REVIEW SYMPOSIUM

A PARTING SHOT AT MISUNDERSTANDING: FULLER VS. KUHN

Steve Fuller, *Kuhn vs. Popper: The Struggle for the Soul of Science*. Cambridge: Icon Books; Crows Nest, NSW: Allen & Unwin, 2003. Pp. 227. £9.99, A$29.95 HB.

By David Mercer

In his last book, *T. S. Kuhn* (2000), Fuller challenged the received view of the academic life and work of T.S. Kuhn. He continues in similar vein in *Kuhn vs. Popper* but adds a further dimension to his story by measuring his deconstructed Kuhn against a reconstructed Popper. Fuller takes aim at our regular misunderstandings: "So Popper was a democrat concerned with science as a form of dynamic inquiry and Kuhn an elitist focused on science as stabilising social practice. Nevertheless they appear with these qualities in reverse. How can this be?"

Fuller answers his own question by emphasising that both thinkers are normally read out of context. For Fuller, reading context involves much more than just considering the setting in which ideas are composed and their reception: we also need to make judgments, according to our current standards, of how players should have behaved and reflect on how our interpretation of the past effects our present commitments. Weaving around anticipated criticism, that he is doing whig history and engaging in the genetic fallacy, Fuller scrutinises Kuhn and Popper in terms of how they responded to their critics, what views they held about science, their intellectual and political aims, what traditions and current trends their work can be identified with, and most important in the case of Kuhn, what he didn't but should have said. In particular, Fuller emphasises that it is only through understanding the specific conditions of the emergence of the Popper/Kuhn debate, explaining the reasons for Kuhn's victory and by making moral judgements about Kuhn's motivations,

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that we can come to properly understand its continuing impact. By considering this expanded (meta-) context Fuller hopes to convince the reader that Kuhn should be held responsible for a harmful dominant ideology in science studies: the failure to adequately engage in making political prescriptions about the most desirable normative and epistemological structure for science. Kuhn is personally damned because he failed to engage in political critique of Cold War science and his sins have tainted the aspirations of future generations of his unwitting followers. Popper in contrast, despite his misleading politically conservative image, did engage in 'real world' Cold War critique and his work represents the 'path not taken' of a politically enlightened focus on the epistemological and normative dimensions of science. Fuller at the end of his text notes that perhaps we might allow Kuhn some dispensation for his "lack of courage, clarity of mind, or sense of the times" (pp. 214–215) if we consider that the integrity of his academic project was more likely to be significant than any kind of critical intervention. But if we grant this, we must acknowledge that we have been beneficiaries of Kuhn's choice to bow to Cold War political pressures and that by excusing Kuhn we excuse ourselves. To move forward, science studies must exorcise its Kuhnian demons and reconnect with the normative trajectory of the Popperian project.

Fuller presents his case by reference to a variety of themes, the religious unconscious of the Popper/Kuhn debate, the need to dispense with concerns about whig history and the genetic fallacy, the importance of taking notions of negative responsibility seriously, the parallels between Heidegger and the Nazis and Kuhn and the Cold War, and the regular failure of philosophers (Rorty is singled out) to grapple with the sorts of issues raised by the debate. Along the way, he also provides a number of interesting anecdotes by reference to various Kuhn and Popper archives.

Does Fuller make his case? And what sort of case is it? I suspect most readers will have few problems with the fairly banal observation that Popper intended to provide a normative framework for science and that Kuhn didn't. But Fuller's interpretations of the significance of 'Kuhn's silence' and the claim that there has been such a clear victory of Kuhn (and some kind of Kuhnian project) over Popper are more likely to be questioned.

Kuhn's work has certainly exerted a significant influence on ideas about science, particularly in the more sociological precincts of science studies and the humanities more generally. In some cases this influence would appear to have encouraged the lack of normative concern that troubles Fuller; in others this has certainly not been the case. Anyone familiar with environmentalist and feminist critiques of science from the 1970s could find numerous examples of Kuhn's paradigms and scientific revolutions being used as rhetorical tools to challenge technocratic framing of debates involving science and technology. In particular Kuhn's work was valued by activists because notions of paradigm incommensurability made the task of arguing for 'the new' much easier. Activists could set their own standards for what counted as valid science and largely evade the standards of evaluation specified by those perceived to have entrenched interests.

How should we treat this effective regular radical (mis)understanding of Kuhn? Kuhn regularly expressed his displeasure at it (Horgan, 1996). But if we turn Fuller's reading of Kuhn's negative responsibility to the Right shouldn't Kuhn be held accountable for not discounting these radical readings more effectively? Did he try hard enough? If we are to credit Kuhn's lack of political engagement as encouraging one strand of influence in science studies, which eschews normative commitment, why can't we also credit the same relative passivity with influencing others that encouraged opposite readings?

Popper has also been read in numerous ways. A couple of years ago in an Australian theatre of the 'Science Wars', Keith Windschuttle devoted a chapter in his anti post-modern diatribe *The Killing of History* (1994) to an alleged decline in academic standards in the history of science. Following David Stove (1982), he identified one of the sources for what he believed was a 'relativist rot', in the irrationalism of Kuhn and Popper. They were both part of the same 'anti-science' trajectory: Kuhn and Popper had both won. To illustrate a contrary trend: at more or less the same time Popper was being labelled as an enemy of mainstream science and rationality by Windschuttle, he was also being used in influential US legal settings to help courts define them. First, Michael Ruse, appearing as an expert witness in *McLean vs. Arkansas* (1982), successfully promoted Popper's falsifiability as a tool for dismissing Creationist claims, and Creationism was deemed as untestable (Ruse, 1996). Popper's apotheosis in such settings was made complete when in 1993 he was cited by the US Supreme Court in *Daubert*, a 'toxic tort' case (Edmond and Mercer, 2002). With a symbolic Popper approvingly looking over their shoulders US Federal courts would from now on be reluctant to admit into court, as expert evidence, knowledge claims that could not be, or had not been, subject to 'testing' (although it should be noted that this standard has not always been consistently applied to restrict the admissibility of State forensic evidence, e.g. fingerprinting; see Cole, 2001).

The use of Popper by the US Supreme Court is important for the sorts of claims Fuller makes. *Daubert* is widely accepted as one of the most significant legal proceedings involving science over the last eighty years, not just for the US. It has spawned an immense literature, most of it celebrating the use of Popper as a way of rejecting 'junk science' and reinforcing textbook or 'mainstream science'. The impact of *Daubert* has not just been on fringe science. It has also inhibited the legal admissibility of various forms of medical and other expertise in ways generally favourable to large industrial and corporate interests. In general, plaintiff experts tend to rely on scientific claims that are more likely to be novel and less likely to have been subject to extended and often expensive testing (Edmond and Mercer, 2004). These difficulties tend to have been compounded by the way Popper is misunderstood as being compatible with a further check-list for admissibility which also includes non-Popperian standards such as whether a scientific principle is generally accepted and has passed peer review (Haack, 2001). In practice Popper's legacy in these settings has been to support the very scientific conformity and powerful political interests that Fuller identifies as part of the Kuhnian trajectory. These legal citations of Popper began to appear in the decade prior to his death; should Popper have objected to the way 'his project' was being put to use? Further, if writing history is subject to revision according to the impacts historical ideas have on the present (as Fuller suggests), does this mean Popper can now be classed as the winner? And in turn, does this mean he should be held morally accountable for failing to anticipate that his work, particularly his more strident populist claims about answering the demarcation problem (Magee, 1974), would be 'misunderstood' as an injunction against scientific novelty?

The examples above (albeit impressionistic) indicate how making judgments about negative responsibility and identifying intellectual trajectories are fraught with difficulties: especially when the environment that ideas are received and interpreted in is complex and the ideas themselves are subject to multiple readings (Gutting, 1980; Mulkay and Gilbert, 1981). These problems should not be taken as an excuse to dismiss the value of *Kuhn vs. Popper*. Fuller provides a rich and complex interpretation of the origins of the debate (even if some of the details may be open to different interpretation (see Ravetz elsewhere in the symposium). *Kuhn vs. Popper* offers fascinating but frustrating reading.

