, Lesebuch*, ed. by Uwe Nettelbeck (Nördlingen, 1986), pp. 151–69.

M. Hagner, *Max Planck Institute for the History of Science, Wilhelmstrasse 44, 10117 Berlin, Germany*

Social Aspects of Science; Religion

Peter J. Bowler, *Reconciling Science and Religion: The Debate in Early-Twentieth-Century Britain*. Chicago, IL, and London: University of Chicago Press, 2001. xiii + 479 pp. 16 plates. US$40.00. ISBN 0-226-06858-7.

Centuries are valuable for breaking up the historical continuum into manageable parts, but they can impose a grid that prevents understanding or even investigation. Because of the writings of T. H. Huxley, J. W. Draper, and A. D. White the idea of an apocalyptic conflict between religion and science in the nineteenth century has long been around, and has received much attention from historians. Religion and science have come to be seen as abstractions that could not be in conflict, and so the focus has shifted to issues, institutions, and personalities—and as a result less heat and more light have been thrown on episodes variously seen before as St George versus the Dragon in a final battle, or as another match in a long run of football games between traditional rivals. However, 1900 (or 1914 when the 'long nineteenth century' ends) has been perceived as a terminus; so that while 'religion and science' is recognized as an 'Open Sesame' into the dark backward and abysm of time for nineteenth-century science, for the twentieth century it has been used only in exploring the curious territory of American Creationism. Peter Bowler is thus staking out quite new ground in

looking at Britain in the first half of the twentieth century, when fundamentalism was not an issue—more or less easy accommodation between modernist theologians and metaphysically minded scientists seeking purpose in the world is what he finds. His story also has a feature that all historians must envy, a beginning, middle, and end—for he sees the second half of the century as being essentially different, as those who held the positions he describes died, and their ideas became old hat.

His big book has a cast of hundreds, and brief biographies of sixty-four of the main protagonists are included in an appendix; there is also a bibliography of over forty pages. This is thus a wonderfully well-informed publication, which will be a mine of information for students and scholars. Its thesis is that attempts to humanize and soften the evolutionary process by building in purpose all failed in the face of Darwinism and neo-Darwinism, and that natural theology in this period collapsed under the assault of Karl Barth and his neo-orthodox disciples. Bowler tells a story of failure, in a profitable investigation of the losers in history. In working through to this conclusion he is very fair-minded and dispassionate, and has resisted temptations to irony and rhetoric that must have frequently presented themselves.

Ronald Fisher was a Darwinian who retained religious faith, but most of Bowler's cast of characters were Lamarckians who saw a direction to evolution, and many of them, such as Charles Raven and the cosmologists James Jeans and A. S. Eddington, were skilled popularizers. That meant that for laymen their views seemed to be within or even composing the scientific mainstream, when in fact the vast majority of active but more reticent scientists did not share them. Clergy who sought comfort and joy in science were thus misled, and it is a good general question how accurate public understanding of science at any time has been. This book poses it urgently: in many ways what is perceived to be the case is at least as important as what is actually so, but in the long run it may be (as here) that things are sullen, and will be as they are whether we wish it or no. Certainly the scientists best known in the wide world at any time may not at all be those with the highest reputations within their field of expertise. Some of Bowler's clergy, notably Raven and Bishop E. W. Barnes, were themselves scientists—but a difference from the nineteenth century is that the role of clergy in the learned world was much restricted.

The book deals first with scientists seeking design and purpose in an otherwise cold and hostile world. However, where they found religious experience or faith, it was often in a kind of pantheism that had been characteristic of nineteenth-century predecessors such as Humphry Davy or John Tyndall, and gradually lost the interest of the new generation. Then Bowler looks at the churches, and finally at the wider debate. He perceives a new confidence in natural theology emerging with Anglican modernism (very different from modernism in the arts) out of the older latitudinarian, broad, or liberal wing of the church—with at first some support from Anglo-Catholics such as Bishop Charles Gore and from liberal evangelicals such as Dean W. R. Matthews. The difficulty was that modernists seemed to be throwing out the baby of Christian faith with the bathwater of pre-scientific accretions, and lost their allies within the church. In particular Barnes with his notorious 'gorilla sermons' about evolution, and his assaults on 'ritualists' and high-church doctrines of the eucharist, set teeth on edge rather than calming fears and promoting harmony. Noisy attacks on evolution from G. K. Chesterton and Hilaire Belloc led to a popular belief that Darwinism was discredited just at the time when neo-Darwinism was steadily triumphing.

