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Book reviews

THOMAS S. KUHN, **The Road since *Structure*: Philosophical Essays, 1970–1993, with an Autobiographical Interview.** Edited by James Conant and John Haugeland. Chicago and London: University of Chicago Press, 2000. Pp. viii + 335. ISBN 0-226-45798-2. £16.00, $25.00 (hardback).

Thomas Kuhn seems destined to be remembered for his books. Indeed, just one classic text may come to mark his place in history, even if historians of science themselves continue also to treasure his monographs on the Copernican revolution and on black-body radiation. Yet there is much that is original and important in

Kuhn's essays and articles. The early papers on thermodynamics remain wonderfully insightful even now. And some of the 'post-*Structure*' papers collected in *The Essential Tension* (Chicago, 1977), 'A function for thought experiments', for example, and 'Mathematical vs. experimental traditions in the development of physical science', have served as important independent sources of inspiration for historians and philosophers. Nothing in the present collection will stand comparison with this earlier work, but these slighter pieces of Kuhn's last years are worth having nonetheless. They include essays reflecting the strongly philosophical direction of Kuhn's work in the 1980s and 1990s, and an extended interview that may prove to be most of what he was willing to make available by way of intellectual autobiography. A complete bibliography of Kuhn's publications enhances the value of the work, but an index, unfortunately, does not.

Those familiar with Kuhn through his earlier work, including *The Essential Tension*, will find little that is new in the present essays. Mostly, they are devoted to tidying up existing ideas, to correcting misunderstandings – particularly of the notion of incommensurability – and to commentary on the work of others that uses his own established position as its frame. Nonetheless, 'Possible worlds in history of science' is a valuable inclusion, wherein Kuhn critically reflects on the causal theory of reference first proposed by Kripke and Putnam in the 1970s. Even more pleasing is the presence of an essay commenting on the work of Sneed and Stegmuller, and in particular on Sneed's *The Logical Structure of Mathematical Physics* (London, 1971). Kuhn enthusiastically welcomes Sneed's as a formal philosophy that, unlike all others, is easy to relate to how science is actually done. And he goes on to show how closely analogous it is to his own historical account – even noting at one point how the Sneed formalism successfully captures the non-routine character of normal science that other philosophers have found so difficult to grasp (p. 179, n. 3). Given that (normal) scientific research is now quite widely understood, following Kuhn, as practical activity wherein exemplary applications serve as models, it is perhaps a little surprising that the work of Sneed and Stegmuller has not been more widely influential.

Following the eleven essays is an extended autobiographical interview, recorded in 1995 and first published in 1997. It confirms, if confirmation is needed, the depth of Kuhn's respect for the natural sciences and his conviction that there is indeed something special about them. It also strongly hints at some of the difficulties with which this conviction confronted him. For although it is clear from his own account that Kuhn was not a person who settled easily into intellectual alliances or collaborations, it is also clear that he greatly desired to associate himself with, and secure the recognition of, precisely those scholars who were most alienated by his 'quasi-sociological' description of science. Indeed, it is interesting to ask why Kuhn, enthusiast for science that he was, and surrounded throughout his career by fellow enthusiasts, was never able to enact the simple ritual performances, and to utter the requisite bromides about truth, rationality, progress and so forth, that would surely have secured the unreserved acceptance that they never accorded to him. For whatever reason, he always preferred to dwell upon the problems he saw in his own convictions, and hence in theirs, which is part of the reason, perhaps, why he ended his life, as Ron Giere has noted, 'professionally homeless' ('Obituary', *Social Studies of Science* (1997), **27**, 498).

Thomas Kuhn believed that scientific research involved, in some sense or other, the systematic investigation of nature, and that any account of the growth of scientific knowledge had, as it were, to make room for nature. But whilst these are beliefs widely regarded as self-evident, Kuhn seems increasingly to have fretted about how they could be justified. His anxieties are strikingly apparent in his brief allusions to Steve Shapin's and Simon Schaffer's splendid account of the controversy between Boyle and Hobbes, *Leviathan and the Air-Pump* (Princeton, 1985). Kuhn is intensely critical of this work, despite its 'damned good' historical scholarship (p. 316) and the care with which it follows Kuhn's own methodological injunction to read history forwards and to avoid the use of the concepts and understandings of later times in making

sense of texts and records. Indeed, amazingly, Kuhn criticizes the book precisely because it avoids Whiggism of this sort: 'It upsets all hell out of me that they can't understand what everybody now learns in high school, or even elementary school about the theory of the barometer' (p. 316). Kuhn is 'bothered' because the essential role of nature in science is not, as he sees it, sufficiently emphasized and reflected upon in the book. And just how deeply he is 'bothered' is made vividly apparent by the extraordinary way in which he formulates his criticism.

It is clear that Kuhn deeply desired a general philosophical account of science that would somehow 'let nature in'. And his felt inability either to produce or to unearth one that he could accept was an evident source of distress to him. I confess that, reading this interview for the first time, this came as something of a surprise to me. For I had reckoned Kuhn an authority on the 'role of nature' and remembered contributions as different as *The Copernican Revolution* (Cambridge, MA, 1957), and 'The function of measurement in modern physical science' (1961), precisely for how vividly they evoked in the imagination the ways in which nature was indeed 'let in'. But however that may be, it is apparent that in the twilight of his career Kuhn was more and more disturbed by a problem that may have challenged and intrigued him but scarcely troubled him in earlier decades.

Just possibly, the direction taken by Kuhn's career in his last years may help us to understand why this was so. In a move the results of which are clearly evident in the essays here, Kuhn effectively abandoned history of science in favour of philosophy: 'in the last ten, fifteen years … I have just stopped reading history of science' (p. 332). Moreover, this move from history to philosophy was also a move away from the problems of scientific practice towards those of speech and language more abstractly addressed. Unfortunately, a sense of the role of nature in science is hard adequately to convey entirely through language. Indeed, my guess is that it features amongst those things that may convincingly be shown but cannot satisfactorily be stated, let alone indefeasibly established by abstract argument. Thus in abandoning history of science, and ceasing to read the kinds of account of scientific activities that had previously inspired his greatest insights, it may be that Kuhn actually walked away from the very resources best suited to addressing his problem and left him bereft of the experience that had previously sustained his ontological intuitions.

This is a depressing conjecture with which to conclude, and it is to be hoped that it is wrong. Certainly, Kuhn himself was very well aware that there were things commonly expected of language that it was simply not capable of doing: he was of course long familiar with the work of Wittgenstein and Quine, and much else of the extensive philosophical literature relevant here. And the editors tell us that his more innovative late essays, pointing towards the soon-to-be-published conclusions of his last philosophical efforts, have not been included in this particular volume. So there is a need patiently to await the work still to come, and to examine what Kuhn has to say in that work on the role of nature in science.

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Malachi Haim Hacohen, **Karl Popper – The Formative Years, 1902–1945: Politics and Philosophy in Interwar Vienna**. Cambridge: Cambridge University Press, 2001. Pp. xiii + 610. ISBN 0-521-47053-6. £35.00, $54.95 (hardback).

A disturbing feature of twentieth-century intellectual history is that the dominant figures of the two main European philosophical traditions were decidedly conservative thinkers with strong authoritarian, even totalitarian, tendencies. I refer here, of course, to Martin Heidegger and Ludwig Wittgenstein. This point is only reinforced when an avowedly liberal thinker like Richard Rorty explains the significance of his own favourite figure, John Dewey, in terms of the views he shared with Wittgenstein and Heidegger. Rorty writes as if Karl Popper had never existed. However, as Hacohen rightly observes, had Rorty taken Popper's achievement more seriously, perhaps we would not be saddled with the postmodern predicament whereby the failure to establish logical foundations for all

thought opens the door to an endless proliferation of community-based epistemic standards. Indeed, a sign of our non-Popperian times is that the most natural way to interpret the idea of 'social epistemology' is in terms of a consensus-seeking approach to enquiry, not, as Popper himself did, as a set of mutually critical agents.

Popper's invisibility from most standard histories of modern philosophy is matched only by the ideological role he has played in the history of other disciplines, especially the social sciences. In the second half of the twentieth century, Popper stood for the scientific method, objectivity, rationality, liberalism and individualism. Within academia he performed much the same function as his English mentor, Bertrand Russell, did outside it. Yet, even in death, Popper remains an awkward figure to place, both intellectually and politically. Nevertheless, Hacohen, a broadly educated intellectual historian, does an excellent job of disentangling the misunderstandings and myths surrounding Popper – many promoted by the man himself – typically by relying on evidence from archives on both sides of the Atlantic. However, in one important respect, the book's title is misleading, since the epilogue provides a thirty-page sketch of how one would research Popper's four decades as presiding philosopher at the London School of Economics. While Hacohen claims he will not do the work himself, it seems to me that it could be easily turned over to someone else.

Hacohen's account implies some fascinating differences between Popper's philosophical personality and that of his world-historic rivals, Heidegger and Wittgenstein. To be sure, these were three megalomaniacs who thought all philosophy culminated in their thought. However, by accident or design, Heidegger and Wittgenstein were surrounded by rather impressionable students who claimed genius for their master, thereby sparing the master the indignity of doing so for himself. In contrast, it would seem that from his late teens Popper was inclined to advertise his genius, which gave him a reputation for arrogance and petulance. Hacohen strikes just the right chord when dealing with this matter. Popper engaged numerous contemporaries in debate – sometimes that seemed to be the only communication of which he was capable – and then insisted on being recognized for some achievement resulting from it. This tendency especially annoyed the Vienna Circle, who first gave Popper some serious philosophical attention by publishing *The Logic of Scientific Discovery* in their book series. As the logician Tarski later put it, Popper always had the better argument but was never the nicer person. Moreover, as Hacohen shows repeatedly, while Popper learned from his interlocutors, he rarely acknowledged shifts in his position, let alone credited their sources. The potential for interpretive confusion is only compounded by Popper's otherwise admirable tendency to craft his prose as simply as possible – a strategy he picked up from Einstein's successful popularization of relativity theory.

Popper's style and practice have not stood him well in the historiography of philosophy. His dogmatic claims for his own genius were often read as philosophical dogmatism. Yet most of his positive views were really negative ones in disguise: his rationalism was anti-inductivism, his liberalism anti-authoritarianism, his individualism anti-holism, and so forth. Consequently, Popper often presented his views as critical sketches that presuppose acquaintance with the details and history of what is being criticized. Failure to appreciate the profoundly dialectical character of Popper's thought has led to his portrayal as a relatively simple-minded thinker, such as the standard-issue 'positivist' that came across to Adorno and Habermas in the *Methodenstreit* of the 1960s. Moreover, it did not help that so often Popper's adversaries were the self-declared keepers of the dialectical tradition.

This raises a more general problem in the interpretation of Popper's philosophical career: he seemed to be acutely aware of the reflexive dimension of thought without managing to escape its entanglements. An illuminating thread through Popper's philosophy would follow his interest in the ways the forms of thought undermine, or otherwise transform, its content. Hacohen observes that Popper's youthful rejection of Marx and Freud was based on the dogmatic attitude that Marxists and Freudians had towards their masters' views, not on the

actual views themselves, with which Popper remained in considerable sympathy for much of his life. However, once this rejection was canonized as 'the demarcation problem' in the philosophy of science, Popper's more globally normative, perhaps even ethical, concerns dropped out, and it became a technical matter that implied the rejection of the content of Marx's and Freud's theories. Conversely, when Hacohen reveals some correspondence between Popper and the man responsible for bringing him to the LSE, Friedrich von Hayek, it becomes clear that what Popper likes about capitalism is not its substantive fixation on free markets but the meta-level consequences of holding such a view, namely that it makes one more responsive to the external world.

The deep point in all this is that certain views may be true (e.g. Marxism), yet because of the times and places in which we live, or the sort of person we are, believing these views to be true may make ourselves and others worse people. Indeed, this explains Popper's aversion in later life to the quasi-religious appeals to unconditional commitment that Michael Polanyi and Thomas Kuhn associated with normal science. (This aspect of Popper's thought was developed in W. W. Bartley III, *The Retreat to Commitment* (New York, 1962).) Unfortunately, as long as the history of philosophy continues to be written as a set of authoritative figures who attract acolytes and spawn canonical texts, Popper will not be given his due and the critical function of philosophy more generally will remain muted. However, to Hacohen's great credit, this will be much harder to do in the future.

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Lorraine Daston (ed.), **Biographies of Scientific Objects**. Chicago and London: University of Chicago Press, 2000. Pp. x + 308. ISBN 0-226-13672-8. £13.50, $19.00.

Significant parallels between the topics investigated by historians and sociologists of science and those addressed by their colleagues in technology studies allow for a fertile exchange of theoretical concepts across these neighbouring fields of enquiry. The two spheres also differ in interesting ways, however. One of the contrasts is that a great proportion of the material objects examined by scientists are conceived of as permanent features of the world, while the whole point of technologists' manipulation of objects is to change their form and properties. Scientific objects are assumed to belong to nature, whereas in a basic sense, at least, technical artefacts, just like scientific theories, are unquestionably cultural entities. The project of constructivist science and technology studies is essentially about extending the sense in which scientific practice and technological development, and the outcomes of both, are understood to be part of specific cultures. The nature/culture dichotomy itself, however, is accepted in most of the STS literature, though some authors have maintained that scientific objects are constituted by the discourse of scientists.

As suggested by its title, that dichotomy is disputed in the volume under review. In her introduction to the collection, Lorraine Daston challenges us to repudiate the traditional epistemology which forces us sharply to distinguish discoveries from inventions, the real from the constructed and the natural from the cultural. Emphasizing that the topics addressed in the book range across the scope covered by the German term *Wissenschaft*, Daston claims that the contributions demonstrate scientific objects to be, at one and the same time, real and constructed, their reality being an achievement, increasing or diminishing as a result of the concerted actions of scientists. She does recognize, however, that the theoretical conclusions drawn from the empirical material analysed by individual authors, whose papers were all presented at a conference in Berlin in 1995, entitled 'The Coming into Being and Passing Away of Scientific Objects', are far from uniform.