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Since Steve has pitched me into the history of the great ideological battle of the 1960s, I feel entitled to give a personal reaction to his book. First, I should say that this time he's done a great job. He's never mediocre, and this is one of the winners. I come in very near the end, as the subject of an incident, apparently the only one on record, where Thomas Kuhn was anything other than peculiarly neutral about the societal aspects of science. What happened was a letter of reference he wrote to the University of Pennsylvania in 1978, advising them against hiring me for a visiting Chair. As it turned out, the reference was irrelevant, for I had already decided for personal reasons not to continue with the negotiation. Also, I don't recall asking him for a reference (the whole process was very informal) so that the negative reference could not be construed as an act of treachery. And from his point of view I had indeed left scholarship for politics. He had already told me that he disliked the radical concluding section of *Scientific Knowledge*, and he knew that in the early 1970s I had taken up a non-academic job in the 'science and society' field. The point of the bad-reference story was to strengthen Fuller's analogy between Kuhn and Heidegger as political beings. The negative reference for Jerry Ravetz was the Cold-War analogue of Heidegger's flirtation with Nazism, played out in Kuhn's later years.

Be all of that as it may, I turn out to be the crucial evidence in Steve's thesis that Kuhn was a child of the Cold War and all its cultural deformities. Having established my credentials as living history, I'd like to contribute a personal perspective on his thesis. There are two aspects, the political and the personal. On the political side, I can claim to have been there, holding a junior post in the Department of Mathematics at the University of Pennsylvania in the academic year 1952–1953. That was the year that the

Rosenbergs were executed. It was perhaps the worst of the years of the reign of terror by the McCarthyite fascist gangsters, well remembered in the world of arts and entertainment, but not so well remembered in academe. The Hollywood Ten at least had the satisfaction of standing up and fighting publicly; their counterparts among the professorate were destroyed individually and usually more privately. And it was not just Joe McCarthy and his pervert sidekicks who did the dirty work; there were the various State Un-American Activities Committees, and trawls by investigators at all levels up to the FBI. These people used the general hysteria about the Commie-devils in order to victimise and destroy people, just to show that they were doing it and thereby claim bigger and better jobs and budgets. (It was a cause of some grim satisfaction to the Left that all the Chairmen of the House of Representatives Un-American Activities Committee of that period wound up in disgrace for corrupt practices.)

During this witch-hunt, anyone who had done anything anti-Fascist over the previous fifteen years was at risk of being discovered, exposed and then beaten to a pulp in all but the physical dimension. And the rest of America was going about its business, devoted to the frantic pursuit of happiness in the new suburbia. For anyone who was not already committed to political activism, the message was: Don't. Indeed, don't even think about it. And since no one except the suspect minority were talking about it (until it was nearly all over), it was easy not even to know that one was in denial. For someone like Kuhn who was brilliant but not overly subtle, the conditioned reflex of Staying Out of Trouble could have been firmly established during those formative years.

Of course the social sciences were the prime target for the witch-hunters of academe. But there is a precious anecdote from that period, which shows how even the history of science could experience the chilling touch. In his presidential address to the History of Science Society, Henry Guerlac recalled an previous incident involving the great guru Alexandre Koyré. Guerlac had written a paper describing the early work of Lavoisier in practical improvements, including street lighting. It came back from Koyré with a comment that it was interesting, *'mais un peu Marxiste'* (Guerlac, 1963). Now, it is always possible that Koyré, as a sophisticated Francophone intellectual, could use that term in a purely analytical way. But he would have needed to be very dense indeed to be unaware that in the America of the 50s '*Marxiste*' was a death-warrant for a scholar. Thus the history of science had its own ideological conformity, enforced explicitly if necessary, but of course more effective if internalised and suppressed. The social history of science did not exist, and the title *Science and Society* was too hot for anyone in the States to handle except for an avowedly Marxist intellectual journal.

This context of State terror and thought control could explain Kuhn's appearing almost as a political eunuch, always blankly refusing all opportunities to engage with the political or even ethical aspects of science when all around him found it obligatory to do so. It could be that the negative reference about me, in spite of our long-standing genuine friendship and mutual affection and regard, was a belated expression of some emotional negativity at a deeper level that was still not entirely resolved. Steve's analogy between early Nazi Germany and McCarthyite America is not at all far-fetched.

This political background can help shed light on the problem of Kuhn and the ideology of science. When I discussed the ideological conflict in the philosophy of science of the 1960s (Ravetz, 1990) I confessed to something of a difficulty with Kuhn. With the others there is no problem: Popper was the boy on the burning deck of scientific integrity, Lakatos was trying to rescue rationality from the Stalinist rape of reason (my thanks here to John Kadvany) and Feyerabend was playing court jester, a justified Situationist response to the collapse of a great ideology. But Kuhn patently had no ideology; and it offended my dramatic sense that someone who just sort of found out these things about science could have been so important in a momentous ideological struggle. Given that (in spite of his many lacunae as a philosopher) he was brilliant and profound, can we guess what was biting him? Do we need to do some sort of depth analysis of the text? Let's try.

The text of the *Structure of Scientific Revolutions* actually gives the game away, as we might have expected. How does it start? If we judge the quality of a philosopher by the opening sentence of his book, Kuhn is really high on the list. "History, if viewed as a repository for more than anecdote or chronology, could produce a decisive transformation in the image of science by which we are now possessed" (Kuhn, 1962: 1). This is as well packed with irony as anything from Descartes! Clearly, history (of science) is, for Kuhn, still a mere repository for anecdote and chronology; and it contributes to our being 'possessed' (*sic*) by a (patently false) image. In case the reader didn't get the point, Kuhn soon likens the history purveyed in the science textbooks to 'a tourist brochure or a language text':

caricatures. Then he demolishes the received image of Scientific Method as savagely as Descartes demolished humanistic education in Part 3 of the *Discourse on Method.*

So Kuhn is an angry young man of the 1950s, rubbishing some inherited pieties about the way things are, and standing ready to offer something in their place. What is that? As the replacement for the accumulation of truth and the exposure of non-scientific error, we find: dogmatism and arbitrariness. It is well known that Kuhn's puzzle-solvers are totally myopic, and there is no scientific test to decide between competing paradigms in a revolution. But it goes deeper; scientific education is 'rigorous and rigid'; and so normal science is a 'strenuous and devoted attempt to force nature into the conceptual boxes provided by professional education'. Anyone notice the irony? Reading this again, I wonder whether Kuhn had heard of John Hersey's famous novel *The Child Buyer* of 1947, about a man who went around collecting gifted children to work on mathematical problems for National Defense. His practice continued even after it was discovered that in the interest of efficiency they were maimed, blinded, and kept manacled to their places of work.

I now see Kuhn as the victim of several sets of unresolved contradictions, which must have worked at all levels from the intellectual to the existential. One is about 'normal science' itself, with its faintly pejorative description as 'puzzle-solving'. Someone once told me that if you want to understand his philosophy, you should see his PhD thesis. I was told that the research was done under P.W. Bridgman. He was known to the outside world as a really distinguished amateur philosopher of science, and he is still worth reading today. But (as I was told by one of his earlier students) his research on low-temperature physics was one huge mass of inspired gadgeteering, solving one puzzle after another in glorified plumbing. When you've spent a couple of holidays trying to find a leak in a vacuum line, you'll appreciate the pathos in the description 'what all scientists do most of the time'. The sting comes, of course, in the next bit: 'what most scientists do all the time', and are happy and fulfilled at it.