Running through the book is the steady decline of the churches through the twentieth century, despite various revivals, and also the loss of the idea of progress, in Edwardian pessimism and then in reaction to the Depression and the rise of fascist, Nazi, and communist governments. Darwin's bleak world became all too believable, and many of the churchmen that Bowler discusses, daunted by dreams of degeneration, embraced eugenics. To those who wanted to lean their backs against an oak, Anglican Modernism seemed a thin and hardly trusty tree, and in contrast orthodox religion, concerned with sin, suffering, and forgiveness, looked relevant to a world heading for disaster and bemused by Freudian psychology. Theologians no longer saw science as very important to them and their congregations. World War II and its aftermath brought about a new state of affairs, which Bowler glances at: his story is over, but one would hardly expect that the engagement of science and religion would not feature even in that strange eventful history of the later twentieth century. He has opened for us a new window on the world.

DAVID KNIGHT, *University of Durham, Department of Philosophy, 50 Old Elvet, Durham DH1 3HN, UK*

Institutions: Education, Libraries, Museums

PETER GALISON and EMILY THOMPSON, editors, *The Architecture of Science*. Cambridge, MA: MIT Press, 1999. xviii + 573 pp. 165 illus. 10 colour plates. US$68.95/£47.95 (cloth). ISBN 0-262-07190-8.

In his introduction to *The Architecture of Science*, the senior editor tries manfully to integrate the twenty-two other essays into pieces of a coherent whole. The usual difficulty of retrospective synthesis of papers prepared for a conference (Galison and Thompson's book comes from a meeting held at Harvard in 1994) is compounded by the disparity in the backgrounds and purposes of the writers—architects, laboratory directors, architectural theorists and historians, and historians of science. The editors distribute their hodge-podge under six heads: 'Of secrecy and openness'; 'Displaying and concealing technics'; 'Modern space'; 'Is architecture science?'; 'Princeton after modernism: the Lewis Thomson Laboratory'; 'Centers, cities, and colliders'. An alternative classification, adopted here, uses the stated aim of the conference: to study the influence of architecture on the course of science, and the influence of science on architectural design.

Closest to the forward direction (architecture to science) are the articles concerning the Thomson Laboratory for molecular biology at Princeton. Its director, Arnold J. Levine, and its principal architect, James Collins, Jr, disclose the thinking behind the design, and the sociologist Thomas Gieryn, right on target, compares the architecture of the Thomson Laboratory with that of New Jersey's Center for Advanced Biotechnology and Medicine in respect of their constituents, missions, locations, and users. In addition, a principal architect of the ill-fated Superconducting Super Collider (SSC), Moshe Safdie, explains why he laid out the 'campus' for the work and play of the machine's attendants as he did; also, inevitably but appropriately, Robert R. Wilson, the builder and first director of Fermilab, a sculptor as well as a physicist, once again gives the rationale for the colour of the plumbing, the plan and height of the high-rise central building, and the bison pasture within the ring, of the premier particle accelerator in the United States.

Next in thematic proximity come three articles on the nineteenth century: George W. Stocking, Jr, on the arrangement of museum collections in accordance with anthropological theory; Sophie Forgan on the same question for comparative anatomy and geology; Myles Jackson on the adaptation of a Bavarian monastery to optical experiments and manufacture. This patchy coverage omits among much else the new university institutes for science, about which much information exists; they would have been a particularly worthwhile subject, since the adequate provision of services, especially electricity, brought something new to scientists and architects simultaneously. Alan M. Brandt and David C. Sloane indicate what can be done in this way in their straightforward article on the cooperation between doctors and builders in creating the modern American hospital.