The essays indeed diverge considerably. The contribution most closely related to Daston's introduction is authored by Bruno Latour, and differs from the others by being wholly programmatic in character. In this paper Latour argues that the historicity that constructivist analysts of science and technology attribute to cultural manifestations such as scientific knowl-

edge, the theory of 'relative existence' under which human achievements are habitually subsumed, ought to be extended to the nonhumans mobilized by the natural sciences. With respect to historicity, scientific objects ought to be placed on an equal footing with technical artefacts and other human constructs. Moreover, Latour contends, we have to realize that labour wholly analogous with that needed to extend technological projects and scientific objects in space is also required to establish the latter in time. Constant efforts of institutionalizing, standardizing and black-boxing are necessary not only to expand the Pasteurian programme, for instance, throughout the modern world that we share, but to make the microbes that Pasteur discovered or invented travel back in time, ensuring that they have always existed – just as it takes considerable work, both intellectual and practical, to make it possible for a bacillus discovered or invented by Koch in the late nineteenth century, to take another example discussed by Latour, to have killed Ramses II three thousand years earlier.

Progress in the historiography of science almost invariably springs from the sentiment that established practices on the part of historians do not go far enough in historicizing the material dealt with. Because what is, for practical purposes, a single programme jointly proposed by Daston and Latour is an innovative one, moreover, there is every reason to give it serious consideration. The arguments adduced in support of the programme are hardly compelling, though. A main argument points to the lack of uniformity in the way constructivists address scientific objects on the one hand and human creations such as technological projects on the other, but the mere fact that a distinction is being made does not, in itself, constitute a reason for rejecting it. So, for instance, Latour explicitly dissociates himself from the notion that technical artefacts might be made to travel backwards in time, thus positing a dichotomy between the properties of scientific objects and those of technological projects. Rather than repudiating differences in our way of understanding various categories *tout court*, then, the judiciousness of every single distinction needs to be assessed by itself, and it is far from clear that the division of largely unchanging phenomena from historically contingent knowledge about these objects is a hindrance to good practice in the history and sociology of science. Indeed, even within the covers of this volume, there is much to suggest that, on the contrary, that dichotomy is a crucial resource in constructivist STS.

As space does not allow a discussion of the individual papers of the collection, in the remainder of this review I will confine myself to a few brief remarks on the extent to which the case studies correspond to the Daston–Latour programme. The contribution coming closest to matching the objectives stated by the editor of the book is arguably an essay authored by Peter Wagner, offering historical documentation in support of the thesis that society is an entity which emerged in the early nineteenth century, concurrently with the advent of sociology, and that the last few decades may well have witnessed its dissolution. The alleged emergence and passing away of society are not assumed to have been marked by cataclysmic events; the concept simply refers to a specific way of organizing social relations which, Wagner seems to suggest, have always been there. As this case belongs entirely on one side of the nature/culture divide, however, Wagner's interpretation hardly helps to break up that dichotomy. Neither does the longest piece in the collection, a paper by Marshall Sahlins on the concept of culture. Determinedly defending the continuing relevance of that concept against anthropologist critics, and insisting that culture is a ubiquitous component of human society, this paper, as far as I can see, is beyond the scope of the book altogether.

A case from outside the social sciences in which the coming into being of new research objects is clearly demonstrated is presented by Rivka Feldhay. Feldhay offers an interesting account of how new mathematical objects were created in Jesuit seventeenth-century culture, and of how they may have been shaped by the competition for professional status between mathematicians, philosophers and theologians. It is far from obvious, though, that the entities thus brought into being have had an existence beyond the realm of discourse. Jan Goldstein's contribution, a reconstruction of how a new

manner of conceptualizing the self was established in French philosophy in the early nineteenth century, raises the same question. The reason is that Goldstein makes it quite clear that he understands the referent of the concepts whose vicissitudes he charts, the self, as a permanent object, much like Wagner's social relations.

The incongruity between the programmatic position summarized above and the historical cases presented is no less evident in the contributions devoted to topics drawn from the natural sciences. In one of those chapters Hans-Jörg Rheinberger discusses efforts to define the nature of cytoplasmic particles carried out within biochemistry and related fields from the 1930s to the 1960s. Professing to give voice to the phenomena themselves, Rheinberger offers an account of shifts that the objects of this research underwent during that period. The notion of focusing on the objects rests on a play on words, however: employing a methodology which allows him no more access to the natural phenomena themselves than historians generally have, what Rheinberger is reporting, of course, is shifting interpretations of those phenomena. The same ambiguity occurs, and is then resolved, in an essay by Ted Porter, demonstrating that the constitution of entities such as hypertension and mortality rates in US medicine during the first half of the twentieth century was partly a result of the efforts of life insurance companies to establish objective measures of health. Though discussing these developments in terms of new scientific objects being created and shaped by the actors, Porter is crystal clear that what is accounted for in his analysis is the emergence of interpretations of pre-existent phenomena. Again, the entities explained by the historian are firmly on the 'representation' side of the representations/objects dichotomy, in sharp contrast to the programme of Daston and Latour.

In sum, the programme to which this collection owes its title is one that not only raises important problems not addressed by its proponents, but is also supported by the case studies offered to a limited extent only. On the other hand, not only is a bold and highly interesting programme for constructivist STS set out in the volume, but also several very good historical papers are included, to which, of course, I have not been able to do justice in this review.

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Christoph Meinel (ed.), **Instrument–Experiment: historische Studien**. Berlin: GNT-Verlag für Geschichte der Naturwissenschaften und der Technik, 2000. Pp. 423. ISBN 3-928186-51-5. £34.00.

This volume consists of the proceedings of the 1997 annual meeting of the German Society for the History of Medicine, Science and Technology. Both the original conference and the volume aimed at an assessment of the 'new experimentalism' in the history and philosophy of science (HPS). The approximately four hundred pages of this volume hold no fewer than thirty-seven papers, mostly in German (three articles are in English). The original meeting brought together scholars of very different academic backgrounds: senior and internationally renowned scholars, graduate students and also independent scholars and amateur historians. This very diverse collection does give the volume an attractively pluralistic air, but at the same time considerably reduces its professional merits as a unified scholarly accomplishment.

The volume is divided into six parts. The first ('Historiographical and methodological perspectives') consists of four longer essays by Klaus Hentschel, Hans-Jörg Rheinberger, Otto Sibum and Paolo Brenni. Hentschel presents a detailed review of the historiography of experiments and scientific practice (pp. 13–51). This review may be very useful for the German student, but it has little new to offer to the reader acquainted with the HPS of the last twenty years. Otto Sibum presents his 'performative historiography' approach, based on the actual reconstruction of historical experiments (pp. 61–73). Such reconstructions, claims Sibum, transform textual evidence into material processes situated in space and time, thus enabling the opening of the 'black boxes' of tacit knowledge and craft skills involved in the production of knowledge. Obviously, Sibum's far-reaching claim can be questioned: performative reconstructions have

just as strong an interpretative element, and are thus no less liable to interpretative distortion than 'textual' interpretations, reflecting the reconstruction of the experiments in the imagery of the historian. Moreover, such a 'performative historiography' must posit the possibility of reconstructions in the first place, i.e. the possibility of reconstructing material conditions as well as performative reasoning similar to those in the historical situation. Hans-Jörg Rheinberger's essay indirectly challenges exactly this assumption, by insisting on the intrinsic and unavoidable tension between the proclaimed scientific pursuit of precision and the actual 'moments of tinkering and fiddling' inherent to experimental practice (pp. 52–60). Rheinberger pleads for a history of science written as a 'labyrinth of errors and confusions', and not as a rational endeavour of standardization, normalization and regulation. Viewed this way, the 'experimental system' with which the scientist tinkers and fiddles takes on the role of the mediator between the 'wild and nocturnal workshop of possibilities' and the resulting logic of justification.

Unfortunately, few of the case studies included in this volume fulfil the high historiographical and epistemological demands put forward in the opening essays. Especially problematic is the fact that many of the authors do not take the trouble to situate their papers within the state of the art of contemporary research. Nevertheless, some of the papers are of seminal importance. In the second section, entitled 'Instruments and the construction of reality', Cornelius Borck describes how the practice of measuring the electrical activity of the brain was established in the 1890s and discusses the role these measurement strategies have played in the construction of the modern science of neurophysiology (pp. 118–27). In another important essay, Friedrich Steinle tells anew the story of the galvanometer (pp. 98–108): beginning as a tool to control the activity of the battery, it soon became the generator of a new concept – Steinle terms it, after Rheinberger, an 'epistemic thing' – namely the electric current. This, in turn, made the galvanometer a central experimental system for the investigation of the electrical realm.

A somewhat disappointing collection of papers deals with microscopy ('The microscopical gaze and scientific understanding'). Microscopy presents one of the most intriguing interfaces between experiment, observation and theory in the history of the modern bio-medical sciences, but none of the papers presented here addresses these epistemological questions. Still, two of these papers deserve a closer reading: Ilse Jahn, the *grande dame* of history of biology in Germany, presents new material concerning C. G. Ehrenberg's view of micro-organisms ('infusoria') as fully fledged animals (pp. 235–41), and Ariane Dröscher gives an account of the reviving debate on the nature of the 'Golgi apparatus' after the introduction of electron microscopy in the 1940s (pp. 260–8). Disappointingly, not a single illustration is included in this section.

The last two sections are entitled 'Instruments and social practice' and 'On the materiality of instruments'. Writing the history of scientific instruments can be many things – the story of a specific instrument, the history of technological innovations, the history of instrument-making or the actual history of an instrument's role in the construction of scientific knowledge. All these and more can be found here. Thus Sabine Höhler presents her provocative thesis on the origins of instrument-based science. Reliance on balloon expeditions, Höhler argues, made meteorology a science requiring 'masculine courage and sobriety'. This masculinity, in turn, was crucial for the institutionalization of meteorology as a 'scientific' practice around 1900 (pp. 325–35). Just a few pages away, Günther Oestermann presents a four-page family tree of the Hager dynasty, a family of clock-builders in the German city of Wolfenbüttel in the seventeenth and eighteenth centuries (pp. 287–94). Again only a few pages away, one finds Volker Hess's very important discussion of the introduction of the thermometer to the clinic, and specifically the different experimental 'strategies' taken by the two physicians traditionally credited with the introduction of standard temperature measurement, Ludwig Traube and Carl August Wunderlich (pp. 316–24). One seeks in vain for a clear conceptual or historiographical frame which could make sense of the way these very divergent papers are grouped.

Of course, some degree of generosity with the volume might be in order, taking into account the diversity of its contributors and the fact that many of the papers presented are merely examples of 'work in progress', some of which were written by graduate students in the midst of their endeavours. But this does not entirely excuse the volume's weaknesses, especially as the volume also includes several very important and intriguing papers which simply disappear in the abundance of historical studies (and the lack of an index makes the volume even more difficult to use). Yet, all criticism notwithstanding, one should welcome the appearance of this volume, which at the end of the day is an important contribution to the professionalization of German HPS.

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James Rodger Fleming, **Historical Perspectives on Climate Change**. New York and Oxford: Oxford University Press, 1998. Pp. xiii + 194. ISBN 0-19-507870-5. No price given.

Historical Perspectives on Climate Change provides an account of views of climate and climate change from the Enlightenment to roughly the present time. The book provides a rich context in which to view contemporary climate change by showing us how climate change has been viewed in previous generations. The book is the first authoritative account of this kind. Notably, it provides a more nuanced and culturally embedded view of the history than predecessors, as well as overturning many of the myths that have become enshrined in the literature on the basis of less comprehensive histories. This book provides key threads in the continuity of ideas on climate and much needed enlightenment for all who have an interest in climate and its study.

Historical Perspectives on Climate Change takes up the issue of how privileged knowledge and positions on climate change were created and defined, starting with continental Enlightenment and colonial American views. Prior to the emergence of modern observing systems, the writer shows how views of climate in this period were often justified through appeals to authority and personal experience. Debate on climate focused on the relationship between climate and culture. Reviews of the influential Enlightenment scholars Du Bos, Montesquieu and Hume reveal a form of climate apologetics for culture (environmental determinism), grounded on appeals to cultural prejudice and largely unopposed. Writers of the period were aware of a moderation of the climate of Europe and attributed it largely to 'improvements' of the land through clearing and cultivation. Moving to colonial American views of climate the writer shows how this theme was embraced and amplified in service of the colonial project. The chapters on this topic trace the debate between those who felt the climate of North America was warming in response to clearing and cultivation, and those who rejected these propositions. Members of both camps began collecting (and promoting the need to make) observations of the climate.

The next chapter describes the rise of meteorological observing systems in Europe and North America in the nineteenth century, showing how this changed climate discourse. The writer places these developments and this period as the foundation for modern climate science. The chapter traces the development of state and international meteorological observing systems, marking the contributions of those who facilitated this transformation. The development of observing systems enables empirical approaches to form a second means to create privileged positions on climate, superseding those grounded on authority and experience.

The following chapters take up the writer's characterization of the third approach to developing privileged knowledge of climate, that of deductive or theoretical approaches 'drawn from physical, mathematical, geological, and astronomical evidence and principles'. This approach is elucidated via chapters on some of the seminal figures in climate research in Europe and North America: Joseph Fourier, John Tyndall, Svante Arrhenius and T. C. Chamberlin. These chapters reveal much about the influences, methods and styles of research pioneered by these figures. They also serve to set straight some misconceptions on the contributions of these figures

that have propagated through the literature on the basis of less rigorous historical accounts. These chapters are followed by an exposition on the atavism of Ellsworth Huntington, whose climatic determinism in the early twentieth century hearkens back to Du Bos and Montesquieu from the eighteenth. While the writer draws various lessons from the Huntington experience, none is perhaps more telling than the choice of title quote for the chapter from Huntington himself: 'Sometimes I wonder whether I shall someday wake up and find my whole scheme of the universe crashing down like a house of cards' (p. 95).