So first there is the contradiction that this wonderful thing called 'research' is a banality and a bore. And no one seems to admit it! Is this another of those conspiracies of silence of the grownups? It's one of those dirty truths that, once discovered, are unsettling to anyone, and particularly to someone as uncertain and confused as Thomas Kuhn. Then Kuhn reads the work of the scholars, hoping thereby to understand this peculiar practice. There he finds comfort, but (being too intelligent) he eventually spots a contradiction. If science is always true, and proceeds not merely by accumulation but also by refutation, what about the theories that we know now to be wrong? The histories of his day were full of character assassinations of the failed side in the revolutions. It was not merely the folk histories of the Aristotelians who refused to look through Galileo's telescope, or the anti-Darwinians who made coarse jokes about being descended from apes on the father's or mother's side. There were real, otherwise successful, scientists who got it wrong; some idiots supported phlogiston even after it was shown to have negative weight, and others supported caloric after Rumford showed how it could be produced *ad infinitum*. These stories were troubling to Kuhn, for with his intelligence and his experience of science he could see that the erroneous scientists were not simply stupid. Finally, in his epiphany on a hot summer day he rescued Aristotle himself from the charge of incompetence in mechanics, by relativising his argument to his culture and its special presuppositions.

So Kuhn shattered Scientism of the old sort. But all he could find to replace it was 'paradigm', a wonderful name with a plausible and powerful concept inside. I've described Kuhn's achievement as 'kicking open Pandora's box'; and Steve shows well how Kuhn suffered, even more acutely than most authors, the awful fate of seeing his progeny take on a monstrous, threatening life of its own. Whatever hesitations and confusions he had about his theory as he was shaping it, were swamped by this quite terrifying development of people seeing things there that were unintended or downright mischievous. Darwin went through six editions of the *Origin*, giving more and more ground to his critics, until he finally shut up shop and turned to the earthworms. Kuhn continued to agonise about details (such as 'incommensurability'), but perhaps wisely let the *Structure*-in-the-world turn into a pure cash-cow.

There is a broader context than Cold-War America. Kuhn was the first deep thinker to come to maturity as a scientist in an age when science was not embattled, when there was no longer anything genuinely heroic about it. The classic Warfare of Science with Theology was over; science had won, and there was no longer an ideology to fight for. The secularisation of mainstream higher education was complete. Organised religion was in a sort of ghetto, quite comfortable for the Episcopalians, less so for the more populist tendencies. The subsequent resurgence of the religious Right couldn't even be imagined. Also, in its social aspects science was then going through its massive transformation consequent on the war effort, with the Manhattan Project, and the post-war military-industrial boom. Science was becoming (in the phrase of the Marxist J.D. Bernal) the second derivative of production. A life in science was well underway in the transition from vocation to career; its final state as a mere job would come only later.

America was then in the state of what Daniel Bell later described as 'the end of ideology'. Arriving as refugees in an alien land, the Logical Positivists left behind the militant secularist ideology that had motivated them back in Mitteleurop. All the Americans saw was a dry, exclusivist doctrine about physical science and its inferior imitations. This barrenness of the ideological landscape explains the peculiar flatness of Kuhn's vision, in contrast to that of the Europeans for whom the symbolism of science was still vital, even in its corruptions. So Kuhn's ideological struggles, actually reflecting the fate of the Enlightenment project, were refracted through his purely personal disillusionments. But they were none the less painful and fruitful for that.

Back to Steve. All this is a footnote to his book, which I really found to be an illuminating tour, covering a broad compass in astonishingly little space.

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The question of the political character of science, and of what side one ought to take about science, celebratory or critical, is a complex and confusing domain which Fuller tries to simplify and package in a novel way in this interesting, footnote-free reflection by identifying, in the history of twentieth century discussions of science, a 'Left rationalism' that can say something critical and novel about science. In his book on *Thomas Kuhn* (2000), Fuller contrasts Kuhn's politics of science to Ernst Mach's. Mach was the good guy, who was for democratising science. Kuhn was the bad guy, a proponent of a 'Cold War' authoritarian conception of science that treated science education as a form of indoctrination, denied external influences on science, characterised science in a way that made democratic scrutiny impossible, and elevated the scientific community to a kind of sacred cow that could only be accommodated and not held to external standards.

This book develops an alternative view of science by bringing in the Popper–Kuhn debate of the 1960s, which was concerned with criticism and the growth of knowledge, as the title of Lakatos and Musgrave's famous book put it (1970). Karl Popper defended a model of science as a debating society, in which conjectures were refuted and the process was iterated. For Kuhn, science was a closed sect that worked on puzzles within an unchallenged framework and ignored anomalies until it no longer could. This 'conservatism' of science was, for Kuhn, a good and necessary thing. But Fuller abhors this conception of science. The problem this book takes on is to get a new account by marrying the Popperian view of science to the idea of democratic science and the political Left to the rule of law and to the ideal of open discussion.

There are some large obstacles to this marriage, as anyone familiar with the past history of the betrothed would note, which can be put in the form of some questions. Wasn't Popper a kind of liberal, and isn't Popper's idea of the Open Society a liberal idea? Isn't government by discussion, along with such notions as the rule of law, quintessentially liberal? Didn't the Left historically dismiss these notions as props of bourgeois power, from Dewey, who railed against constitutionalism in the United States, to European thinkers, including the Frankfurt School's legal theorists, who always spoke of the rule of law as 'bourgeois', saw it primarily as a means of defending property, and in the Weimar era sought to eliminate judicial review as a threat to parliamentary sovereignty (cf. Kirchheimer and Neumann, 1987)? Didn't the Workers in the Weimar Republic march to the chant 'A Republic is not so Grand, for Socialism we take our Stand', suggesting that their support of the liberal democracy that the SPD itself subscribed to was at best instrumental? Didn't the behaviour of the SPD in Weimar represent a pattern of running away from political responsibility and a preference for critique and moralising that was inappropriate for a party that was supposed to be a participant in a liberal democracy? Didn't the aristocratic Red Left of Science in England – J.D. Bernal, Patrick M.S. Blackett, Lancelot Hogben, and their allies disdain existing liberal democracy, prefer the rule of experts, and espouse a syndicalist model of social organisation and decision-making? Weren't the Frankfurt School theorists – both generation I and II – dismissive of the results of public discussion under 'liberalism', treating political discussion and contention as a machine producing false consciousness? Didn't the farther Left, especially after Lenin, believe in vanguardism and the necessity for moving the forces of history along by violence rather than persuasion – did they not look, in the phrase of Fidel Castro, for absolution by history rather than the present consent of the governed? Didn't the principles of 'no enemies on the Left' and 'no criticism of socialist countries' mean that Left-wing scientists shut their mouths about Lysenko and the liquidation of his scientific enemies by Stalin? Didn't the Left prefer a view of science as technology (Boris Hessen and Bernal) or description (Karl Pearson) or 'pragmatic' (Habermas) rather than one in which theory and debate played a large role? And wasn't the Lysensko affair itself a product of the idea that genetics was so much idealist theory while 'vernalisation' was a working technology? Wasn't part of the attraction of science – and for that matter planning – for the Left that science promised to provide a substitute for public discussion and 'political' decision-making? Wasn't 'planning', to which the people would assent but not control, the big idea that united science and the Left at least until the mid-1950s?

The answers to all these questions is 'yes'. So part of what makes reading Fuller's argument so intriguing is watching him pick his way through this minefield, trying to emphasise the claims that allow for a conception of science that fits with the present instincts of the Left. One can see where he wants to go: he has the idea that both science and liberal democracy would be better if the autonomy of science was cancelled and science was both criticised and governed democratically, and if science itself was more of an open society with freer criticism and debate. Fuller is a participatory democrat with a libertarian streak, and not an Old Leftist. So he ignores the traditional hostility of the Left to liberal democracy, and dismisses the contemporary manifestations of this hostility (such as cultural studies, which treats citizens as only enjoying sham freedoms and as captives of propaganda concealed as entertainment or education). And he tries to construct a counter-history that supports the idea by finding an historical missed opportunity. He finds material for this reconstruction, strangely enough, in commonalities between Theodor Adorno and Popper, who usually have been regarded as arch-enemies hurling accusations at one another – Popper treating Adorno as a purveyor of holistic gibberish arrived at through a bogus supra-scientific 'dialectical' method that was all too congenial to totalitarianism; and Popper, as a rigid excluder, through his restriction of science to the falsifiable, of the only kind of critical reason that could overcome the enslaving false consciousness of 'bourgeois' liberal democracy.