From the mid-nineteenth century *The Architecture of Science* jumps to the seventeenth, omitting observatories, botanical gardens, academies of science, libraries, etc., indeed, all the 'sites of knowledge production' (to use the current jargon) of the intervening 250 years. The two articles at the far end of this blank, Paula Findlen's 'Masculine prerogatives: gender, space, and knowledge in the early modern museum' and William R. Newman's 'Alchemical symbolism and concealment: the chemical house of Libavius', are meritorious but tangential.

Findlen places Ulisse Aldrovandi's 'theater of nature', that is, his natural history collection, within his palace, relates access to it to Renaissance cultural conventions, and describes architectural theories and practical realizations of domestic spaces devoted to the display or pursuit of knowledge in early modern times (no ladies, please, the entry to the study is through the bedroom). Newman successfully does battle with Owen Hannaway, who represented Libavius's laboratory as an open and accessible place for chemical research and dismissed the alchemical motifs in its floor plan as a joke. Libavius's chemical house, like Aldrovandi's theatre of nature, belonged to him, and he designed it for his purposes; it has little in common with the great laboratories of the twentieth century, built with public money, open to students and qualified researchers, and designed by a squad of architects restricted in realizing their client's wishes by budgets and building codes.

Turning the arrow the other way, from science, or rather science-based technology, to architecture, we have Emily Thompson's well-considered 'Listening to/for modernity'. Its subtitle, 'Architectural acoustics and the development of modern spaces in America', aptly

indicates her subject, the changes wrought by accommodation to the theories and practices for cutting noise and reverberations in office buildings, concert halls, and churches. According to Thompson, soundproofing often meant covering walls with a mixture of asbestos and cattle hair; according to the *American Architect*, the purpose of the coating was 'to safeguard and promote the health of ... office workers'. *Omne ignotum pro magnifico est*. Kenneth Frampton develops a theme similar to Thompson's but with emphasis on energy conservation and environmental factors, and Sophie Forgan, in the article mentioned earlier, outlines efforts of Victorian architects to come to grips with Victorian science. A group of articles by architects may also concern the influence of science on architecture, but, as the science seems to be architecture itself, they are too self-involved for further notice here.

That leaves what for the reviewer are the two most remarkable items in the book. M. Norton Wise writes about the disguising of steam-engine houses in Germany to look like castles—to be sure, not all engine houses, only those for pumping water around large gardens and other places where their naked presence would blight the landscape. Wise makes good use of the opportunity of colour illustration to support his description of this concealment of technology, which he tries to attach to the main theme of *The Architecture of Science* by a link to the social aspirations of Germany's leading scientist, Hermann von Helmholtz.

The second remarkable piece, by Galison and the art historian Caroline Jones, has a story line that runs as follows. After World War II physicists adopted the housing and organization of a factory while artists created their works of genius shut up in small individual studios. Then, led by Andy Warhol, Frank Stella, and Ray Lichtenstein, they tried semi-industrial methods of fabrication and settled in abandoned lofts and workshops. In this last stage of modernism, the factories of science and art had identifiable leaders and locations. However, the gyre widened, the centre could not hold, physics flew into the ether, and art into the field. In a word, both have gone postmodern. Transient groups run physics experiments, sometimes far from where false consciousness might call the experimental apparatus, and send the data through the internet for analysis by other groups somewhere. Artists wrap buildings in plastic or arrange pebbles on the shore, photograph the fleeting results of their creations, and produce works guaranteed not to exist for long anywhere and yet available everywhere. These precarious parallels conflate laboratory science with quark physics and art with put-ons—brilliant as synecdoche, inadmissible as history.

J. L. Heilbron, *Worcester College, Oxford, OX1 2HB, UK*

Historiography of Science

Celina A. Lértora Mendoza, Efthymios Nicolaïdis and Jan Vandersmissen, editors, *The Spread of the Scientific Revolution in the European Periphery, Latin America and East Asia*. Proceedings of the XXth International Congress of History of Science (Liège, 20–26 July 1997), Vol. V. Turnhout, Belgium: Brepols, 1999. 194 pp. €37.50 (paperback). ISBN 2-503-50992-4. The book is trilingual: most papers are written in English, several in Spanish, and a couple in French.