The final chapters review work and perceptions of climate change through the first half of the twentieth century, focusing on possible warming and cooling. This section covers in particular the contributions of Arrhenius, G. S. Callendar, Gilbert Plass and Roger Revelle. As in the previous section, these chapters illustrate the manner in which these writers responded to one another and to earlier works (or not). Together, these sections of the book trace out the development and fortune of views of the carbon dioxide role on climate, highlighting successes as well as missteps and dead ends. This section also reveals the richness of the earlier discourse on carbon dioxide and climate. Citing a newspaper article from 1950 the author notes that

> there is little that is new or unique in popular climate discourse. Topics of climate speculation cited in the article included a warmer planet; rising sea levels; shifts of agriculture; the retreat of the Greenland ice cap and other glaciers; changes in ocean fisheries, perhaps due to changes in the Gulf Stream; and the migration of millions of people displaced by climate change (p. 119).

The book closes by heralding a fourth era on means to establish privileged positions on climate via the technology of the late twentieth century: modern meteorological instrumentation, satellites, numerical and statistical methods and, crucially, computer-based climate models. There are clearly lessons in Fleming's study of the first three climate dynasties that pertain to the fourth. I can find little enough to fault in this book that any contribution of space on it here would be out of proportion. The history is very much focused on Europe and North America. That leaves room for future accounts of contributions to the history of climate from elsewhere. Notably, the first systematic meteorological observations can probably be attributed to China, going back several thousand years. As we look to coming decades and centuries, however, I can think of few better places to start than *Historical Perspectives on Climate Change*.

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Colin A. Russell (ed.), **Chemistry, Society and Environment: A New History of the British Chemical Industry**. Cambridge: The Royal Society of Chemistry, 2000. Pp. xvi + 360. ISBN 0-85404-599-6. £59.50 (hardback).

Writing a history of the chemical industry is fraught with difficulties. What approach is to be adopted? A nuts-and-bolts account of developing technologies similar to F. Sherwood Taylor's *A History of Industrial Chemistry* (London, 1957), an economic history along the lines of L. F. Haber's *The Chemical Industry During the Nineteenth Century* (Oxford, 1969) and *The Chemical Industry, 1900–1930* (Oxford, 1969), a business history (which is quite different), a history that integrates the chemical industry into the broader history of chemistry, as previously attempted by F. Aftalion in *A History of the International Chemical Industry* (Philadelphia, 1991) or an environmental history? Should it be a history of the British chemical industry (is there a British chemical industry?), the European chemical industry or the whole world? Does the account stop at a safe historical distance, say 1945, or is it necessary to cover the corporate musical chairs of the last decade which have changed the chemical industry beyond all recognition? Is a history of the chemical industry inevitably a history of the survivors or should one attempt to include companies that fell by the wayside long ago? Should one focus on the big corporations, although the chemical industry contains numerous small firms with tiny turnovers? Even defining the chemical industry is problematic. Does one include the soap industry,

the match industry or the fertilizer industry? The explosives industry might appear big enough to warrant a history on its own, yet for many years it was closely tied to chemical corporations such as Du Pont and ICI. Similarly it would not be self-evident that the plastics industry should be included in a history of the chemical industry if it were not for the close involvement of several chemical firms such as Hoechst and Dow. Furthermore, the pharmaceutical industry has always been half in and half out of the chemical industry.

What is clear, however, is that there has been no satisfactory history of the British chemical industry for many years. D. W. F. Hardie and J. Davidson Pratt published their *History of the Modern British Chemical Industry* in 1966 and while it is excellent in many ways it soon became out of date in its approach and its contents (particularly the otherwise very useful potted histories of individual companies). The late Alec Campbell wrote *The Chemical Industry* in 1971, but its appearance in a series of industrial archaeology guides limited its scope and availability. More recently, the proceedings of a Royal Society of Chemistry symposium in 1991 were converted into *Milestones in 150 Years of the Chemical Industry*, which took the unusual step of covering the topic from the viewpoint of the consumer, but it also adopted an international perspective and, as often is the case with multi-author volumes, it was uneven in scope and quality. These weaknesses were also apparent in E. Homburg, A. S. Travis and H. G. Schröter's *The Chemical Industry in Europe, 1850–1914* (Dordrecht, 1998) and A. S. Travis, H. G. Schröter, E. Homburg and P. J. T. Morris's edited volume *Determinants in the Evolution of the European Chemical Industry, 1900–1939* (Dordrecht, 1998).

There is no doubt that the volume under review, subtitled 'a new history of the British chemical industry', aims – rather self-consciously – to fill this gap, but it also attempts to plug even more glaring lacunae, by billing itself as an environmental and social history of the British chemical industry. How far does *Chemistry, Society and Environment* fulfil these ambitious aims? Certainly the authors are eminently qualified to produce such a book. Colin Russell has studied the history of the chemical industry for many years and has paid particular attention to the wretchedly depleted records of the industry. Alec Campbell (who, sadly, died as this book was going to press) is well known for his work on the early chemical industry and Noel Coley has published on pharmaceutical history as well as being co-author (with Gerrylynn K. Roberts and Colin Russell) of *Chemists by Profession* (Milton Keynes, 1977). Sarah Wilmot pioneered the environmental history of the chemical industry while she was a Leverhulme Research Fellow at the Open University.

In a short review, it would be tiresome to outline the contents in full. Let it suffice to say that its coverage is comprehensive. Unusually, it opens with a short chapter on the 'Records of the British chemical industry' including the secondary literature. The second, equally unorthodox chapter, 'The shape of the British chemical industry', gives an overview of the industry by technical sectors and attempts to deal with some of the definitional problems I have mentioned. Although this is a useful scene-setter in many ways, I would have liked to have read more about the economic and business aspects of the industry. The following chapters cover the history of the British industry from the Roman occupation up to about 1960 (there is no sharp cut-off point). For the period after 1800 or thereabouts, the chapters are by industrial sectors (alkalis, organic chemicals, polymers) rather than chronological. Metal extraction and refining is also covered, inevitably rather cursively and it could be argued that the space it takes up should have been given over to other more obvious sectors of the chemical industry. The final chapter, 'Chemical industry and the quality of life', discusses some of the key environmental issues such as ozone depletion and 'health and diet', not a self-evident topic for a history of the chemical industry.

So how far is this a truly path-breaking history of the British chemical industry and how well does it fulfil its remit to be a history of 'chemistry, society and the environment'? Surprisingly, given the authors' association with the Open University, there are relatively few illustrations and none in colour (except for the

frontispiece portrait of the ninth Earl of Dundonald), and they are mainly the 'usual suspects', except for the industrial archaeological photographs. Many of the illustrations are also difficult to make out as they are both small and rather dark. Furthermore, the coverage of many important topics is very brief. The major plastics are treated in short, very cursive encyclopaedia-type entries. Photographic chemicals are given just over half a page, much of which is taken up by a table of chemicals and the rest by three-line histories of Johnsons of Hendon and May & Baker. If every topic was covered in depth, there is a danger of producing a six-volume history, *pace* Williams Haynes's *American Chemical Industry* (New York, 1945–54), that might never be completed and certainly would never be read, but steps should have been taken to cover a smaller number of topics in more depth and to provide an integrated analysis of each sector rather than breaking them into small factual chunks. Alec Campbell's chapter on the alkali industry shows how this can be done. It is well integrated and provides an excellent account of the technological development of the industry that complements *Chemical Foundations: The Alkali Industry in Britain to 1926* (Oxford, 1980), Kenneth Warren's description of the same industry from the viewpoint of the geographer.

Leaving aside the environmental aspect for the moment, it has to be said that there is little in *Chemistry, Society and the Environment* that has not been covered elsewhere. Much of the early history was already described by Campbell in his *Chemical Industry* and by the Clows in their reprinted classic *The Chemical Revolution: A Contribution to Social Technology* (London, 1952; reprinted Reading, 1992). The technological details of the later period can also be found in Hardie and Pratt's book, B. G. Reuben and M. L. Burstall's excellent book *The Chemical Economy: A Guide to the Technology and Economics of the Chemical Industry* (London, 1973), or even in Sherwood Taylor's history. The book's coverage of polymers and petrochemicals is similar to *The New Chemical Industry*, Block 6 of AST281, *Science and the Rise of Technology Since 1800* (Milton Keynes, 1973), and the excellent survey by Frank Greenaway and his colleagues, 'The Chemical Industry: Part II, Raw Materials for Organic Chemicals' in T. I. Williams (ed.), *A History of Technology*, *Volume VI: The Twentieth Century*, c. *1900 to* c. *1950* (Oxford, 1978) (pp. 514–69). Colin Russell does break new ground by covering chemical products from natural sources such as wood, but even here a fresh perspective is lacking. Although the authors chide their predecessors for not saying anything about the impact of the industry on British society, there is not much more in this volume before we reach the final chapter. Campbell wrote a section about the alkali workers and mentions the dangers of nineteenth-century pigments, topics he had covered well elsewhere, but otherwise I could only find a short apologetic section on the problems of the pharmaceutical industry including thalidomide, halothane and over-prescribing of drugs.

One might have hoped that the environmental aspects could have been fully integrated into the historical account. Perhaps, in practice, this is too difficult until a much larger body of environmental history exists. The environment enters into several chapters, notably Colin Russell's chapter on the (earlier) organic chemical industry. His account of the problems created by the coal-tar dye industry is excellent, although he does not attempt to establish why many manufacturers caused pollution or exposed their workers to hazards. Similarly, Alec Campbell gives a colourful description of the pollution caused by the early alkali industry and how manufacturers sought to solve these problems (if only for financial reasons). In the final chapter, Noel Coley and Sarah Wilmot tackle the environmental question in general terms. This is by far the best and most original chapter in the book, the organization of the material is excellent and most of the key issues – in terms of costs and benefits – are covered. Their discussion of DDT, for instance, is very clear, but it is a pity it is spread over two sections. The two authors use measured, one might say diffident, language – reminiscent of reports produced by royal commissions – which will not win over readers already concerned about the behaviour of the chemical industry.

Chemistry, Society and Environment is clearly

written and nicely printed (though personally I do not care for the broad left-hand margins) and it provides a handy introduction to the history of the British chemical industry in a single volume. Perhaps it will be regarded by later historians as a transitional work, one that recognized the need to address environmental and social issues in a history of the chemical industry, without being able to do so completely successfully. It is an accessible book, whatever its shortcomings, and I hope it will increase interest in the history of one of Britain's most important industries among academics and the public at large. The authors have taken great pains to be 'fair', nonetheless many members of the public (and even some historians) will regard it as 'propaganda'. I am not sure how this unfavourable perception of the chemical industry can be changed unless we begin with the industry's problems and failures, rather than apologetically tucking them away into the final sections.

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Gerard L'E. Turner, **Elizabethan Instrument Makers: The Origins of the London Trade in Precision Instrument Making.** Oxford: Oxford University Press, 2000. Pp. xiv + 305. ISBN 0-19-856566-6. £79.50 (hardback).

One might not feel that the writing of catalogues was a difficult matter. The description of scientific instruments appears to be a relatively straightforward task. However, really good versions of such catalogues are few and far between. Indeed, comprehensive catalogues of any description are hard to find. Twenty years ago, F. A. B. Ward's *A Catalogue of European Scientific Instruments in the Department of Medieval and Later Antiquities of the British Museum* (London, 1981) was severely criticized in a review by Anthony Turner for the lack of information, poor scholarship and paucity of plates. Since that time, few attempts at cataloguing instruments have been made. The Whipple Museum has produced a number of useful descriptions of sections of their collection, yet these are essentially little more than handlists. Two important works on the development of the ivory sundial trade have included extensive catalogues of British and American collections.

However, the situation has begun to change in the last two or three years with the appearance of a number of reliable catalogues. Leading the way in this respect have been *Western Astrolabes* (Chicago, 1998), in which Roderick and Marjorie Webster have detailed the holdings of the Adler collections, and Elly Dekker's exemplary *Globes at Greenwich* (Oxford, 1999).

Gerard Turner's work on the instrument-makers of Elizabethan England is another such detailed catalogue, treating the work of a group of makers, rather than a particular form of instrument. The catalogue is introduced by a series of short chapters detailing various aspects of the English instrument-making trade during this period. The first chapter briefly discusses the introduction of instrument-making to Britain and its development well beyond the end of the sixteenth century. In the second chapter, Turner provides information on the individual craftsmen with particular attention to their methods of engraving, which have been so important in identifying unsigned instruments. Since almost all of the makers featured were also book and map engravers, a third chapter is devoted to providing brief entries on the surviving examples of the pictorial work of Gemini, Cole, Ryther, Whitwell and Beckit. Chapter 4 investigates the instruments themselves, considering various aspects of their construction and functions. Most of the chapter is devoted to the astronomical compendium, which remains the centre of the corpus of these makers' work. However, I felt that clearer explanations of the use of some of the other instruments, particularly the sector, would have been a helpful addition.

The final chapter considers the importance of Sir Robert Dudley as a patron of the trade and the group of instruments taken by him to Florence in 1606. This group has tended to form a central focus of the book. They are all very interesting items in themselves but the effect is to skew the study away from the makers and towards Robert Dudley. Indeed, Dudley's biography is a good deal more detailed than those of the instrument-makers, and one feels that Turner finds more interest in this story than in the lives of the makers themselves.