So what are the commonalities? Fuller says that they were both against superstition, which seems pretty thin, and argues that the text version of the *Positivismusstreit* (Adorno et al., [1969]1976), a debate that began in 1961, which pitted Popper as the 'Positivist' (a title he of course rejected) against Adorno the dialectician, actually pointed to a great deal of common ground. So what was it? They both believed in the dialectical process and criticism, and, as Fuller construes them, both were hostile to academic specialisation and disciplinary boundaries and to the underlabourer conception of philosophy. He adds to this the historical claim that Popper was connected to a Socialist pedagogical programme in the 1920s and implies that some of the Leftism of Popper's youth carried over into his later thinking, which he construes as unconventional and congenial to Socialism. He also tries to save Adorno from his mechanical appropriation by cultural studies, which Fuller dismisses as a misunderstanding, though he concedes that Adorno in later life fell into obscurantism and political ineffectiveness.

Interesting, but not really credible. The contrast between them could be put in a somewhat cartoonish way as follows. Adorno and Popper believed in dialogue and reason but in conflicting senses. Popper understood science to be a forum for discussions with reasonably well defined boundaries. The statements that could be advanced as conjectures in this forum were admissible only if there could be falsifications of them. When Popper talked about democracy in *The Open Society and its Enemies* (1950) his definition of liberal democracy was parallel government by discussion, which is itself bounded, though not quite as sharply or in the same way as discussion in science is bounded. The place in which his discussion of the conditions of liberal democracy, which includes these limitations, is most explicit is in his essay 'Towards a Rational Theory of Tradition' ([1963]1965), which aligned him with Polanyi, with whom he had a brief intellectual flirtation, and who shared the experience of coming from Central Europe and discovering how differently political discussion was conducted, and admiring the British way.

What undermines and amounts to non-participation in this limited discussion, which is the 'open' part of the open society, are forms of so-called 'reason' that are designed to shut the process of discussion down by reaching to some higher level of truth, such as the religious, which is why liberal democracies exclude religious issues from politics. Among the examples Popper gives in *The Open Society* of its enemies is the kind of sociology of knowledge practiced by Karl Mannheim, which, as Popper tells it, attempted to discredit positions Mannheim disagreed with by analysing them as concealed statements of class interests, rather than something to be rationally debated. Worse, Mannheim based his position on a claim to have a method for apprehending wholes that went beyond the usual methods of science or common sense reasoning. This is a pretty good description of Adorno's approach as well. Adorno was committed to the idea that there was some kind of overarching knowledge, to be reached by a special method.

A little historical context is relevant here. The whole debate over holistic versus specialised knowledge that led to the so called crisis of the sciences in the Weimar Republic was started by Max Weber's speech on the vocation of science in which he argued that science or in this case *Wissenschaft* had no business providing holistic, practice-informing, interpretations of the world, because science had developed, necessarily, in such a way that the only real achievements were those of specialists. This was at the time a shocking and disappointing thesis – to 'Mandarins' and students alike, *pace* Ringer – which occasioned responses and fervent contemplation from a great many people including Horkheimer, who jumped in on the side of holistic reason in his inaugural lecture, giving a Marxian twist to what was in origin a thesis of the Romantic Right.

Whether Weber's claims were right or wrong, this much seems to be the case. There is a kind of 'liberal' conception both of science and democracy which is procedural in the sense that it identifies both liberal democracy and science with bounded discussion, governed under rules of a game that limited and excluded or bracketed certain kinds of values, claims, and philosophical theses. If we recognise the limitations of political discussion and liberal democracies and the limitations of science, and recognise that both spheres work to the extent that they do because they limit discussion in specific ways, one can still be a critic and concerned with the revision of the conventions, but to claim the authority of reason with a capital 'R' in the context of either science or liberal democracy is to reach outside of the domain, to put oneself in a position beyond the limited kind of persuasion possible within the conventions of liberal politics and of science.

This is far from being an academic issue, and here Fuller is puzzling. The rise of Hitler is often (and better) seen as the consequence rather than the cause of the collapse of the Weimar republic, and since 1933 a major historical theme has been the assignment of responsibility for this collapse. The Left was divided between the KPD (the Communists) and the SPD (the Socialists and party of Marx), and the SPD was always watching its Left flank. The SPD served in coalitions with the Liberals, a small fractional group, but was uncomfortable in power and preferred opposition. Taking political responsibility was alien to the SPD, which was ideologically rigid and narrow in its interests. Many historians have pointed the finger for the failure of the Weimar Republic at the SPD because, as the larger party, it seemed to have the capacity to do something, but lacked the will and flexibility to do it. The collapse in 1930 of the last cabinet based on a parliamentary majority was precipitated by the refusal of the SPD, a refusal pressed by its union constituents, to agree to a small alteration of unemployment benefits. Rule by decree followed immediately, and the Nazi electoral triumph, a gain of ninety-five *Reichstag* seats, occurred at the next election.

Fuller strangely portrays this as a period of creative political ferment between liberals and Socialists and suggests that Popper was part of this ferment and himself a kind of liberal Socialist. It is true, as Fuller suggests, that on the Left there was a certain amount of creativity – Paul Tillich's *Kairos Kreis* comes to mind. But the creativity was not spent on saving liberal democracy. Much of it was spent appropriating the critiques of liberalism invented by the Right, especially its communitarianism and hostility to individualism or 'atomism'. Tillich himself was an adjunct professor at the proto-Nazi Hans Freyer's Sociology Institute in the 1920s (Muller, 1987: 154–155), and Freyer himself was respectfully reviewed by the Socialist press. None of this creativity was spent where it was needed most, namely on promoting the kind of political civility that made democratic politics possible, on curing the Left of its moralism and preference for critique over accepting political responsibility. The post-war amalgam of 'bourgeois' notions of political freedom and ideas of rights with social welfarism was very far in the future, especially for Germany. When SPD Chancellor Helmut Schmidt, fifty years after the Weimar Republic, embraced Weber's ethics of responsibility, it was front page news, precisely for this reason. Not responsibility but critique was the SPD legacy.

The liberal view of science is that science is a problem because the state needs science but the state ought not to be giving preference to opinion. So if science is opinion, and the liberal view is that it is, something needs to be worked out in the form of an exception. Polanyi argued that the wise liberal regime recognises science as a kindred activity which deserves support and also some degree of freedom from accountability as long as it is genuinely open and competitive. Don Price took the view that science needed to be treated like the established church, as an estate (1965), which was also similar to Polanyi's view.

Conant took a tougher line than Polanyi in a series of important books (1947, 1951, 1952) that polemicised against the Left view of science, represented by Karl Pearson, which had been the key source for the British scientific Left (Porter, 2004: 278). Both he and Polanyi had a liberal approach to science, 'liberal' in the following sense – they thought it was best to govern science indirectly, by facilitating and assuring competition.

Conant had a deflationary view of science. He argued that it was continuous with common sense, and pointed out that far from being political paragons scientists were particularly prone to hobby horses, to over evaluating the significance of their own little patch of turf, to excessive faith in their own theories, and to group-think. Indeed, if one reads Kuhn through Conant, far from celebrating the conservatism and tendency to close ranks of scientists, these characteristics of scientists are among their principal defects and the reasons why their claims required scrutiny. And Conant didn't trust them, arguing for the creation of devil's advocates and novel forms of examining technical proposals rather than merely accepting whatever proposal the scientific consensus favoured. Nor did he encourage what he called the idolatry of science. Not for nothing was Conant loathed by his fellow chemists, who were appalled at his becoming President of the AAAS. Nor was Conant politically quiet. He devoted a huge personal effort to reforms of schools to make science education accessible to all students of talent and to promote equality of opportunity – making, incidentally, Ivy League educations possible for people like Fuller himself. Conant, speaking as a distinguished scientist, did as much as anyone to discredit the idea that science could be a substitute for politics or that scientists had special politically relevant virtues. Hollinger's famous essay on *'laisssez-faire communitarianism'* (1996) asks what happened to this idea. One answer might be that Conant killed it.