'Periphery' is a very vague area in the history of science and the purpose of the present work is to delineate the journeys of the early modern scientific ideas in this slippery and misty landscape. It is doubtful whether the question 'what is periphery?' has ever received an adequate answer. This did not prevent, however, the distinction 'centre–periphery' from serving for many years various branches of the social sciences, although in each particular case the definitions varied. This is probably an indication that the clear definition of the elementary concepts is not always a necessary condition in order to discuss a number of issues in the history of ideas. Quite the contrary; the way we discuss these issues reveals the ideological load we pose on them. The editors of the present work chose to adopt an empirical–geographical approach: as organizers of a session in the XXth International Congress of History of Science, they initially focused on the European periphery, but soon they realized that colleagues from Latin America and China treated the spread of scientific ideas in their countries in similar ways. 'Ainsi, la synthèse a été faite au présent Symposium, où les trois unités présentées [...] tentent de couvrir un espace géographique varié et représentatif.' (p. 8). The outcome of this synthesis was a book consisting of nineteen papers

(including 'Avant-propos') divided into three geographical sections, an arrangement that identifies the scientific periphery of the early modern period with the economic periphery of the twentieth century. The time frame is defined loosely around the scientific revolution. Practically it extends from the late sixteenth to the late eighteenth centuries and offers a relaxed context for the study of the diffusion of scientific ideas in a variety of intellectual milieux.

The explicit purpose of the collection is declared clearly in the somewhat programmatic 'Avant-propos': 'L'étude de [l']épopée du savoir humain' in the periphery (p. 7). So far, the author claims, historians of science have focused on the formation of scientific knowledge but scarcely dealt with its diffusion. Only in recent years has an interest in this subject emerged and have a number of works enriched the corresponding bibliography. The main historiographic questions these works address are of the following kind. What were the mechanisms of transmission of the scientific knowledge to the countries of the periphery? How was this knowledge accepted in the new environment? What were the continuities and the ruptures with the scientific knowledge [*sic*] that already existed in these areas? An implicit methodological claim is thus made: the 'centre' produces science and the 'periphery', more or less willingly, embraces it; the task of the historian is to trace the procedure of the transfer and establishment of the new science from 'centre' to 'periphery'. The gathering of the works contained in the present volume aims at contributing to this historiographic perspective. Nevertheless, although several papers observe this stereotyped historiographic view, others appear to be quite informed by the recent discussions on the subject.

Reception studies *per se* is not a recent field in the history of science. There have been many studies examining the diffusion of the new ideas about nature in England, Scotland, France, the Low Countries, and Germany during the seventeenth and eighteenth centuries; many problems related to the reforms by Peter the Great in Russia have also been analysed; there have been inquiries on the introduction of the new scientific ideas in Latin America; so is the case for many aspects of science in the Scandinavian countries; the reactions to the Darwinian theory have been the subject of serious scholarship; the introduction of modern physics in a number of countries is also well documented. Furthermore, there have been extensive studies on the question of science, technology, and imperialism, while the establishment of university chairs in countries beyond Western European 'centres' has also formed part of systematic investigations. All this scholarship spans a period of at least three decades going back to the early 1970s.

Recently, however, new approaches on the subject have been articulated. The new context in the history of 'sciences in the periphery' is defined mainly by two methodological developments. The first concerns a certain 'deconstruction' of the idea of 'scientific centre'. Referring to the early modern period, the homogeneity of such cognitive enterprises as 'Scientific Revolution', 'science', 'physics', and 'Newtonianism' is extremely vague. The broad discussion—triggered to a great extent by F. Cohen's work—about the historiography of Scientific Revolution and the discussion on the multiple aspects of Newtonian physics—initiated as early as in 1960 by C. Truesdell and continued on a different path by several historians in the 1980s—have been quite convincing in moving away from a homogeneous view of early modern science.[1] In any case, the point is that *during these years a clear-cut line of natural investigation never existed* and, in this respect, the notion of 'scientific centre', *as a place where a well-defined and homogeneous practice was consensually agreed on*, is heavily problematic. Some of the contributors of the present volume adopt this view quite explicitly. Celina A. Lértora Mendoza, for instance, in the introduction to the Latin American section of the book, highlights the fact that the works of Newton determined the character of scientific activity in two different ways. On the one hand, the *Principia* initiated the 'General Physics' which consisted of celestial mechanics, rational mechanics, hydraulics, and the theory of vibrations and was developed by mathematicians such as Bernoulli, Euler, Clairaut, d'Alembert, Lagrange, and Laplace. On the other hand, a different line of investigation was initiated by the *Opticks*, continuing the pre-Newtonian experimentalist tradition. This line ran under the name of 'Special Physics' and was cultivated by natural philosophers such as 'sGravesande, Nollet, Boerhaave, and Musschenbroek (p. 90; original in Spanish).