The remaining two-thirds of the book are devoted to a catalogue of 103 instruments, and the importance of this contribution to the study of instruments cannot be overstated. For the first time, all known instruments by sixteenth-century English makers are gathered together in one volume, allowing a useful comparison to be made. Several items by the seventeenth-century maker Elias Allen are also included, to allow their study in association with his master Charles Whitwell, and others of Allen's predecessors. Each item is described in great detail, with ample illustrations to support the text (although one or two items are, unfortunately, not illustrated). Introductory material to each entry provides information on the date, maker, signature and size of the object; the entry concludes with details of provenance, location and useful literature. Tables of latitudes, perpetual calendars and star positions are reproduced carefully to permit comparison of the information provided here with contemporary and modern data.

While the catalogue is admirable, I had several minor problems with its presentation. In particular, the complex terms used for some of the descriptions would be off-putting to a scholar unacquainted with the field: the book would have benefited from an accompanying glossary. Similarly, the lack of translation of some town names causes problems for a modern reader: how many would be aware that 'Lutecia' (no. 83, p. 255) was a former name for Paris? Perplexing also is the lack of indication in the header material for the entries of the location of the signature: one is obliged to search through the whole of the description to extract this information. Nor can I understand why some of the tables (such as the tide tables on the Cole and Kynvyn compendia) are not reproduced, when their information is as relevant as the tables of latitudes, which do appear. Lastly, Professor Turner has, at times, been poorly served by his typesetter, with various of the details in the transcribed tables being incorrect when compared with the photographs of the instruments. In such a catalogue, where attention to detail is so high, it is unfortunate to find that we do not know if the transcriptions can be trusted for their accuracy or not.

An appendix provides much interesting information on Turner's methods for analysing unsigned instruments, and making attributions to particular makers. The book concludes with a description of the means for capturing and storing images of the engraved characters on computer – a laudable addition, but one which will become obsolete long before the book itself, due to the rapid changes in computer hardware and software.

I have discussed this work very much in terms of the catalogue of instruments, since this is the most important aspect of the book. However, the title promises so much more, suggesting a much fuller discussion of the makers themselves, their place in the contemporary society and their relationships with the mathematicians and natural philosophers of the time. That book still remains to be written, but the current work provides an extremely useful introduction to the subject.

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Robert Fox (ed.), **Thomas Harriot: An Elizabethan Man of Science**. Aldershot: Ashgate, 2000. Pp. xii + 317. ISBN 0-7546-0078-5. £47.50 (hardback).

This book brings together the first ten Thomas Harriot Lectures, which have been held annually at Oriel College, Oxford, since 1990, thanks initially to the auspices of the late Dr Cecily Tanner, and subsequently the continuing support of the Renaissance Trust and Lord Egremont. The generally high standard of the lectures and the evident historical importance of Thomas Harriot (*c.* 1560–1621) ensure that this is not just a book for the Harrioteers, as his devotees like to be known. The ten lectures all focus on different aspects of Harriot's work, with the exception of two maverick pieces. The first of these is a typically elegant essay on the Huguenot physician Theodore Turquet de Mayerne, who treated Harriot's cancerous nasal polyp, by Hugh Trevor Roper. Taking its starting point from the remarkably warm and personal closing words of a letter from Harriot to his physician, Lord Dacre seeks to explain Harriot's sympathy by examining Mayerne's personal circumstances at the time the letter was written. The other maverick piece is an interest-

ing survey of the history of atomism up to and including Harriot's time (but saying little about Harriot's attitude to atomism) by John D. North. Although somewhat idiosyncratic, North's erudition and thoughtfulness ensure that this is still an extremely useful article.

The book begins with two predominantly biographical chapters. One, by David B. Quinn, concentrates upon the period when Harriot was Sir Walter Ralegh's man, and one, by Gordon R. Batho, concentrates on the period when he was the pensioner of Henry Percy, the 'wizard' Earl of Northumberland. Quinn sees Harriot as a 'problem solver' whose linguistic and cartographic abilities were recognized by Ralegh as useful for overcoming the problems of establishing and maintaining the attempted colony in Virginia. Batho provides a survey of the work which Harriot pursued under the patronage of Henry Percy. These are followed by three papers which seek to understand Harriot's significance in the history of early modern science. Hilary Gatti reiterates claims which she has made elsewhere, which see Harriot as an English exponent of Italian Renaissance naturalism, and especially of the philosophical cosmology of Giordano Bruno. Proceeding mostly by considering not only what Harriot wrote but also what he seems to have read (or at least knew about), there is much of value in what Gatti has to say but, as a number of the following lecturers point out, she sometimes tries to build too much upon passing comments. But perhaps the main problem with Gatti's piece is that, as its title announces, it takes it for granted that Harriot had a natural philosophy. One of the most important things to emerge from the following two articles, by Stephen Clucas and J. A. Bennett, is that it is by no means obvious that Harriot was in any historically significant sense a natural philosopher.

What Clucas's and Bennett's lectures both clearly show is that Harriot was first and foremost a mathematical practitioner. One of the most significant of recent developments in our understanding of the nature of the scientific revolution has been a new awareness of how the disciplinary boundaries between mathematical practitioners and natural philosophers weakened and then fundamentally changed. At the beginning of the sixteenth century there were mathematical sciences and there was natural philosophy, and they were very different from one another. Before the end of the seventeenth century, however, as we all know, it became possible to talk of the mathematical principles of natural philosophy. As a number of scholars have recently and brilliantly shown, this was no small change. What Clucas and Bennett do in their articles is to show how Harriot can be used to illustrate and further substantiate this change. Ever since 1611, when Sir William Lower lamented the fact that Harriot's discoveries were taken away from him as they were independently discovered and published by others, Harrioteers have been exasperated by their hero's seeming failure to understand the riches he had in his grasp. This collection, and especially these two central papers by Bennett and Clucas, strongly suggest that Harriot was hardly ever concerned with natural philosophy, and consequently was never tempted to pursue the implications that his observations or calculations might have had for traditional natural philosophy. In this regard, as John North pointed out in a paper published in 1974, he makes a striking contrast with Galileo. Clucas takes North to task here (p. 102) for what he sees as a Whiggish approach, but North was surely right to note the matter of fact (as he defends himself on p. 189): observations of sunspots clearly did not have the same significance for Harriot as they did for Galileo. But Clucas and Bennett reveal why. Galileo wanted to use these observations and his mathematical calculations to reform natural philosophy; Harriot just wanted, as Quinn might have said, to solve more problems or, as Clucas suggests, to apply his 'instinctive ability to quantify and calibrate a particular problem' (p. 119). Unlike Galileo, then, Harriot can be seen as an old-style mathematical practitioner, satisfied with what he did and almost completely unconcerned with extending his work towards, or turning it into, natural philosophy.

It is a pity this problem-solving, non-philosophical aspect of Harriot's mathematics was not addressed by Muriel Seltman in her reassessment of Harriot's work in algebra. Seltman is concerned to understand what Harriot actually achieved in algebra and how those achievements were represented in the posthumously printed *Artis analyticae praxis* (1631), which was com-

piled from his papers by his friend Walter Warner. Seltman does not suffer fools and makes no concessions to them (at least not if they are as mathematically foolish as this reviewer). If you do not understand the difference between the algebraic zero and the arithmetical zero (p. 169), this is not the paper for you.

John J. Roche offers a history of Harriot scholarship, including brief discussions of various controversies: did Harriot introduce tobacco into England, as Mayerne said? Was he an atheist? Were his mathematical papers publishable? Were they worth publishing? The book is brought to a successful conclusion – apart from three extremely useful appendices on portraits of Harriot (or not), his manuscripts and recent secondary literature on Harriot – with a study of what can be said about Harriot's religious views, by Scott Mandelbrote. This is an extremely judicious and well-argued piece which seems to provide the full measure of the debate and to reach the safest conclusion, which is that Harriot was no atheist.

This is an excellent collection. Perhaps the bringing together of these assessments of this undoubtedly great figure in the history of science will prove the turning point in Harriot studies. The Harrioteers have done enough over the years to establish Harriot's historical importance, so that his name has to be invoked by historians of science of the period, even if only *en passant*. What is required now is for his historiographical importance to be established. Maybe the realization that he can be used to show how very different high-achieving mathematical practitioners who did not share an interest in natural philosophy can be from a Galileo, or a Descartes, will do the trick.

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E. C. Spary, **Utopia's Garden: French Natural History from Old Regime to Revolution**. Chicago and London: University of Chicago Press, 2000. Pp. xv + 321. ISBN 0-226-76863-5. £16.00, $25.00 (paperback).

To a group of people in eighteenth-century France, Utopia had a definite location, even more so its garden, as this lucid study convincingly shows. Examining the cultural and political setting of natural history and the self-presentation of a group of French naturalists in the eighteenth century, it locates this garden in the Jardin du Roi in Paris. Formerly a small medical garden and transformed into the Muséum d'histoire naturelle in 1793, by 1800 it had become the leading public establishment of natural history. How is a site like this to be adequately described? As a complex of buildings and grounds, as an institution with its records and regulations, as the centre for the making of French natural-history knowledge or as the outcome of interactions of human beings? All of these definitions have been utilized to come to terms with the transformations the Jardin du Roi and the practice of natural history in France underwent under the Old Regime up until 1789 and under the new regimes during the Revolutionary period. This book discusses the interactions and definitions of the naturalists working at the royal Parisian botanical garden.

It was in the second half of the eighteenth century that the Jardin was transformed into the centre of French natural history. Its naturalists acquired a prominent place within a scientific community, whose subject matter was discovered to be extremely valuable to the monarchy and the land-owning aristocracy. When the Revolution started out to reform society according to its ideals, symbols of the monarchy and the leisured elite were ruthlessly attacked and all of the scientific institutions closed, with one notable exception: the Jardin, later Muséum. Plans were even made to enlarge considerably the grounds and the variety and number of objects on display in order to turn the garden into a site of Revolutionary transformation – of individuals as well as of society at large. How did the Jardin's naturalists promote the institutionalization of natural history after 1750? How did they manage to cater for radically different regimes? Emma Spary offers answers to these and connected questions by analysing the intellectual, political and physical space of the Jardin as constructed through the negotiations and associations of the scientific practitioners working there. She pictures natural history as a network, concentrates on that element of scientific practice which aims at constructing consent within the scientific community and at securing

support in the political sphere. Since historiography on French natural history of that period has, to a large extent, dealt less with the institutionalization of the science and more with problems of classification and nomenclature, this approach not only illuminates the variety of activities the Jardin's naturalists engaged in but also demonstrates their remarkable ability to manage them well.

Most prominent among the naturalists were Georges-Louis Leclerc de Buffon, the Jardin's intendant from 1739 to 1788, and André Thouin, head-gardener from 1764 to 1793. Buffon emphasized the utility of natural-history knowledge and its emotive appeal. In his celebrated *Histoire naturelle* he presented nature as constantly degenerating, and at the same time offered an antidote in the form of science effectively combating this tendency. Naturalists, portrayed by him and others as able managers, had the expertise to contribute to the improvement of nature and in this way to the improvement of society. As intendant Buffon had considerable control in the Jardin, which he utilized to advance his own protégés. An intricate system of patronage power plays resulted from this that went far beyond nepotism and a struggle for riches, power and prestige. At the time of the Old Regime this system offered scientific practitioners a means to continually construct and reconstruct social status and, more basically, to do business. André Thouin was a master in making both functions of the patronage system work to his – and French natural history's – advantage. He constructed a network of correspondence reaching far beyond the boundaries of Europe. Patronage and instructions to plant collectors sent overseas enhanced his control of the increasing flow of new species into France and in this way underpinned the naturalists' position as experts in creating or safeguarding order in the natural as well as in the social and economic world. However, as Spary does not forget to point out, travellers and specimens could only be controlled to a certain extent, even by Thouin. In the long run, they acted as potent agents of change in European society.

The chapters on French natural history of the Old Regime are full of interesting insights. This is even more true of the sections on the evolution of ideas, self-presentation and language after the Revolution, in short, on the way the naturalists adapted to various new regimes. The scientists adopted a political language, worked out a political agenda for natural history and in this way convincingly presented the Jardin/Muséum to the Assemblées and later to the Jacobins as a powerful instrument for Revolutionary projects of a far-reaching transformation of society. Whereas historiography has hitherto tried to assign naturalists to specific political groups, Spary accentuates the openness of their political affiliation, in fact, the versatility of their self-image as managers of nature and economy and society. Here lies the key to an understanding of their adaptability to radically different regimes. After 1789 they were able to convert monuments of despotic luxury into representations of public virtue, to persuade deputies that, as citizens in constant contact with the best and most effective teacher of all, nature, they had cherished the revolutionary values of liberty, nature and justice all along. In fact, the Jacobins were willing to accept the claim that the Muséum as a site of disciplined visions of possible, i.e. desirable, futures and the naturalists' talents as instructors went a long way in bringing about the ideal, the utopian society there and then. Robespierre envisioned a public controlled by art and its emotional response to nature. The Muséum d'histoire naturelle, as the professors did not hesitate to point out, was the ideal place to instruct people in the latter. The public would be morally and physically re-created in the encounter with nature in the Muséum. Where else could utopia be created if not in its garden?

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Brian Dolan (ed.), **Malthus, Medicine, & Morality: 'Malthusianism' after 1798**. Wellcome Institute Series in the History of Medicine: Clio Medica, 59. Amsterdam and Atlanta, GA: Rodopi, 2000. Pp. v + 232. ISBN 90-420-0851-2. £42.00 (paperback).

From life assurance calculations in late Georgian Britain to the eugenics movement in America, the work of Thomas Robert Malthus has informed an immense variety of work, some of

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which we might believe to be enormously benevolent and some of which we tend to dismiss as totally immoral. Like the changing uses of Malthusianism, there now exist countless modern commentaries on the topic itself, many of which are fine accounts and too many of which, if we cannot consider them immoral, we can dismiss as irrelevant. Part of the problem with many of the uninspiring accounts of Malthus and Malthusianism is their narrow focus, often primarily on economics. Brian Dolan, however, rejoices in the relatively recent rehabilitation of Malthus, especially because his works are now studied in relation to a broad range of disciplines. Nevertheless, we are assured, the study of Malthus's work in relation to medicine and ethics remains grossly understudied. A remedy is provided in the form of a book with nine chapters, the end product of what began as a one-day conference on Malthusianism at London's Wellcome Institute.