Polanyi, Conant, and Popper are in the same political family, a family of liberals and anti-planners. Fuller, though he does not want to be, is part of this family as well. The force of his arguments depend on the liberal parts of his Leftism, especially his hostility to scientific authoritarianism, and not on the 'Left' parts. He has little in common with the tradition that follows from Pearson. What makes him 'Left' is his insistence on the importance of critique. But the troubles of the SPD with its devotion to critique raise a question which I would like to see Fuller address. At one point he commends Conant, perhaps ironically, for his realism. To what extent is the 'realism' which he attributes to Conant simply a matter of acting in accordance with the Weberian ethic of responsibility? Or to put it differently, how much 'Left' attitudinising about science is a matter of falling back into the old habit of 'critique' from an overarching viewpoint above that of ordinary political agents and indulging in the pleasures of occupying the high moral ground without taking responsibility? This is a question that his own comments on Kuhn's irresponsibility and his praise of Foucault as a public intellectual make unavoidable, and it is critical to the project of the creation of a Left rationalism about science. For a responsible Leftist, the attitudinising of a Foucault is not enough. And one wonders if the responsible view of the governance of science wouldn't, 'realistically', wind up to be pretty close to the present reality. I doubt that 'democratic control of science' can ever be more than a slogan. Who would hold power in a functioning 'democratic' governance regime in science? I do not know. A sure bet, however, is that it wouldn't be 'the people'.

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Author's Response

By Steve Fuller

Kuhn vs. Popper is published in a series called 'Revolutions in Science'. The debate I discuss constitutes a 'revolution' in the sense of a massive shift in the burden of proof about how one describes, explains and justifies the existence of science. It is important to be clear about what is involved in the process of shifting the burden of proof, a theme that has run continuously from my PhD dissertation through the development of social epistemology, (Fuller, 1985, 1988: Chapter 4; Fuller and Collier, 2004: Chapter 10). It refers mainly to a shift in second-order, not first-order, beliefs. For example, two societies may have comparable levels of racist beliefs, yet in the first these beliefs are taken for granted, whereas in the second one is always forced to show their relevance to the case at hand. When the law is used to correct prejudice, its intent is typically to shift the burden of proof in this sense. There is no pretence that people's beliefs will change overnight. Rather the law engages in the more indirect strategy of changing the world so that it becomes harder to utilise those beliefs as a basis for action.

The overall effect of Kuhn's *The Structure of Scientific Revolutions* has been to shift the burden of proof in just this sense – specifically, to make a universalist, formalist approach to science appear *prima facie* implausible. Of course, universalist and formalist tendencies remain in the philosophy of science, but they are restricted to high-rent ghettos like Bayesian epistemology that are safely ignored by science and technology studies (STS) researchers. Kuhn himself might not have welcomed this impact of his work, and those influenced by Kuhn might not necessarily agree with his exact account of science, let alone its underlying motivation. But with the passage of generations, the default settings have shifted the burden of proof in Kuhn's favour. A good way to appreciate the 'objective reality' of Kuhn's dominance is to look at the struggles faced not by the old philosophical guard but by the self-declared *avant garde* elements in STS. They too are impeded by Kuhn. Only those intimate with STS's internal debates realise the stakes in claiming that science constitutes a 'heterogeneous network' rather than a 'paradigmatic community'. To more casual observers, including sympathisers like Richard Rorty, Bruno Latour simply looks like a Maxwell to Kuhn's Newton. Thus, Kuhn's long shadow blocks recognition of other would-be radical shifts.

However, David Mercer does not suffer from this problem. He appreciates the heterogeneous character of science, perhaps too much so. That science exists in complex relations with the rest of society is a banality – not a discovery of STS, let alone Kuhn, who was always guarded on this point. The interesting question is how to deal with this obvious fact. One can either take the complexity as a model for one's own practice or think that somehow things would be better by altering that complexity in the name of 'improving', 'realising', 'reducing', 'focusing' and the like. Is this point insultingly clear or deeply counter-intuitive? For me the blurring of these alternatives marks our post-Kuhnian condition. Kuhn undeniably helped to make more of the history of science accessible to more non-specialists. However, in the process, he – perhaps unwittingly – conveyed the impression that previous historians, philosophers and scientists knew less about the history of science, whereas they simply had a rather different attitude toward it.

After all, is it plausible that politically engaged scientists from, say, 1850 to 1950 had an inferior understanding of the multiple contexts of research than today's studiously apolitical STS researchers? Of course not. However, because these earlier scholars were trying to contribute to science's future, not simply trying to understand its past and present, they presented the historical aspects they highlighted as pointing in a certain direction – and, not surprisingly, ignored or downplayed aspects that worked against their case. Kuhn was no less selective but without all the pointing (Fuller, 2001). Indeed, I believe that he was strategically disengaged from the normative side of contemporary science, unwilling to second-guess what his mentor James Bryant Conant and others were doing to preserve science's integrity in Cold-War America. The bulk of the ascendant generation of STS researchers have taken Kuhn's disengagement as a default epistemic attitude, albeit without his own historically specific reasons. This sums up my case against Kuhn and how it bears on today's concerns.

Mercer rightly argues that legislative and judicial uptake is an important context for evaluating the impact of – and hence responsibility for – one's ideas. However, perhaps because our time is preoccupied with 'precautionary' principles, I need to stress that the relevant sense of 'responsibility' pertains not to anticipating and preempting bad consequences but to admitting and rectifying such consequences once they happen. This point is especially significant for the concept of negative responsibility, which regards both action and inaction *vis-à-vis* a set of options as equally fraught with risks and uncertainties. Indeed, by identifying intellectual responsibility with negative responsibility, I am conceding that your hands are always dirtied whenever you unleash an idea into the public domain – and conspicuous silence may be among the loudest ways to affect the flow of ideas. There are no safe options. The question of intellectual responsibility turns on whether – and to what extent – you then engage with that fact.

I can envisage three levels of engagement, ranging from most to least blameworthy: (1) You refuse to engage with consequences at all, as if your ideas amounted to a 'message in a bottle'. (2) You ignore or refuse responsibility for the bad consequences while gladly accepting responsibility for the good ones, a posture intellectual historians seem to tolerate and even indulge in their subjects. (3) You engage with both sorts of consequences, perhaps even suggesting their mutual entanglement. In preferring (3), I am undoubtedly demanding much of intellectuals. But I regard intellectuals as occupying a special social role, one in which ideas are primarily things produced for public consumption, rather than, say, representations of private mental states, as ideas might be to non-intellectuals, who would then be justified in a more limited engagement (Fuller, 2003). This point may help to clarify Stephen Turner's puzzlement at my unflattering comparison of Kuhn with Foucault. I only mean to commend the intellectual's relationship to his ideas that is exemplified in Foucault's public responses to interpretations of his work that were as wide-ranging and controversial as interpretations of Kuhn. Although he became notorious for announcing 'the death of the subject', Foucault's own practice suggests someone who felt himself very much responsible for the impact his words had on others.

Before turning to Mercer's interesting discussion of the fate of Kuhn and Popper in the hands of the legal system, let me respond to the valuable and generous – both to me and to Kuhn – observations of Jerome Ravetz. In the seven years I spent writing *Thomas Kuhn: A Philosophical History for Our Times* (Fuller, 2000), I was counselled – especially by my editor, the late Susan Abrams – not to lodge any personal charges against Kuhn. Two main reasons were offered: (1) I lacked conclusive evidence that Kuhn did anything especially wrong. (2) It is bad form (a.k.a. 'the genetic fallacy') to blame major thinkers for the bad consequences of their ideas. (The advice continued even after Kuhn died in 1996.) I found these reasons compelling only as a veiled threat, along the lines of 'Judge not, lest ye be judged'. I could not accept them as genuine intellectual reasons, mainly because it set the empirical standard of moral accountability so high that all thinkers would be excused for their ideas. (Fuller [2004a], an omnibus response to forty critics, elaborates on this point.)