The same premise occurs in some other papers, especially those of the Latin American section. However, it is difficult to discern an analogous consideration in the rest of the contributions. The papers devoted to the Greek-speaking world of the eighteenth century, for

example, are permeated by questions of the following type: 'At what moment can we say that this world begins to be aware of and participate in the shared culture of European Science, formulated by the ideas of the Scientific Revolution?' (p. 34). Along similar lines, most of the papers devoted to Eastern Asia seek to trace—in a quite philological way—the dissemination of a self-contained Western science in the Far East. The presence of the various Catholic missions, the different views between the religious orders, and the philosophical debates in which these orders participated in the metropolis glimmer but never surface. What seems to really matter is the expansion (or the reception, depending on the standpoint) of *the* West.

The other methodological development in the history of 'sciences in the periphery' that relates closely to the theme of the present volume has to do with the role of the receiving cultures. Many of the drawbacks of the centre–periphery dichotomy are avoided by focusing the analysis on the ways in which ideas, methods, instruments, and techniques that originated in a particular cultural and historical setting are introduced in a different place with its own specific intellectual traditions, and its political and educational institutions. *New ideas are not introduced to be placed in any kind of void*; they are asked to displace other, usually strongly entrenched, systems of thought. In this sense, new ideas aim at providing alternative methods and answers to questions for which peoples and cultures *have already adequate answers*. This vantage point highlights the characteristics of the 'receiving culture' which does not act as a passive and neutral recipient of whatever is being 'received'. Not only does the transmission presuppose a selection among a whole range of different 'items', but also the 'items' that are transmitted undergo unexpected, and often startling, modifications, within the multiple cultural traditions of a specific society during a certain historical period. Particular forms of ideological resistance, the role of local scholars and audiences, the influence of the political context, and the public rhetoric of modernization are all points to be taken into account in analysing the appropriation of the scientific ideas in the various environments. The practical outcome of a historiography based on this notion of appropriation is the investigation of the discourse that is being eventually articulated in order to embody the scientific ideas within this complex intellectual and social setting.[2]

Interestingly enough, the only section of the book that seems to take into account these considerations is again the one referring to Latin America. Lafuente and Pimentel, for instance, introducing their paper observe that 'The aim of this paper is to assess how and to what extent local contexts re-elaborate and re-construct scientific practices and theories in the process of globalization of science. [...] Diffusionist theories, the automatic identification of science with emancipation or the radical oppositions such as creoles vs. metropolitans or ancients vs. moderns [...] simplify excessively realities that are actually, more dynamic, more patent, more pluralistic.' (p. 99). Also, a little further, 'That which is now noteworthy is, we believe, not the existence of good and bad scientists, the publication of praiseworthy texts or whether arrival of the ideas of authors such as Newton and Buffon took place sooner or later. The crucial point, beyond the degree of excellence achieved by science in the periphery, is the latter's ability to articulate other discourses on reality.' (p. 101). This is the spirit that permeates, although not always with the same intensity and clarity, most of the works of the Latin American section. Colonial circumstances, institutional policies, and utilitarian pursuits participate in the elaboration of local scientific discourses. However, this is not the case with many other papers. For example, the question of priority seems to draw the attention of most authors of the Eastern Asian section, while all four papers of this section devote their greatest part to the content analysis of certain works in order to trace the introduction of the 'original' scientific ideas in the territories of China. No mention, for example, is being made of the long-standing Confucian tradition or other aspects of the local intellectual life. The actors appear devoid of their own cultural traits and their function is confined to the more or less efficient agency of Western science.