Most papers in *Malthus, Medicine, & Morality* are highly sensitive to both context and geography. The first two chapters are representative of this worthy trait. By paying close attention to Malthus's travels to Scandinavia during the years between the first and second editions of his *Essay on Population*, Dolan's own contribution traces the transformation of the *Essay* from a theoretical treatise to what he calls a kind of travelogue. Similarly, Timothy Alborn follows Malthus to Haileybury College, an educational facility outside London that trained adolescents to serve in the East India Company and where Parson Malthus worked for a number of years. Concentrating not upon the *Essay* but upon his 1817 defence of the college, Alborn uses Malthus's *Statements Regarding the East-India College* simultaneously to reconsider Malthus's changing views on consumerism and moral restraint and to link together high finance, empire and atonement *à la* Boyd Hilton.

Like Alborn's chapter, the contribution of Brian Young shows how the study of political economy informed some of the greatest moral questions of the day, particularly those related to theology and the cure of souls. Young revives the political turmoil in 1790s Cambridge and reminds us of the philosophic radicals to which Malthus was exposed at Jesus College in order to weave together an intriguing story about the connections between natural philosophy, religious practices and the corporeality of humans and minds.

Other papers in the collection also resuscitate Malthus and Malthusianism in relation to the political ferment of late Georgian Britain. Roy Porter's discussion of 'Malthouse' hops along with characteristic vigour and wit; examining some of the responses of early nineteenth-century physicians to the *Essay*, Porter recaptures some of the anxieties concerning the work of Malthus which we may now find quaint or irrelevant but nonetheless exemplify the diverse ways that scholarly works would be analysed during the first quarter of the nineteenth century.

The remaining papers choose not to follow the footsteps of Malthus himself but rather the changing uses of, and reactions to, his work. Bob Young – in the most normative (and the least understated) of the contributions – tries to settle an old score about the impact of the *Essay* upon another Cambridge alumnus, Charles Darwin. Young chastises those who have tried to drive a wedge between Malthus and Darwin because they did not want the 'pure' science of evolution polluted by 'ideology', but he also argues against the notion that the duo of Malthus and Darwin reduces human morality to a kind of fatalistic determinism.

Like Roy Porter's chapter, a number of other papers concentrate upon reactions to Malthus's work by medical practitioners. In their study of 'Malthus and the doctors', Christopher Hamlin and Kathleen Gallagher-Kamper concentrate on two case studies – the outbreak of fever in Ireland in 1817 and debates about Poor Laws in Scotland – in order to underscore how issues of public health were inescapably informed by Malthusianism, no matter how nebulous the term 'Malthusianism' might be. Narrowing her field of focus to the topic of birth control, Lesley A. Hall produces a similar message about the relevance of Malthusianism to public health.

While Hall's *longue durée* story includes some penetrating insights about British medicine, such as the troubles that Victorians had in reconciling their values of prudence and foresight with sex, or the ruthlessness of abortifacient manufacturers, Angus McLaren concentrates upon a much narrower topic, namely the work of the French radical and *fin-de-siècle* bricoleur Paul

Robin. Robin, we learn, contrasted with most English neo-Malthusians in that he portrayed birth control as a means by which the lower orders could prime themselves for the toppling of capitalism, and not the means by which the poor might adjust to the demands of modern, capitalist economies. In the final chapter, a recurrent theme in the book – are the lower orders immoral breeders, or is the system immoral? – is recapitulated by Antonello La Vergata. Although he brings us, and Malthusianism, into the twentieth century, we are reminded of the persistence of theodicy and natural theology in debates about Malthus.

Brian Dolan's outstanding choice of contributors and his steady editorial work leave little to gripe about. In the past, editors, when dealing primarily with a particular work, have often struggled in an attempt to create utterly coherent, unified and authoritative accounts. Thankfully, this is no longer the case, especially in this instance; for when considered as a whole the strength of this book lies in the disparate ways in which Malthus's works were interpreted and harvested by others. The volume might not bring together its intended themes quite as consistently as, say, *Science Incarnate*, but the differing historiographical sensitivities of its contributors do not detract significantly from the volume. While economists, theologians, evolutionists, radical bricoleurs and physicians all found Malthus relevant to their interests, so too should a wide range of academics – be they historians of medicine, sociologists or cultural historians – find this book on Malthusianism relevant to their scholarly interests.

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June Z. Fullmer, **Young Humphry Davy: The Making of an Experimental Chemist**. Memoirs of the American Philosophical Society, 237. Philadelphia: American Philosophical Society, 2000. Pp. xvi + 385. ISBN 0-87169-237-6. $30.00 (hardback).

Readers of *BJHS* will need no introduction to Humphry Davy, and neither will they need to be reminded that, despite – or perhaps because of – his diversified interests and massive achievements, we have imbalanced and out-of-date full-length biographical studies of him. Harking back to 1831 we have John Ayrton Paris's 'official' but uncritical *Life of Sir Humphry Davy*; 1836 saw the arrival of his brother John Davy's devoted *Memoirs of the Life of Humphry Davy*; Henry Mayhew's fanciful *The Wonders of Science, or Young Humphry Davy* appeared in 1860; within some living memory we have Anne Treneer's *The Mercurial Chemist: A Life of Humphry Davy* (London, 1963), Harold Hartley's *Humphry Davy* (Wakefield, 1972), David Knight's *Humphry Davy: Science and Power* (Oxford, 1992), and the unpublished Ph.D. theses by Judith Goodstein, 'Sir Humphry Davy: chemical theory and the nature of matter' (1969) and Richard Ziemacki, 'Humphry Davy and the conflict of traditions in early nineteenth-century British chemistry' (1974). We also have, as anyone who has worked on Davy will know, June Fullmer's many articles on his life and career. This book, which was intended to be the first of a multi-volume biography, is the result of years of research by an eminently qualified scholar, who sadly died as this, her first volume, was in press.

The book is divided into three sections, covering Davy's years in Penzance, Bristol and (in one short chapter) his arrival in London – roughly the first twenty years of Davy's life. A brief introductory chapter runs through some of the ways that Davy has been portrayed in the pages of history by different interest groups – from literary critics to historians of science and technology – and leads Fullmer to acknowledge that his various achievements have by no means been neglected by scholars. But, she says, a 'fundamental mystery' remains: 'How could Davy achieve so much so quickly?' (p. 5). Her goal in this biography is to try to uncover what made Davy grow into the famous experimentalist we are so familiar with – to find the 'root' which all biographers search for in their subjects. To do so, Fullmer here focuses on Davy's early, less familiar, life. Being careful not to speak teleologically of a heroic genius in the making, she represents Davy as, it seems, those who were close to him saw him in his youth: wide-eyed, inquisitive and a touch precocious.

A chapter on 'Birth and family' presents us with glimpses of Penzance, Cornwall, around the

time Davy was born in 1778. We are introduced to an important family friend, the surgeon-apothecary John Tonkin (referred to as Davy's 'surrogate father' by Fullmer, who also inaccurately repeatedly refers to him as 'Dr Tonkin'), with whom Davy lived and received his education for ten years. Later, when dealing with 'Mentors and friends', we are introduced to the ironmonger and instrument-maker Robert Dunkin, who encouraged Davy to explore his religious convictions, and the wealthy, mathematically minded dilettante Davies Giddy (later Gilbert, who was to succeed Davy as President of the Royal Society). These are, of course, important people to be introducing to this study, but at times Fullmer appears to go off on tangents, such as with the discussion of Tonkin's mayoral responsibilities or the rather indiscriminate discussion of political unrest around 1800 in the middle of her chapter on Davy's friendship with Coleridge. Elsewhere, central issues are skimmed over too quickly, including her passing reference to the early financial investment in a 'pneumatic establishment' by those Davy was befriending. This cropped up in her chapter dedicated to Thomas Beddoes, about whom I feel we do not need a special chapter, especially at the expense of taking the opportunity strategically to uncover the common concerns amongst the members of the Lunar Society which will soon lead to Davy's first job.

Sometimes the brevity of the chapters (a few are only about two pages long) provides what feels like anecdotal reading rather than fully developed biographical analysis. Mentioning that Tonkin allegedly complained about the noise Davy made performing 'chemical experiments' in his bedroom, which Fullmer gleaned from John Davy's *Memoirs*, for example, also betrays a sense of desperation to provide signposts of what went into the 'making of an experimental chemist'.

Her analysis, however, begins to take shape in her chapter on Davy 'Beginning chemistry', which introduces readers more directly to what she identifies as the early influences on his commitment to science. The two dominant sources informing him, Fullmer informs us, were the two-volume *Dictionary of Chemistry*, compiled and written by William Nicholson (founder of the *Journal of Natural Philosophy*, *Chemistry and the Arts* in 1797), and Lavoisier's *Traité élémentaire de chimie* (1789). Davy was nineteen when he started his systematic chemical research, particularly into the Lavoisian views on the nature of combustion, and soon 'identified himself as a "pneumatic chemist"', notes Fullmer, 'until the products of the Voltaic pile claimed his full attention' (p. 53). It is, therefore, principally Davy's and others' work in pneumatics that occupies Fullmer's attention in the volume, ending just as phase two – the 'Voltaic phase' – begins in London. This discussion weaves in and out of section two of the book (which, curiously, begins anew with chapter one).

This (essentially) two-part approach to organizing Davy's early life geographically, then thematically by chapter – almost alternating between friends and family and the topics of his work – leads to a feeling of disjointedness in the analysis. It seems strange, for instance, that Chapter 6 in Part II of the book begins with 'standards' of evaluating Davy's qualifications to work with Beddoes, something that could have been placed in 'A job offer', seven chapters back.

Fullmer does do well, however, to cover many areas of Davy's researches which continually formed parts of his chemical philosophy, such as his theory about how the purity and quantity of oxygen in an atmosphere affects physiological traits in populations. I only wish the analysis of these less well-known elements of Davy's work, including his whole interest in medical philosophy, was further developed. Some twenty years ago, Michael Neve (in his article 'The young Humphry Davy', cited by Fullmer) pointed to a curious link between the beginning and end of Davy's career, connected, said Neve, by his commitment to establishing a 'Romanticist view of life'. Neve began to search for the roots to young Davy's philosophical programme by looking at his interest in medicine and his relationship with provincial dissenting doctors. The task, sadly, has never been followed up. It is unfortunate that we are denied the chance to see how June Fullmer would have tackled this enigmatic part of Davy's philosophy had she lived to complete her multi-volume biography.

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PAUL KNIGHTS (ed.), **The Manuscripts of Michael Faraday (1791–1867) from the Collections of the Royal Institution – the Institution of Electrical Engineers – the Guildhall Library.** Wakefield: Microform Academic Publishers, 2001. 23 microfilm reels. £1058.00 (complete), £46.00 (individual reels).

FRANK A. J. L. JAMES, **Guide to the Microfilm Edition of the Manuscripts of Michael Faraday (1791–1867) from the Collections of the Royal Institution – the Institution of Electrical Engineers – the Guildhall Library.** Wakefield: Microform Academic Publishers, 2000. Pp. 94. ISBN 1-851170-31-6. £10.00 (paperback).

One of the most profound changes we are currently witnessing in the practice of history is the rapid increase in the availability of primary source material in both searchable electronic form and on microfilm. A number of ongoing projects are bringing to the historian's fingertips large quantities of manuscript and published material that was previously both difficult to locate and often required expensive and time-consuming visits to far-flung libraries. Not only are scholars gaining ready access to increasing quantities of source material, but many questions that once frustrated the historian are now potentially soluble with the aid of electronic searches. Faraday scholars have benefited more than most from this increase in available source material. His main published works are appearing in new editions, his collected correspondence (under Frank James's editorship) is in progress and several of his more substantial manuscripts – such as his 1822 'Chemical notes', his tour of Wales and his travels in Europe – have recently been published in annotated editions. Yet, until the publication of this microfilm edition, Faraday scholars had to visit several London archives to consult the bulk of his scientific manuscripts.

The coverage is impressive. This twenty-three-reel microfilm edition contains a high proportion of the surviving scientific manuscripts; it comprises the complete Faraday holdings at the Royal Institution, the Institution of Electrical Engineers and the Guildhall Library. The inclusion of the few relevant manuscripts in the library of the Royal Society, one of which has recently been published by José Romo and Manuel Doncel, would have been welcomed. (I understand that these may be published at a later date.) In temporal terms we start with Faraday's earliest writings – his travel diaries, commonplace book and lectures to the City Philosophical Society – and end with his lighthouse investigations on behalf of Trinity House that spanned the period from 1840 to the early 1860s. But for many users of this edition Faraday's researches on electricity, magnetism and chemistry will be its main attraction. They will find the complete text of his laboratory notebooks (previously published in seven volumes in Thomas Martin's transcription as *Faraday's Diary* (London, 1932–6)) and the annotated offprints of many of Faraday's papers, which have rarely been used by researchers. The amount of annotation varies considerably but in some cases there are substantial lists of references and notes by Faraday that deserve close study.

The other main type of source comprises lecture notes, including those taken by Faraday when he heard Humphry Davy and John Tatum lecturing between 1810 and 1812. But the bulk of this section consists of notes for a hundred and fifty lectures he delivered mostly at the Royal Institution between 1825 and 1862. For many lectures he devoted a double-page spread, with a list of experiments on the left-hand side and apparently disjointed text containing the main points on the right. As a loyal servant of the Royal Institution Faraday was prepared to lecture on almost any topic that would prove attractive to subscribers: from the Thames Tunnel to electro-statics and from 'Silified plants and fossils' to lightning rods. The inclusion of these lecture notes will be welcomed by historians interested not only in Faraday but also in the Royal Institution's role in the popularization of science.