However, as I have already suggested, I believe that intellectuals have special obligations by virtue of their peculiar relationship to ideas, which is typically manifested in their unique social position. Unfortunately, these obligations are rarely enforced, perhaps because the enforcers fear a similar fate might befall them in the future. However, exceptions to this rule can be found in the treatment given to intellectuals safely outside 'our' moral universe – such as sympathisers with Nazism or Soviet Communism. In that context, it seemed, all sorts of sophisticated (albeit controversial) things could be said about the terms on which intellectuals are accountable for their ideas. Why couldn't similar things be said about the intellectuals to whom we retain some moral attachment?

Ravetz provides an enriched account of the culture of Cold-War America that made Kuhn frustrated with science's trajectory yet reluctant to speak out against it. However, the question before us is whether Kuhn had some special obligation as an intellectual – above that of even the ordinary academic – to do more than he did under the circumstances. Kuhn's considered strategy was to keep his head down and advise his students similarly (e.g. *Kuhn vs. Popper* reports Kuhn's lengthy correspondence with Paul Forman, advising him to finish his PhD and stay away from the student unrest at Berkeley in 1964–1965). I now regard this strategy, which for Kuhn was the path of least resistance, to have been an act of cowardice. This was not my original view. Nevertheless, I became drawn to it by the voluminous correspondence in the Kuhn archive at MIT, which I examined only in the course of writing *Kuhn vs. Popper*. The archive documents the multifarious opportunities Kuhn was offered – and duly avoided – to declare the political and policy implications of *Structure*. However, Kuhn's massive sin of omission, his failure of negative responsibility, truly came into focus once I encountered colleagues who envied Kuhn's freedom to pursue a life of the mind unfettered by external standards of accountability. What had begun to strike me as Kuhn's loss of nerve came across to them as an assertion of autonomy! For the first time, I could appreciate the cynicism of those who think that 'free inquiry' is merely a euphemism for pure licence: i.e. doing whatever you want, no questions asked.

Kuhn vs. Popper ends by posing a choice to the reader: either Kuhn's studied inaction is excused by the repressive conditions of Cold-War America or, if the US turns out not to have been so repressive, Kuhn's inaction is simply an instance of intellectual irresponsibility and moral cowardice. Ravetz appears to favour the first option, which would certainly justify his own migration to the UK and, at the same time, exonerate his friend Kuhn who remained in the US. However, I am not sure that this is the option I would take. In any case, I appreciate Ravetz's acceptance of the terms of engagement because once we refuse to admit an easy third way out of this dilemma, we realise what is at stake in passing judgement on Kuhn for our understanding of contemporary history – let alone the history of science or STS.

For his part, Mercer seems to think that Kuhn's failure to engage critically with the consequences of *The Structure of Scientific Revolutions* for the politics of science may have had the positive unintended consequence of permitting the radical (mis)readings of his work associated with feminism and environmentalism. (Mercer grants my claim that Kuhn meant to be presenting a much more conservative view of science than his admirers have supposed.) Of course, a silver lining can be found in even the greyest of clouds, yet I wonder about the exact benefit that these progressive movements have derived from Kuhn's work over the past quarter-century. Yes, they have cited Kuhn a lot, but has it helped to advance their causes? I would argue that, insofar as 'Kuhnification' has made any substantive difference, it has been to leave the impression that simply articulating an alternative point-of-view, or 'paradigm', is sufficient for making one's point – i.e. without having to confront, let alone convince, the opposition. In that respect, Kuhn's incommensurability thesis has helped to provide an epistemological basis for the fragmentation of the public sphere and the conversion of formerly oppositional parties with universal aspirations into self-selecting species of lifestyle politics. If 'progressive' movements have felt that Kuhn has aided their causes, it is probably because they have lowered their expectations in the process.

On the Popper side, Mercer is certainly right that falsificationism has been invoked to set the standard for admitting scientific evidence so high as to allow judges to dismiss many – if not most – scientifically based risk assessments made in the public interest. Like Mercer, I too regret that falsificationism is used for this purpose, and agree that Popperians should do more to counter this interpretation of their principle. Indeed, they may well exhibit failures of negative responsibility comparable to that of Kuhn and his followers. Nevertheless, (again) lest we be incautious about our use of the precautionary principle, the idea of placing the burden of proof on those who would restrict already permitted action is not itself a bad one, and is in fact the cornerstone of any liberal approach to jurisprudence. The problem is that, in the cases to which Mercer alludes (analysed more fully in Edmond and Mercer, 2004), big business turns out to be the main beneficiary of the principle. Perhaps where Mercer and I disagree is that, unlike most STS researchers, I do not see the falsification principle as itself the source of the problem here.

One quick-and-dirty way of trying to disqualify the falsification principle as a standard for judging scientific evidence is to show that it fails to capture how science is ordinarily practised. This is certainly true, but for Popper, as for Kant, 'ought' implies only 'can' not 'is'. Here STS needs to engage with the relationship between legal and metascientific principles in a more creative fashion. It seems that when STS researchers study something intensively, institutional boundaries dissolve before their eyes. Here I mean STS's failure to recognise that, in their official guises, law and science are distinct entities that operate with different means and ends. Instead STS research has tended to blur the distinction between scientific and legal decision-making – stressing the discretionary and contested character of both, as well as highlighting the emergence of the quasi-legal entity, 'regulatory science'. Not surprisingly, STS researchers are wrong-footed when judges invoke principles of scientific method with a finality and rigidity they would not normally extend to legal principles. With that diagnosis in mind, I now offer some observations about the unfortunate uses to which Popper's principle has been put in legal proceedings.

The cases of concern to Mercer are tried in an accusatorial (or adversarial) legal system, which means that there is no judgment to deliver until a charge is raised and the judgment applies, in the first instance, to the case at hand. This contrasts with an inquisitorial legal system, which allows the judge to expand her investigation of the case to determine whether it is symptomatic of a deeper disorder. Now consider Popper's falsification principle in the light of these two legal systems. The principle has attracted controversy in philosophy of science because its strict application would end up discrediting most normal science as well as what might be called 'junk science'. (Imre Lakatos liked to raise this point to show that Popper was more radical – and less correct – than his reputation would suggest.) But, of course, if normal science is not itself on trial, then this aspect of the principle would not necessarily come up. Indeed, the rhetorical power of the label 'junk science' trades on our being neither tempted nor allowed to check systematically whether normal science does any better by the standards to which the alleged 'junk' would be held. It is simply presumed that normal science passes Popperian muster for the purpose of epistemically anchoring judicial decisions, even if the truth probably lies elsewhere.

Like the 'demarcation criteria' of the logical positivists, falsificationism was not meant to epitomise expert opinion but to provide an independent, possibly countervailing, standard that any suitably rational agent – i.e. one equipped with certain basic capacities of observation and inference – could apply to decide whether a knowledge claim merits attention or allegiance. The ideal legal context for applying such a principle is an inquisitorial system with its own scientists and testing facilities, not an accusatorial system in which science is represented only by experts and counter-experts hired by the claimants. 'Popperian justice' may be illustrated by a verdict that the Danish Research Council's 'Committee of Scientific Dishonesty' could have reached on Bjørn Lomborg's *The Sceptical Environmentalist*. Instead of focusing exclusively on Lomborg's own biased reading of the ecological data, the Committee could have demonstrated that systematic bias – in various ideological directions – can be routinely found in environmental science, not least because of the rather open ('underdetermined') relationship between the data collected and the knowledge claims at stake. The Committee could have then concluded that a principled view toward environmental scientists requires equal treatment – either condemning or condoning – of both eco-sceptics like Lomborg and eco-dogmatists like Stephen Schneider (Fuller, 2004b).

From the Popperian standpoint I am defending here, STS should be challenging the law's tendency to pigeonhole science as 'expertise', which over the years has bred 'counter-expertise', the overall effect of which has dissipated the significance of science in the judicial process. It is worth recalling that even though a strong sense of empiricism – i.e. the drive to establish difficult facts – motivated Hugo Munsterberg's invention of 'expert testimony' in the early twentieth century as a field of applied psychology, he still imagined that scientific experts would be working for the courts rather than the contestants. In any case, a strong distinction needs to be drawn between the role of science as a form of reasoning or decision-making procedure and as a producer of facts that bear to varying degrees on particular cases. The former, which has been the traditional focus of philosophical interest, should be embodied in a judge, jury or agent of the court, not a witness. Witnesses, including representatives of the scientific establishment, should be restricted to presentations of particular and general facts purportedly relevant to a case. This is much more in the spirit of 'expert', a word that originated as a contraction of the participle 'experienced'.