The papers examining the spread of scientific ideas in the Greek-speaking world of the eighteenth century appear more sensitive in this matter. They do take into account the local social factors in the process of assimilation of the scientific ideas. However, doing so does not mean that they account for the elaboration of a local discourse, since they cannot surpass the methodological pattern of *filtering*: intellectual and material backwardness did not allow the Greek-speaking scholar to participate substantially in the ferment of scientific knowledge. For such scholars, the only participation consists in the appreciation of some general methodological principles and, especially, in the ideological use of science in order to impugn

ignorance and superstition. The authors overlook entirely the *active* role of neo-Aristotelianism and Christian Orthodox theology—that dominated the Greek intellectual life of the period—in the appropriation of the contemporaneous natural philosophy and the elaboration of a local philosophical discourse about nature. The same holds for the complex political agitations and social restratifications of the emerging Greek society, the virtual significance of which is drastically minimized owing to their subsumption under the generic idea of the expectation of national emancipation.

Notwithstanding the varied historiographic character of the works contained in it, the book has a special interest for those who study the sciences in the 'periphery'. The fact that it offers a panoramic view of three extended geographic areas is a valuable contribution by its own right. Even the descriptive approaches enrich the bibliography with significant pieces of information. The fact that a great many of the papers constitute part of long-term projects safeguards the original character of the data presented and invites the interested reader to keep up with the future developments in the field.

Three papers, which diverge from the general taxonomic schema followed in the book, add substantially to its value. A. Hessenbruch's '18th Century Science and the Nation State' examines the complex political and economical circumstances which favoured the development and the institutionalization of measurement in Scandinavian countries. A. Carneiro's, A. Simões', and P. Diogo's 'Science and Technology in 18th Century Portugal. The Naturalist Correia da Serra' exemplifies an interesting historiographic approach. The notion of *network* is personalized in the eighteenth-century naturalist Correia da Serra, whose journeys overcame the distinction between the centres and the peripheries of his age. A. E. Ten's 'Scientific Periodicals, Scientific Communities and Science Dissemination in a Peripheral Community' finally, although not exactly a typical paper, rather than an ongoing research project presentation, brings forth the discussion about the quantitative studies in the history of science. Drawing on Thomas Kuhn's taxonomical categories, the author claims quite convincingly that the study of the periodical scientific publications can offer substantial evidence about the distribution of the various communities which displayed an interest in the sciences as well as about the degree of dissemination of the scientific ideas in nineteenth-century Spain.

Although the book would have profited by the better printing of the images, fewer typographical errors, and, of course, an index, the richness of case studies and historiographic views compensates the reader for these secondary deficiencies.

[1] H. F. Cohen, *The Scientific Revolution: A Historiographical Inquiry* (Chicago, IL, 1994). C. Truesdell, 'A Program toward Rediscovering the Rational Mechanics of the Age of Reason', *Archives for the History of Exact Ideas*, 1 (1960), 3–36. Concerning the recent developments see, indicatively, *The Ferment of Knowledge. Studies in the Historiography of Eighteenth-Century Science*, ed. by G. S. Rousseau and R. Porter (Cambridge, 1980); *Newton's Principia and its Legacy*, Proceedings of a Royal Society discussion meeting, 30 June 1987, ed. by D. G. King-Hele and A. R. Hall (London, 1988); T. L. Hankins, *Science and the Enlightenment* (Cambridge, 1989).

[2] *Tradition, Transmission, Transformation*, Proceedings of two conferences on pre-modern science held at the University of Oklahoma, ed. by F. J. Ragep, S. P. Ragep and S. Livesey (Leiden, 1996); *The Intellectual Appropriation of Technology: Discourses on Modernity, 1900–1939*, ed. by M. Hård and A. Jamison (Cambridge, MA, 1998); *The Sciences in the European Periphery During the Enlightenment*, ed. by K. Gavroglu (Dordrecht, 1999).

Manolis Patiniotis, *Dibner Institute for the History of Science and Technology, Dibner Building, MIT-E56-100, 38 Memorial Drive, Cambridge, MA 02139, USA*