My own extensive use of microfilm has led me to hold a highly ambivalent attitude towards that medium, and to recall my sheer exasperation with films that are either too faint or out of focus to read. Although I have sampled only a small proportion of the many thousands of images that comprise this edition of Faraday's manuscripts, it appears generally to be of high quality. However, occasional pages in the laboratory notebooks were too faint to read, as Faraday had presumably written in pencil. This problem was

easily resolved by consulting Thomas Martin's transcription.

Frank James has provided a helpful guide to this substantial addition to the available primary sources on Faraday.

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Rick Rylance, **Victorian Psychology and British Culture 1850–1880**. Oxford: Oxford University Press, 2000. Pp. x+355. ISBN 0-19-812283-7. £45.00 (hardback).

The title is clear, but quite what 'psychology' encompasses and quite which audience the book addresses remain troubling questions. This study appears torn between conflicting demands: to achieve a systematic description of a field of endeavour, called psychology, and to provide the historical context for reading a specific text, G. H. Lewes's *Problems of Life and Mind* (1874–9). In the concluding pages the discussion culminates with a specific critique of New Criticism, which has encouraged literary scholars to view Lewes (and, by implication, George Eliot too) separated from historical context. Like many before, Rick Rylance observes that Lewes somehow never seems to get his due as a psychological theorist. To rectify this, Rylance locates him in the context of the Victorian creation of a scientific psychology. Part I of the book purports to describe with some comprehensiveness four discourses of Victorian psychology, while Part II consists of long chapters on the psychology of Alexander Bain and Herbert Spencer, as well as of Lewes.

The question of the book's scope is pressing. Rylance is caught by his own assumption that there is a trans-historical field, 'psychology', which means that he deals with Victorian variety in terms of 'the porous boundaries of the discipline' (p. 3). He does not deal with the historiography concerned with the 'shaping' of the field or discipline formation. If the book intends to characterize Victorian psychology as a whole then it short-changes in major respects. In Part I, the chapter on the discourse of the soul (which is equated with faculty psychology) is extremely weak on religious belief – though there is more on religion in the chapter on philosophy. There is no reference to spiritualism or mesmerism in the (admittedly short) index, let alone discussion of this major dimension to the popular spread of psychological ideas. The chapter on the discourse on medicine more or less says that so much has been written on the subject that the author will restrict his discussion to two authors, J. G. Milligan and Sir Henry Holland, but we do not really learn why the chapter is on these authors and how their often commonplace comments fit into the huge contribution of medicine to popular psychological discourse. There are errors of detail and questionable generalizations. Moreover, the book, though citing earlier work, almost completely ignores scholarship in the history of psychology from the last twenty years, scholarship that has done an enormous amount to deepen historiography. The author cites Michel Foucault – but not at all to deal with the major challenge to the history of psychology flowing from his writing, elaborated in the work of Nikolas Rose, which suggests psychology originates in the practices of government and discipline in liberal society. The chapters on Bain and Spencer contain elaborate philosophical commentary – which suggests the author may have yet another audience in mind for his book. Yet much has already been published on these psychologists. Unlike other commentators, however, Rylance does not place the wider evolutionary debate centre stage nor comment on Darwin's contributions to psychology. Lastly, in spite of the stress on historical context, the book makes regular asides about the supposed relation of Victorian psychology to modern theories of mind and consciousness, supposing that the book's audience is interested in modern speculations.

This book does not explicitly address the reflexivity issue, which might suggest a view of 'psychology' as both the world of individual lived experience and the disciplines that try to bring that experience under the sway of knowledge. But it deals with reflexivity implicitly, relating the innovative psychology of the period with Eliot's fictional representation of mind and conduct, by stressing Lewes's desire to create a psychology true to life (in contrast to the metaphysical exercises of the idealists or the arid formulations of the associationists), and by many

comments on the ethical, religious and aesthetic sensibilities touched by new ideas.

I think the book is at its best in deepening knowledge of the periodical literature. Other historians have read the main texts, but this study goes much further in embedding them in the critical context in which they were read. Rylance has no time for idealists, but his reading of the periodicals brings out the often interesting reservations that Victorians had when faced by apparently reductive notions of the self fostered by the new psychology. The book can therefore be read alongside Jenny Bourne Taylor and Sally Shuttleworth's collection (with its wider-ranging view of 'psychology') *Embodied Selves: An Anthology of Psychological Texts 1830–1890* (Oxford, 1998).

Rylance is a professor of English literature, with an interest in the intellectual origins of modernist writing and in the philosophy of mind. It is easy to share his dissatisfaction with 'the two cultures' and the arts/science disciplinary divisions that blight our ability to do justice to a field like the history of psychology. His book, however, takes the viewpoint of modern science for granted and talks in terms of a 'new' (biological or physiological) psychology replacing an 'old' (metaphysical or idealist) psychology. In my reading, by contrast, his material is replete with evidence that he is dealing with a fundamentally contestable area. Be this as it may, I suspect his real topic is Lewes, and he may even wish to claim that Lewes points the way to a 'true' psychology; but if this is so, the book should have had something to say on Lewes (and Bain) and pragmatism.

Other readers may well pick out different strengths and weaknesses. If so, this is a sign of the extraordinary challenge of doing intellectual justice to an area as multifarious and reflexive as 'psychology'. This book is no guide to the historiography, but it does contain a wealth of 'thick description' (a goal the author much favours) about the debates on psychology in the periodicals and the links of psychology to general intellectual discourse. The book, certainly, is also necessary reading for any future work on Lewes.

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Felix Driver, **Geography Militant: Cultures of Exploration and Empire**. Oxford: Blackwell, 2001. Pp. viii + 258. ISBN 0-631-20112-2. £16.99, $29.95 (paperback).

In *Geography Militant*, Felix Driver examines the relations between geographical knowledge and cultures of exploration within a predominantly nineteenth-century British context. This work contributes to a re-examination of the relationships between science and empire undertaken by historians of science in response to the influence of postcolonial theory which, in turn, has often represented scientific activity as the mere tool of imperial power: 'Postcolonial criticism has frequently given way to an essentialized model of "colonial discourse" which obscures the heterogeneous, contingent and conflictual character of imperial projects' (p. 8). Driver pays close attention to the cultural contexts within which the formation of geographical knowledge and processes of exploration took place. He demonstrates that the purposes of exploration were highly variable with scientific, missionary, commercial, diplomatic, military, journalistic, literary and personal objects all competing for primacy. Practices of exploration were constrained and shaped by personal patronage relationships: 'Crucially, the explorer's search for a reputation depended on social relationships at "home" as well as in the "field", with patrons, publishers, editors and image-makers acting as vital mediators' (p. 9).

The author is convincing in his attempt to demonstrate the heterogeneous and highly contested nature of geographical science through discussion of the careers of David Livingstone, Winwood Reade and Henry Morton Stanley. Livingstone's career is shown to have been supported by a mix of missionary organizations, anti-slavery groups, scientific societies and governmental departments. His moral outlook satisfied religious and philanthropic sensibilities whilst simultaneously fulfilling the political object of representing imperial activity as a benevolent, civilizing enterprise. Scientific and political communities valued his contribution in bringing certainty, knowledge and authority to hitherto blank spaces on the map of Africa. Reciprocal benefits were accrued by Livingstone

and the Royal Geographical Society (RGS) from his research. The revival in popularity of the RGS from the 1850s owed much to Livingstone's adventures and their promotion by Roderick Murchison, who acted as chief patron and publicist.

By contrast, the career of Reade illustrates the role of contingency in the practice of exploration and the wide range of motivating forces which could underscore the enterprise. Reade is presented as an author and freethinker on the margins of scientific society with an ironic obsession with the martyrdom of explorers in Africa (he died aged 36). His exploration in Africa inspired the production of a scientific exposition of human descent that aimed to popularize the work of Charles Darwin. Reade's research was discredited in scientific society by his perceived lack of scientific credibility, although posthumous recognition followed.

Stanley's career served to highlight the contested and controversial nature of geographical science. Stanley's exploration by warfare offended philanthropic and missionary groups and placed the RGS in the position of wishing to be associated with his geographical achievements whilst remaining aloof from his methods.

Striking parallels could be drawn between the sciences of geography and statistics in this period. Both gained institutional representation in the 1830s and were highly contested enterprises marked by battles between armchair theorists and researchers in the field. Both sciences attempted to describe and quantify physical space and were applied to colonial contexts and deprived British urban communities. Driver's only reference to statistical science is made in relation to the work of Charles Booth in surveying urban poverty in London. Driver makes no reference to similar activities pursued fifty years earlier by members of the Statistical Society of London whose attempts to enquire into the 'Condition of England' parallelled government attempts to quantify society along scientific principles through the expansion of the national census and the foundation of the General Register Office.

In Chapter 9, Driver demonstrates how the cultural history of exploration remained a field of conflict and controversy, rather than a narrative of progress, in the twentieth century. Media interest in exploration in the twentieth century served to ensure that many of the debates over what constituted legitimate geographical enquiry endured. A crisis point was reached in 1933 when a significant proportion of academic geographers left the RGS to form the Institute of British Geographers (reunion occurred in 1993).

The author argues that historians of science have paid little attention to the RGS:

> This neglect may perhaps reflect continuing uncertainty over the 'scientific' status of geography in general and the heterogeneous character of geographical exploration in particular. Moreover, the patently hybrid character of the Society itself – part social club, part learned society, part imperial information exchange and part platform for the promotion of sensational feats of exploration – may have had the effect of diminishing the significance attached to it by those historians seeking 'purer' lines of descent for modern scientific endeavour (p. 25).

It would be disturbing if any of these explanations were accurate. Work that considers the contested status of individual sciences and the heterogeneous nature and functions of scientific societies now represents mainstream historiography in the field. Similarly, it is anachronistic to assert that historians of science are guided by a desire to trace pure lines to modern scientific endeavour. Driver's provocative explanations for neglect identify issues that are likely to encourage rather than obstruct research in the field. This, of course, may have been his intention.

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Peter Zigman (ed.), **Einblicke in eine sterbende Ära: Das Ende des Mythos der guten alten Zeiten**. Philosophica XXXII. Bratislava: Comenius-Universität, 2000. Pp. 191. ISBN 80-223-1425-0. No price given (paperback).

Scholars of the University of Bratislava and the University of Regensburg collaborated to produce this volume. It is a good example of the

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positive results which academic exchange can achieve. The editor presents the beginning of the German nineteenth century as determined by the deaths of Hegel (1831) and Goethe (1832). After these symbolic events a new intellectual era was initiated which culminated in 1933 when National Socialism began to dominate the political and cultural agenda. The polyphacetic period from 1830 to 1933 was characterized by a stage of transformations which strongly determined the twentieth century. It was the age of science, historical thought, idealism in crisis and European humanist culture. The eight articles collected by Peter Zigman investigate different aspects, phenomena, processes and figures involved in these intellectual trends, mainly in the German world.

The volume is structured in three parts, each related to different general topics. The first one consists of two articles focused on Ernst Haeckel, an intellectual symbol of the time. The article by Peter Zigman exposes the life, intellectual background, works and influence of the author known as the German Darwin. He emphasizes the role of Haeckel as a scientist situated between the generation of 'nature searchers', strongly impressed by the old natural science tradition, and the new generation influenced by the exact sciences. The article by Rainer Brömer provides a complete historical view of Haeckel's role in the monist movement of the time. He explains the meaning of Haeckel's monism by relating it to the historical, cultural and scientific background. In so doing he looks at scientific realism, August Schleicher's use in linguistics of early Darwinian theory and the different conceptions of materialism maintained by authors like Karl Vogt, Ludwig Büchner and Jacobus Moleschott. Specially interesting are Brömer's insights into the consequences of monism in the interpretation of religion.

The second part, composed of two articles, illustrates the 'spirit of the age' by exposing two special cases, pointing out at the same time the contrast between them and Haeckel's monism. The article by Lenka Bohunickás deals with the resonance of the monist tradition in Czech philosophy, in the particular case of the positivistic ethics of František Krejci. The copious work of Krejci represents the highest point of Czech positivism and influenced strongly the philosophical agenda from the end of the nineteenth century to the first half of twentieth century. On the other side, the article by Zlatica Plašienková examines the peculiar interpretation of the Darwinian theory in the world view of Pierre Teilhard de Chardin. As a point of departure Plašienková claims that, from the viewpoint of Darwinians, it was 'completely natural' to put evolution in the core of a religious or an ethical system, provided that the concept of evolution represented the ground of the world structure (p. 94). In this sense Teilhard's thought represents an alternative interpretation of evolutionary theory in the context of a theologically and teleologically minded world view.

Finally, the third part is devoted to the great intellectual transformations caused during the period. The first article, by Peter Zigman, is an attempt to expose some of the important changes undergone by the paradigms of science. He maintains that after the crisis of idealism and the rejection of the speculative natural science, the *Naturwissenschaften* as well as the *Geisteswissenschaften* tended to investigate only the *reality* (*Realität*) and to fight against metaphysical and (post-)idealist speculations. However, scientists gradually realized that this aim was impossible to achieve. Consequently they needed to appeal to abstractive procedures in order to make up a systematic world view, a sort of 'illustration' of reality. Thus the nineteenth century witnessed a change from a glorified reality to an illustrated abstraction (p. 139). In the same way, Sigmund Bonk's article deals with the changes in German philosophy before the First World War. Bonk maintains that, from Immanuel Kant to Friedrich Nietzsche at least, there was a process which culminated in a radical voluntarianism. At the same time a process of 'loss of reality' took place. Both phenomena contributed to preparing the ideology which was partly responsible for the catastrophe of 1914. The article by Christoph Meinel is concerned with the transformation undergone by the natural sciences and mainly by physics. Meinel points out how two completely opposite processes coexisted and interacted during the decadent *fin de siècle*. While inside physics the dream of the explanation of nature in

mechanical terms came to an end and the age of modern physics consequently began, the wider cultural background underwent a deep crisis. The last contribution by Peter Zigman deals with the transformations of the spiritual background witnessed by the ideology of the definite class of German professors, the *Idealtypus* of German *Mandarinentum*, which shows an oscillation between the idealism myth and the catastrophe of National Socialism.