The court should not have to call expert witnesses on the nature of science any more than it needs to call expert witnesses on the nature of law. The court should be itself already equipped with a sufficient grasp of science as a form of inquiry to draw its own conclusions. Even witnesses who claim to enjoy the official backing of scientific associations should be under the same burden to establish the epistemic standing of their testimony. After all, the spirit of Popperian and other demarcation criteria was to allow one to tell whether scientists themselves truly uphold scientific standards. To be sure, all of this may require reform of how lawyers are trained, juries instructed, and scientists commissioned to work for the court. (Here STS – especially the normatively inspired side – could make some interesting contributions.) But that is the level at which the problem should be addressed – before actual litigation begins. Science itself should not be formally on trial every time a case of scientific relevance goes before the court. Of course, both science and law always need to change, but that is a matter for explicit legislation in forums specifically designed to evaluate past track records in light of current and future needs.

Precedents for my proposal can be found in the Legal Realist movement that informed the heroic period of Progressive and New Deal judicial activism in the United States in the middle third of the twentieth century. It was clear to the Legal Realists that a background in case law and personal experience (including one's own legal practice) was inadequate for making just decisions in a rapidly growing and diversifying society. For that reason, they called for the social sciences – understood broadly to include even psychoanalysis – to be incorporated in ordinary legal training and to inform judicial decision. Moreover, an important part of this training involved learning how to interrogate and evaluate expert testimony, so that the sheer proliferation of experts and counter-experts did not pose a problem to adjudication. A populist version of this sentiment, which I also endorse, can be found in the citizens juries and consensus conferences increasingly used at least as sounding boards for prospective legislation in matters relating to science and technology. Here the jurors take testimony from various parties, each of whom brings an 'expertise' to a matter on which the jurors must draft guidelines for legislation. What stands out is that, once instructed with some basic interrogational skills and provided the opportunity to exercise them on witnesses, the jurors succeed in drafting guidelines that are both politically and scientifically credible.

Finally, I turn to Stephen Turner, who has grasped perfectly the *raison d'etre* of the book under review. Although the book is called *Kuhn vs. Popper*, it is ultimately not about settling an old score in the philosophy of science but treating the wider intellectual confusion of which the named debate is symptomatic. This confusion is epitomised in the widely held belief that knowledge and reason may be qualities of the individual or of the collective, but not both. Furthermore, this forced choice typically carries some vague but no less momentous political import, as both individualists and collectivists declare themselves 'progressive' and their opponents 'reactionary'. These lingering philosophical residues of the Cold-War imagination obstruct any rapprochement between liberals and socialists as a unified party of the 'rationalist left' that I would promote. Of course, Turner is not guilty of this confusion, but nevertheless thinks that liberalism and socialism, with their respective emphases on the individual and the collective, are ultimately irreconcilable. The force of Turner's critique is that the actual history of these movements reveals that the 'rationalist left' is little more than a figment of my imagination. In the balance lies how one tells the history of the Left. I can do little more here than sketch what I take to be our differences on a matter that I hold to be of utmost relevance to the prospects of a normative social epistemology.

In terms of European history, for me the history of the Left is about universalising Athens at its most civic republican, whereas for Turner it is about escaping Rome at its most papal authoritarian. The former is Germanic (and is the history to which both Popper and Adorno allude for legitimacy), the latter Anglo-French in provenance. Some plot summaries are in order:

(1) *Fuller's Plot*: Classical Athens embodied the civic republican ideal of the citizen obliged to speak his mind about the collective interest. Such speech could occur with impunity because a citizen's livelihood – typically a hereditary estate – was itself not open to debate. Thus, citizens had no need to defend their interests openly, and were routinely discredited if they appeared to do so. Indeed, credibility accrued to those who appeared to speak against their interests, as a rich person who called for higher taxes. In any case, citizens could be voted down one day and return to contest policy the next. Athens provided, in most respects, a brilliant crucible for forging *res publicae*, 'public things' of value beyond the aggregate of self-interested citizens. The one not-so-small problem was that the Athenian ideal was available only to a few whose leisure was purchased on the backs of the majority who remained enslaved. In that case, the political version

of squaring the circle is how to preserve a robust sense of Athenian liberty while extending citizenship to an ever larger and more diverse population. The more successful attempts have involved expanding the polity's production of wealth, typically by minimising the use of human labour, and systematically redistributing its fruits to minimise inter-personal differences in status. However, these attempts have faced three main obstacles: (a) the division of labour that accompanies expansion breeds a *de facto* rule by experts that undermines the sense of equality needed to speak one's mind with impunity; (b) patterns of consumption outpace those of production, resulting in the fiscal crisis of the welfare state; (c) groups historically removed from the Athenian genesis find their assimilation culturally oppressive. It follows from this history that liberals and socialists basically share the same ends but disagree over the means, the former preferring economic and the latter political innovation. If liberals are reluctant to extend liberties in order to preserve already existent ones, socialists are too willing to reduce liberties in order to extend them to more people.

(2) *Turner's Plot*: Before the Peace of Westphalia in 1648, the legal fiction obtained that Europe was unified under 'Christendom'. As this fiction became subject to increasingly divisive interpretations, it lost its grip on the political imagination. But what was to replace it? A geographically bounded version of the Pope's absolute sovereignty, as embodied in a monarch? Or, a secular version of the work normally performed by the papal agents armed with 'natural law', namely, conflict resolution when the local parties are incapable of settling their differences? The first option traces the demise of Christendom to the errant ways of Roman Catholicism, but nevertheless insists that a just and orderly society must be closed under a set of (genuinely true) beliefs. This is the source of the communitarian epistemology that informed Calvinist theocracy, Saint-Simonian technocracy, and Soviet bureaucracy. The second option diagnoses Christendom's demise precisely in terms of Catholicism's hubristic insistence on closure under a set of beliefs. Accordingly, piety to God is best shown by tolerance for each sincere believer's mode of spiritual access. Thus, the only allegiance required of citizens is to a set of procedures for resolving conflicts that arise as a by-product of the free pursuit of conscience (later secularised as self-interest). This is the source of the libertarian epistemology that informed Hobbes and Locke and underwrites modern liberalism. It follows from this history that socialists and liberals hold irreconcilable views about what is salvageable from the demise of Christendom, and that the modern state is a politically unstable entity

ever prone to excesses of totalitarianism or anarchism, as long as both ideologies are in play. Weimar Germany is the obvious case in point.

As Turner's critique suggests, *Kuhn vs. Popper* moves between Weimar Germany and Cold-War America to capture the formative contexts of Popper and Kuhn, respectively. I shall pass over Turner's interesting, albeit (ideologically) liberal, interpretation of the fall of the Weimar Republic to focus briefly on what he says about the United States. Generally speaking, I believe the intellectual history of the United States fits the liberal side of Turner's plot with a couple of big exceptions, which were already present in the original 1787 Constitution, namely, the nation was born with expansionist ambitions and the promotion of science (specifically through the establishment of a patent office) was singled out as a vehicle for increasing the commonwealth. This fits my plot better, and not surprisingly Hegel found Jacksonian America a possible resting place for the world-historic spirit. Moreover, Kuhn's mentor, James Bryant Conant, must be counted among those who succeeded in 'Germanising' the United States in the twentieth century by presenting the nation to itself as what we would now call a 'knowledge society' that aspired to stratify and integrate education and employment, while instituting an explicit mutual dependency between science and the state. Turner is right about all this, and indeed I meant to be quite sincere – not ironic – in giving Conant's political realism higher marks for intellectual responsibility than Kuhn's political quiescence. This is not to say that, in Conant's position, I would have made the exact same decisions – and tradeoffs – as he did. However, unlike Kuhn, Conant grasped something of the world-historic spirit and tried to make the most of it.