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Sven Widmalm (ed.), **Vetenskapsbärarna – Naturvetenskapen i det svenska samhället, 1880–1950**. Hedemora: Gidlunds förlag, 1999. Pp. 368. ISBN 91-7844300-8. No price given.
Gustav Holmberg, **Reaching for the Stars: Studies in the History of Swedish Stellar and Nebular Astronomy, 1860–1940**. Lund Studies in the History of Science and Ideas, 13. Lund: Ugglan, 1999. Pp. 243. ISBN 91-628-3837-7. £10.00 (paperback).

There are many possible strategies to get at the big historical picture. It seems the most fashionable is the 'serial postcard' approach. A series of diverse stories reveal facets that add up to a kind of big picture. Simultaneously, the serial postcard approach can be contextual and locally sensitive – in contrast to the unfashionable big picture yielded by statistics. Have your cake and eat it?

Sven Widmalm's edited volume is a serial postcard history of science in Sweden from 1880 to the end of the Cold War (not to 1950 as the book's title would have it). There are chapters on the general expectations of the populace around 1900, mechanistic explanations in the life sciences, the relationship between earth sciences and the exploitation of the Kiruna mines (north of the polar circle), the formation of biochemistry as a discipline, four women pioneers in medicine, the expansion of physics education in primary and secondary schools, private versus state funding of plant-breeding research revealing the negotiation of objectivity, social engineering, and nuclear physics in relation to democracy and Cold War politics. Finally, there is a chapter arguing that the way to understand post-war Sweden is not in terms of the much-vaunted model of social democracy but rather in terms of the country's military requirements. My guess is that the editor did not start with a serial postcard plan and then commission these papers but, faced with the task of justifying the coexistence of ten disparate papers between the covers of one book, he deployed the serial postcard approach to turn weakness into strength. The book is the result of funding for a project on history of science in the period from 1850 to 2000 (see Tore Frängsmyr's foreword, p. 7) and it is noteworthy that almost all the contributors are post-docs without tenure. Perhaps the funding recipients are coextensive with the book's contributors, and the editor was faced with the challenge of fashioning coherence out of what was at hand in order to satisfy the requirements of a block grant.

If this is the case, then Widmalm's introduction is a well-crafted text. He strengthens his serial postcard claim admirably. The title of the book – 'the science carriers' – puns on the phrase cultural carriers, as in the German *Kulturträger*. This term refers to the supersession of learning (as in the German *Bildung*) by scientific expertise in all walks of life. Widmalm juxtaposes a twentieth-century statistic, mapping the dwarfing of arts by science faculties at Swedish universities, with a sarcastic poem on the aesthetic qualities of square clouds. In this sense the book is about the conquest of a society by new professions all sharing a scientific outlook (whatever that might be) – a conquest exhibited in many small battles against local resistances.

Many of the chapters describe such battles resulting in the penetration of science into the nooks and crannies of society. Thomas Kaiserfeld's chapter on the expansion of physics in primary and secondary education is perhaps the most important example. Kaiserfeld examines it not just in terms of an increase in the slice of the curriculum, but also in terms of teachers' training and the funding for larger facilities with laboratories supplied with water, gas and electricity. He further analyses the historical shift in the stated purpose of education: from discipline and general learning (as

in *Bildung*) towards critical observation and thus independent thinking and initiative. This chapter begins to fulfil a current need. Interest in science and the public sphere is on the rise, but basic knowledge – such as the degree of training of the general population in science – is missing. (It is absent also in my 'Science *as* public sphere: x-rays between spiritualism and physics', in *Wissenschaft und Öffentlichkeit in Berlin, 1870–1930* (ed. C. Goschler), Wiesbaden, 2000, 89–126.) Studies such as Kaiserfeld's are a *sine qua non* for the topic of science and the public sphere.

Widmalm's own paper juxtaposes a publicly funded plant-breeding institution with a private establishment in order to analyse the perceived objectivity and purity of work paid for by the state and its mirror image, the sullied interestedness of profit-related research. One aspect that Widmalm brings to the fore is the conflation of science and state. Political decisions were to be taken 'rationally' and on the basis of 'objective' research, and objectivity could only be assured within the funding provided by a state untarnished by partisan interests. The penetration of society by science became so thorough because science accompanied the actions of the state, for instance in the context of social engineering.

Ulf Larsson examines the work and thinking of one particular social engineer, Hjalmar Cederström. One important theme in Swedish public debate in the 1990s has been an evaluation and sometimes denunciation of the basic tenets of the *folkhem*, a term that refers to Swedish society as the home of the entire population. Several suspect policies, such as forced sterilization, have been highlighted. The conception of a Hegelian benevolent state forcing individuals, purportedly in the interest of the entire community, was the target. Larsson's starting point is of course the distanced late-1990s perspective, from where social engineering has become so alien as to require a contextualized explanation. In his introduction, Widmalm draws a parallel between such a typically Swedish critique and debates in other countries on colonialism and science.

Ulrika Nilsson's paper on four women breaking into the male-dominated world of medicine is another example that Widmalm has managed to present under the theme of professionalization: it mattered less whether you were male or female, and more whether you had undergone a particular form of training.

Karl Grandin examines the penetration of scientific expertise to the pinnacle of political power, namely the privileged access of physicists to the government with regard to nuclear policy. The physicist Torsten Gustafson had private access to the prime minister Tage Erlander, that is to say outside the normal machinery of democratic decision-making. Expert advice at the highest level without any democratic checks and balances made Gustafson feel 'a little queasy' (p. 345). Such undemocratic and unaccountable practice was institutionalized in neighbouring Denmark and elsewhere (Flemming Petersen, *Atomalder uden kernekraft: Forsøget på at indføre atomkraft i Danmark 1954–1985 set i et internationalt perspektiv* (Århus, 1996); and Arne Hessenbruch, 'Energy and Experts', *Annals of Science* (1996), **53**, 627–31, 630). The tension between democracy and expertise is thus not a local Swedish story. Bernadette Bensaude-Vincent has recently argued that the gulf between the scientific expert and the general populace widened considerably with the emergence of relativity and quantum physics, and if she is right, then the elitist and anti-democratic physics of the mid-twentieth century was institutionalized in the post-war expert advice on matters nuclear (Bernadette Bensaude-Vincent, *L'Opinion publique et la science: à chacun son ignorance* (Paris, 2000)).

That matters nuclear matter almost goes without saying, but Hans Weinberger goes further by arguing that an assignation of causal primacy to the military – nuclear or otherwise – yields a better big picture of Cold War Sweden. Most Swedish general historians have characterized the period as the reign of social democracy, but Weinberger argues that an emphasis on the military is more useful because defence decisions were taken first, setting the framework within which social policies were made. He consciously fashions his argument along the lines of David Edgerton's accounts of Cold War Britain in *England and the Aeroplane: An Essay on a Militant and Technological Nation* (London, 1991). As an aside, it ought to be

mentioned that this argument is convenient for a historian of technology since it places his or her skills above those of the general historian.

All these papers are thus brought under the heading of one theme. Widmalm, Larsson and Grandin's papers all bring out the connection between a positivistic science and the *folkhem*. In the middle of the twentieth century an expert was considered objective and benevolent. Kaiserfeld shows vividly the penetration of physics in primary and secondary education. Nilsson illuminates professionalization from a gender perspective. Grandin recounts the apotheosis of the scientific expert at top-level politics, and Weinberger argues for the centrality of the military–industrial complex to the writing of history. These papers contribute to Widmalm's stated theme of the science carriers' expansion within, and conquest of, Swedish society.

The other chapters are less easily subsumed under Widmalm's heading. Anders Lundgren contributes a lucid account of the institutionalization of biochemistry in Sweden, pointing in particular to the growth of recognizably 'biochemical' work *avant la lettre* within other institutions, and noting that the naming of a discipline only becomes important when submitting applications for funding. Anders Ekström provides a stream of consciousness concerning a *mentalité*-like expectation of science and technology around 1900. Torbjörn Gustafsson identifies an inter-war *mentalité* troubled by the place of life and mind within a science allowing only mechanical explanations.

The book is of course written in Swedish. The intended audience is defined by the boundaries of the nation state. The introduction and the last chapter both contain the word 'we' (pp. 9, 363). Swedish historians of science and technology have internalized the consequences of overwhelming Anglo-Saxon uninterest in a small peripheral country. After all, how many Brits or Americans know whether Swiss refers to Sweden or Switzerland? Widmalm's introduction accordingly addresses the *folkhem*-debate and Swedish science policy (he argues that close political control is illusory because the relationship between science and politics is in constant flux).

By contrast, Holmberg's English-language ex-Ph.D. thesis, *Reaching for the Stars*, addresses the astronomy aficionado. It chronicles astronomy in Sweden between 1860 and 1940. It is structured primarily around the professors of astronomy Nils Dunér, C. V. L. Charlier, Knut Lundmark and Bertil Lindblad. The main themes are the rise of spectroscopy and photography, stellar statistics and nebulae classification, and the displacement of astronomy by astrophysics and cosmology. There are many interesting details in this book. For example, Holmberg dismisses Peter Galison's claim that human judgement was reintroduced during the 1940s in the labour of classifying stellar spectra (Peter Galison, 'Judgment against objectivity', in C. Jones and P. Galison, *Picturing Science*, *Producing Art*, New York and London, 1998, 327–59). Holmberg counters that in fact the classifying labour did not involve human beings judging a spectral type by directly investigating the photographic plates. Rather, the labour involved several steps. The history of this labour, involving all the steps, is clearly towards a greater, not lesser, degree of mechanization.

But it must be said that the topic could have been better digested. The book reads as if Holmberg has been more concerned to use all his notes rather than create a compelling narrative. The last chapter even contains a hodgepodge of everything that did not fit into the themes of the chapters on professors. There are many repetitions – seemingly again because including all the notes took precedence over a good narrative. To use a phrase of Charles Rosenberg's, he used all of the pig but the squeal.

Frank criticism is the reviewer's prerogative, but the reader should keep in mind that this was a Ph.D. thesis, and that the comparison with a published academic book in Britain or the US is inappropriate. All Swedish Ph.D. theses are published. This may have been sensible so long as a Ph.D. thesis could mature in peace (Svante Lindqvist's wonderful 1984 thesis *Technology on Trial: The Introduction of Steam Power Technology into Sweden, 1715–1736*, is a case in point). But today Swedish Ph.D. students are subject to great pressure to deliver a thesis quickly and maybe the policy lesson to be learnt is to relinquish the requirement of publication.

Holmberg's understanding of both the science and the historiography seems shaky at times. The Stark effect is explained (p. 152) as a broadening of spectral lines produced under high pressure (rather than a strong electric field). Occasional descriptions of technicalities are sloppy and potentially misleading (e.g. 'density of photographic plates', referring to blackening of photographic film, p. 150). The discussion of general themes in social studies seems perfunctory. For example, in one part of the thesis (pp. 49–53), mechanical objectivity is discussed citing Galison and Daston approvingly (Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago, 1997); Peter Galison and Lorraine Daston, 'The Image of Objectivity', *Representations* (1992), **40**, 81–128), but when the issue of mechanical objectivity crops up elsewhere (pp. 139, 150), Holmberg plainly ignores the theory and even intimates that a machine simply is more objective than a human being. Holmberg is probably well advised to have written in English, because he does not relate his history of astronomy to general Swedish history, as Widmalm does. Holmberg's English is a kind of Scandinavian pidgin, exhibited for instance in a pervasive use of present continuous where native speakers prefer the present tense. This may not be a bad thing, often non-native speakers find polished, native English harder to understand than a pidgin.

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Nicole Hulin (ed.), **Physique et humanités scientifiques: autour de la réforme de l'enseignement de 1902. Etudes et documents**. Villeneuve d'Ascq: Presses universitaires du Septentrion, 2000. Pp. 339. ISBN 2-85939-624-1. FF 170.00, €24.92 (paperback).

It is not surprising that many volumes have been dedicated to the history of education in France. The number of reforms, the depth of public debate and the calibre of the intellectuals involved in the planning of national education well justify the interest that it has attracted. *Physique et humanités scientifiques* has a very precise focus: the role of physics in the 1902 reform, thanks to which the sciences came to play a more important role in boys' secondary education. The volume is divided into two parts of roughly the same length; the first part comprises six essays, the second part a choice of documents. The official aim of secondary education in France was – and still is – that of the shaping of the individual, of the transformation of the child into a worthy citizen and human being, rather than, say, technical or vocational training. It was also widely maintained that the means to achieve this goal was that of *culture générale*. *Culture générale* was intended to summarize a whole civilization, indeed human intellectual achievements synthesized into a higher unity. The problem was that of deciding which disciplines could be said to contribute to it. Literary and philosophical subjects needed no justification, but the sciences, and experimental sciences in particular, were only then acquiring a status higher than that of merely technical subjects. What is notable about the 1902 reform, and what justifies this volume, is that for the first time experimental sciences were admitted to the selected circle of the subjects contributing to *culture générale*. This new status of the experimental sciences was clearly expressed in the words of Louis Liard, Vice-director of the Academy of Paris, who, in a speech in 1904 addressed to teachers, maintained, 'In secondary education, scientific studies must, like the others, contribute to the formation of man. They too are in their own way, "humanities", in the broad sense of the term, [they are] "scientific humanities"' (p. 248 and quoted in the 'Avant-propos' on p. 11). Gabriel Lippman, professor at the University of Paris, also addressing teachers in 1904, remarked, 'the teaching of the sciences is, in fact, culture, the development of certain faculties, an *education* [in the French sense of the word] rather than instruction that improves memory' (p. 256). This theme of 'scientific humanities' is introduced in Nicole Hulin's 'La conception de l'enseignement de la physique dans la réforme de 1902', and further developed by Bénédicte Bilodeau and Nicole Hulin in the essay 'La physique au lycée au tournant du siècle: des analyses critiques à la réforme et son application' (pp. 45–73). Bilodeau and Hulin

argue that in fact the claim that the sciences form the individual and the citizen dates back to Georges Cuvier. The other strong theme of this book is the teaching of physics and in particular the balance between theoretical and practical instruction, or 'deductive' and 'inductive' methods. In 'De l'instauration des exercices pratiques à l'évaluation des savoir-faire expérimentaux', Danielle Fauque analyses the introduction of practical instruction in the school syllabuses, seen as 'the most innovative aspect of the 1902 reform' (p. 101). The authors of the 1902 reform had to justify not only that the sciences were integral parts of *culture générale*, but crucially that an experience-based approach in teaching them served the aim of *culture générale* in the best way. The connection between these two themes – *culture générale* and practical instruction – which I believe is crucial, is addressed in several places in the essays and in the documents. Christine Blondel, in 'L'impact de la réforme de 1902 sur l'enseignement théorique et expérimental de l'électricité', explores the connection between the introduction of practical instruction and general pedagogical aims in the specific setting of the teaching of electricity. She points out that those who advocated practical experiences employed both epistemological and pedagogical arguments (p. 94). These two types of argument are of course interdependent. In fact, Pierre Duhem's attack on the 'inductive method' promoted by the reform (Bilodeau and Hulin, p. 56) was based upon his epistemological convictions. Duhem's epistemology is summarized in Robert Locqueneux's 'Les théories physiques aux environs de 1900: bilans et perspectives' (pp. 135 ff., especially 139 ff.) within the context of the most important debates on the nature of physical theory at the beginning of the twentieth century. Michel Blay, in 'La méthode inductive: analyse critique des recommandations de 1904', offers a detailed analysis of the 'inductive method' as presented in Lucien Poincaré's speech appended to the volume and mentioned above.

The essays, though relatively compact, examine many aspects of the teaching of the sciences, sometimes in considerable detail. Nicole Hulin, in 'La conception de l'enseignement de la physique dans la réforme de 1902', provides many 'hard facts' of the reform, from the number of hours dedicated to the sciences to the members of the sub-commission dedicated to the reform of physics. The reader also finds information about the training of teachers (Bilodeau and Hulin); the reactions to the reform (Bilodeau and Hulin, Blondel, Fauque) and a detailed analysis of Lucien Poincaré's 'recommendations' to teachers (Blay). It is somewhat surprising that the state of the contemporary debate in physics is the only 'context' presented in the volume (Locqueneux). Many readers might be interested in the political significance of the contemporary debate about the reform and in finding out more about the social background of the pupils affected by the reform and the trajectory of their subsequent study or career. A little more care could also have been taken in the production of the volume. I was puzzled by the use of the author–date system of citation in the 'Avant-propos', in the absence of a list of references (the works cited in the Avant-propos are not to be found in the brief list titled 'Éléments bibliographiques' at the end of the book). However, *Physique et humanités scientifiques* is a very valuable tool indeed, and not only for the readership to whom the book is primarily addressed, that is historians of science ('Avertissement'), but also for historians of philosophy. Because of the breadth of the debate on education in France and the range of intellectuals it involved, I have found in my own work on Gaston Bachelard that the study of the pedagogy and school syllabuses helped me find an interpretative key to his philosophy of science. This volume is interesting reading for anyone working on French education and history and philosophy of science in the first decades of the twentieth century.

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James Gillies and Robert Cailliau, **How the Web was Born**. Oxford: Oxford University Press, 2000. Pp. xii + 372. ISBN 0-19-286207-3. £8.99, $15.95 (paperback).

It 'is a major new medium … one comparable with print, radio, and television, and which

could have as significant effects on society and our lives as those did and still do. Like them it may well lead to major changes in social habits and styles of life, and have long-lasting as well as complex economic effect' (p. 106). 'It', in this case, was a convergence of computing and communications, a means to provide searchable hypertextual data over a network, displayable on a million screens. The 'it', so breathlessly described here, was not the World Wide Web in 1993, but Viewdata in 1979, and it was a resounding flop.

One of the strengths of this solid, if rather sparkless, history of the Web is the authors' demonstration of how many times something similar had been launched into an unwelcoming world. Hypertext enthusiasts have long held up Vannevar Bush, Doug Engelbart and Ted Nelson as their historical heroes. Gillies and Cailliau paint a much more detailed picture, with vignettes of Viewdata, Austrian Videotex and the University of Southampton's archival software innovations, amongst many others, contributing to a portrait in which the Web remains the main subject. The pattern is a familiar one to historians of technology: out of a plethora of potential candidates, one technology is picked and a name – the 'inventor' – attached. This process of simplification and attribution is, of course, one of the political uses of history. A few years ago if I had been asked 'who invented the World Wide Web?' my probable response would have been either to say 'don't know', or perhaps to suggest that it was an anarchic, headless, leaderless technology, one in which the usual attributions of personal creation did not apply. However, slowly, but steadily, the name of Tim Berners-Lee has been attached, with credit accruing to his employer, CERN. As historians such as John Krige and Dominique Pestre have shown, the European nuclear physics laboratory provides a fascinating prism through which European politics has refracted. Given that this book, which aims to be the definitive account of the Web, is co-authored by a CERN employee and praises another, it is not surprising to see some complex national and international politics at play.

The Web depends on many technological ingredients: the Internet, some hypertext rules, browser software and networked personal computers, each equipped with a mouse to point and click. Gillies and Cailliau trace the histories of each of these, going beyond the standard stories. Thus Donald Davies at the National Physical Laboratory gets equal billing with J. C. R. Licklider, Bob Taylor and Larry Roberts at ARPA for package-switching data networks in the 1960s. The French network Cyclades is also brought into the story, and its incompatibility with Giscardism (it was a project with a much more Gaullist feel) convincingly highlighted in explanation of its demise. (Cyclades also reminds us that there was more to French networks than the familiar Minitel.) While the spread of the ARPANET in the United States is fairly well known, how nodes were added in Europe is not. The first two sites were a nuclear test monitoring station near Oslo, and University College London. The choice of UCL rather than the more obvious NPL was the result of a calculation of the balance of European and American relations:

> a highly Europhilic Heath administration was preparing to lead the country into the Common Market. Any hint of a special relationship between the UK and the USA was to be avoided, and a UK national laboratory putting transatlantic links first was simply out of the question (p. 52).

(The authors provide no evidence for this plausible claim – although each chapter has a bibliography, there are no footnotes and no direct referencing of quotations.)

However, the protocols – TCP/IP – behind the ARPANET were not the only candidates for internetworking computers. Indeed, they were at one stage viewed merely as temporary stepping stones before the imposition of governmental or proprietary standards. The outcome of the three-way 'protocol wars' (the 'rough consensus and running code' of TCP/IP, the bureaucratic but open X.25 standard of the ISO, and the proprietary proposals of private business) proved crucial to establishing the culture of the 1990s Internet (p. 64). The stampede of users won the battle for TCP/IP. A telling anecdote concerns Peter Kirstein's UCL Internet (i.e. TCP/IP) node: in 1985 he messaged everyone who had been using the network in the previous six months.

'"When I recovered from the e-mail responses", Kirstein recalls, "I found I'd been sending this to some two or three thousand people when I thought there were only forty to a hundred users"' (p. 85). The surprising growth of the Internet had begun, the consequence of thousands of individual choices rather than a plan directed from a centre (or an inventor).

The authors of *How the Web was Born* are often content to quote key players, and merely sit back if they disagree. Historical questions are left unanswered. For example, why did TCP/IP win out against ISO standards? Gillies and Cailliau report Kirstein's view that the main factor was 'the whole policy in the US of encouraging software development in academia and making the software freely available to industry', in addition to military support through ARPA (p. 88). UCLA ARPANET pioneer Leonard Kleinrock credits the vision of the ARPA bureaucrats: 'We never had any hassle ... no senators, no congressmen, no military, no nothing in our way' (p. 89). Another ARPANET hand, Bob Cooper, blamed ISO's 'cumbersome' approach to standards, while NPL computer scientist Derek Barber points the finger at the profit-seeking computer manufacturers (p. 89). Where does the best explanation lie? In one or more of these factors? The authors do not tell us.

The giant accelerators at CERN generated huge amounts of data. Computers were needed to handle this information, as well as control the actions of the myriad physics instruments that surrounded the particle beams. The wide distribution of computers across the CERN site made the laboratory a fertile place for networking techniques to develop. In the 1970s these techniques were only needed in-house, but in the 1980s the question of how to connect to the outside world arose. CERN, of course, was paid for by European governments and such a high profile state-funded organization could not easily break away from ISO to TCP/IP. It did so surreptitiously, publicly backing ISO while pragmatically connecting to the Internet when necessary. In 1989 the switch-over was made official, and by the following year 'CERN became the biggest Internet site in Europe in terms of traffic [largely because IBM funded the main US–Europe Internet link there], and the laboratory's expertise was widely called upon by other organizations wishing to jump on the Internet bandwagon, including, perhaps as a final act of capitulation, the ISO itself' (pp. 87–8). By the turn of the decade, then, CERN was a good place from which to launch a new networked hypertext project.

Tim Berners-Lee was unusual even before he moved to CERN: his parents, Mary Lee and Conway Berners-Lee had worked at Ferranti in the 1950s, which makes him a second-generation computer scientist. Tim must have seen the Ferranti mainframes, since he built cardboard computer models as a child, as he recalled: 'the main features were that you could push paper tape in one side and pull paper tape out of the other side, and there was a clock in the middle. And that's a pretty good model for a computer' (p. 151). He was educated at Emanuel School and Queen's College, Oxford, where he repeated his trick, this time building a working computer out of an old television and a keyboard from a discarded adding machine. *How the Web was Born* is not good on linking Berners-Lee's life to his work: a sensitive biographer would rejoice on discovering Tim's strong (Unitarian Universalist) religious convictions and there is no mention of a private life. In 1980, aged 25, Berners-Lee was working for a small start-up outside Southampton, when he applied for, and got, a temporary job at CERN. He was confronted with a massive laboratory, with a moving army of physicists, all dependent on paperwork. In the 1980s the documents were archived in a traditional hierarchical way through the system 'CERNDOC'. But Berners-Lee had tested on himself a different archival structure: with his program Enquire, a name he took from the Victorian compendium *Enquire Within Upon Everything*, he linked documents in an arbitrary way.

The Web – initially just Berners-Lee's solution to CERN's document-handling difficulties – was Enquire writ large: an arbitrary structure of hypertext pages transportable over the Internet. In March 1989 he outlined it in a memorandum under the title 'Information management: a proposal'. In it, he wrote precociously that the 'problems of information loss may be par-

ticularly acute at CERN … but in this case CERN is a model in miniature for the rest of the world … CERN meets now the problems the rest of the world will have to face soon' (p. 182). Was Berners-Lee profoundly insightful or lucky in this claim? For in its accuracy lies an explanation for the spread of the Web. The proposal first received a puzzled reception. 'Vague, but exciting' was his boss's response (p. 181). But the wider CERN management was just then considering the likely problems to be encountered in the next expansion of the laboratory, the construction of the Large Hadron Collider, and information management was near the top of the list. Good timing therefore secured Berners-Lee's proposal limited support. Various names were kicked around – 'Mine of Information' and 'Information Mesh' – before Berners-Lee and colleague Robert Cailliau settled on 'World Wide Web' (p. 199).

Ironically, the lukewarm support from the CERN management contributed to the Web's success, as Berners-Lee was forced to turn to geek-power: unable to develop the software in-house he released toolkits and let enthusiasts do much of the work. The CERN server (http://info.cern.ch) went public in 1990, and the subsequent story is one of appropriation and diffusion, as users discovered the Web's potential and wrote software to view its contents. It was this process that decisively shaped the Web – indeed its 'invention' should be located as much with the developers as with Berners-Lee and CERN. The first browser was written by Nicola Pellow, a maths student doing a sandwich course at Leicester Polytechnic. The freely circulated Viola, Lynx and Mosaic followed. Within a decade, firms based on selling browser software, not so different from Pellow's, would be worth billions of dollars.

While European politics were imprinted on the early Web through its development at CERN, tensions between the United States and Europe shaped its later life. In 1993 the Web grew by 350 per cent, mostly with new sites across the Atlantic. Europe was losing out. In response, Cailliau wanted the European Commission to create an 'Alexandria' project: a multi-million-ECU centre of excellence based at a French technology park. Jacques Delors did not call back. In fact Berners-Lee was already talking to MIT about a US–Europe deal, and it was from his initiative that a global World Wide Web consortium (W3C) was to emerge. The negotiations, described in detail, were protracted, but we should not underestimate their importance: what was being settled was the global management structure of a new media technology. One outcome, however, was that CERN backed out: 'it was', the authors explain, 'becoming obvious how big the Web was going to be even in Europe' (p. 286) and the laboratory was being distracted from its particle physics. Replacing CERN as a partner with MIT was the Institut National pour la Recherche en Informatique et en Automatique (INRIA). In this way, France, famous for resistance to the American Internet, came to co-host the Web. There remained a small issue of credit. 'The ideal solution', note the authors, 'might … be for CERN to let the Web go, as long as they could somehow let the world know that it had been invented at CERN. The Web was set to be the most valuable spin-off from fundamental science ever, and the political value of that was what most mattered most to CERN's management' (p. 286). From the horse's mouth: the inventor of the World Wide Web had to be invented.

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