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REVIEW SYMPOSIUM

THE STORY OF H_2O?

David Philip Miller, *Discovering Water: James Watt, Henry Cavendish and the Nineteenth-century 'Water Controversy'*. Aldershot and Burlington, VT: Ashgate Publishing, 2004. Pp. xii + 316. £55.00 HB.

By Jan Golinski

Like other historians of science in recent years, David Philip Miller has shown the value of studying a controversy. Debates between scientists bring their underlying assumptions to the surface, exposing to scrutiny beliefs that are normally hidden from view. A prolonged controversy can stir deep waters indeed. As scientists dispute one another's claims, they raise questions of method, competence, and propriety; sometimes they even challenge their opponents' ethical or political motives. All of this yields grist for the historian's mill. The standard procedure for analysing such incidents, which Miller follows, is to start with the central issues in question and then work outward to the relevant context. The recommended approach is a 'symmetrical' one, which avoids taking sides on the topic under dispute and treats both parties' positions as equally in need of explanation.

Miller's controversy is atypical, however, in that it concerned not with a factual claim but a historical one: the question of whether James Watt or Henry Cavendish had discovered the composition of water in the 1780s. More than half a century later, the question was heatedly debated by practising scientists who seemed to be taking history unusually seriously. A lot was at stake in deciding who deserved credit for the discovery, as Miller shows. Advocacy of the cause of Watt or Cavendish had implications for the social profile of British science in a critical period of institutional formation. In the 1830s, Cavendish was the hero of the specialist vanguard, the 'gentlemen of science' who set about a comprehensive reform of the

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scientific scene. By the middle of the nineteenth century, supporters of Watt's claim were discernibly marginalised in the discipline of chemistry and in the leading scientific institutions of the nation.

Miller does not propose to arbitrate the controversy by stipulating who really discovered the composition of water. His interest is focused on the more general question of why people assigned the discovery to one individual or another. He does, however, offer a carefully neutral account of what happened in the early 1780s, when Watt, Cavendish, and others were investigating the ignition of 'inflammable' and 'dephlogisticated' airs, subsequently identified as the gases hydrogen and oxygen. He carefully teases out who said what, when, in what context, and how it was interpreted. He indicates the somewhat tense political background of the time, in which Watt was tied both to the Scottish chemical tradition and to his Birmingham friends in the Lunar Society, while Cavendish was more closely allied with metropolitan institutions and Sir Joseph Banks's 'learned empire'.

For the most part, however, Miller's focus is on the fierce squabble of the 1830s and 1840s, when champions of Watt and Cavendish fought out their dispute long after the two heroes had been laid to rest. Watt's claim to have discovered the composition of water was loyally advanced by his son, James Watt Jr, taken up by the French physicist François Arago, in an *éloge* of Watt delivered in 1834, and then developed by a group of Scottish writers that included Henry Brougham, Francis Jeffrey, and J. P. Muirhead. The counter-attack on behalf of Cavendish was led by W. V. Harcourt in an address to the British Association for the Advancement of Science in 1839, and backed up by members of the Cambridge network then asserting its control over the new association, including William Whewell and George Peacock. Miller clearly lays out the various dimensions of the debate. Methodologically, Watt stood for a simple – to his critics, naïve – empiricism. For his son, temporal priority was the only issue: Watt had been the first to declare that water was a product of two gases, and quibbles as to what exactly he understood by this were nothing but sophistry and evasion. The younger Watt asserted that his father deserved credit for the discovery as he deserved to hold patents on his inventions; it was a straightforward question of property rights. At the same time, Watt was regarded by his defenders as having properly made his findings public, in contrast to the reclusive and taciturn Cavendish. The Scottish party saw

Watt as a model of virtue in this regard, and they suspected the Cambridge group of political animosity to the nexus of religious dissent and industrial utilitarianism that he represented.

From the other side of the fence, the issue looked quite different. Cavendish was seen as commendably detached from utilitarian concerns and hence a superior 'philosopher'. His work on precise measurements of physical quantities greatly impressed Whewell, Peacock, and their associates. To them, he exemplified the mathematical approach to the physical sciences that they sought to establish as the foundation of all scientific method. Elevation of Cavendish to the Cambridge pantheon helped secure patronage from the Cavendish family, which established – and gave its name to – a professorship and a laboratory later in the century. Many leading chemists, like the Edinburgh lecturer George Wilson, came to share this vision of a science founded on precision and quantification, for which Cavendish was an appropriate hero. It also seemed that his understanding of chemical theory was less contaminated than Watt's by the notion of phlogiston and by the material theory of heat, both discredited concepts by the early nineteenth century. By 1850, the dispute was clearly tending Cavendish's way, and in the second half of the century attributions of the discovery of the composition of water to Watt had almost entirely disappeared from textbooks and encyclopaedias.

Miller's account of all this is cogently presented and grounded in extensive archival research. He persuasively demonstrates how much of nineteenth-century British science can be understood by reference to this debate. But his study has an additional value for those (such as myself) whose primary interest is in the eighteenth century. Miller's work helps to address the historiographical difficulties caused by the fact that the earlier period largely comes to us mediated by later interpretations. This poses particular problems in relation to chemistry, where the eighteenth century was divided from the nineteenth not only by dramatic political and social changes but also by the comprehensive theoretical restructuring initiated by Lavoisier. Chemists in the early nineteenth century found their backward view obscured by fundamental changes in theory, such as the disappearance of phlogiston and the displacement of other 'imponderables' (heat and light) from their prior centrality in the discipline. The chemistry of just a few decades earlier seemed conceptually very distant indeed.

Furthermore, as Miller shows, Cavendish became a hero of nineteenth-century chemists through the work of scientific reformers who fairly comprehensively rejected the legacy of the provincial Enlightenment with which Watt had been associated. The transformation had methodological and political – in addition to theoretical – dimensions. Watt, and particularly his colleague Joseph Priestley, appeared to later chemists as paradoxical figures: apparently progressive in politics and industrial innovation, but wedded to the antiquated notion of phlogiston. Even some aspects of the work of Cavendish himself were lost from view, including the interest in processes of 'phlogistication' that had led him to explore the composition of water in the first place. In the early 1780s, phlogiston was viewed by some as a proven reality, by others as a hypothetical concept with widespread explanatory uses. For example, the members of the Coffee House Philosophical Society in London invoked it to explain phenomena from the domains of chemistry, heat, and electricity. In this group, phlogiston served as a resource for free-wheeling and speculative discussion, ranging across many of the natural sciences; it played an important role in an institution for scientific conversation that was part of the enlightened public sphere (Golinski, 2002). The situation subsequently became obscured, not just because phlogiston was dethroned, but also because Enlightenment values were severely challenged in British public life. As Miller points out, by the fourth decade of the nineteenth century, even the defenders of Watt were declining to enter into interpretation of his ideas, uncoupling him from his background in the chemistry of heat that had been pioneered in enlightened Scotland. The advocates of Cavendish and of Watt both participated in a process by which a veil was drawn across their own history, dividing them from the intellectual world in which their heroes had worked a few decades before.

Miller's excellent study indicates how this gulf of misunderstanding was created. The argument over whether Watt or Cavendish discovered the composition of water produced agreement on the value of Cavendish's style and methods of science. But, paradoxically, the settlement also consolidated a kind of historical discontinuity that made it more difficult to understand the perspectives and motivations of both figures. A controversy about the history of science ended up with a superficial historical account, which was then digested and purveyed in textbooks and encyclopaedias. Subsequent historians have had to work hard to get behind this packaged version, to build a richer historical understanding of British chemistry in the late eighteenth century.

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By Simon Schaffer

A couple of years ago I was asked by the editors of the new *Oxford Dictionary of National Biography* to contribute an extensive entry on Henry Cavendish, hero of enlightened English natural philosophy. The task seemed straightforward. Though judged notoriously taciturn in an age which loved to talk, and hesitant to publish his impressive chemical and physical experiment and speculation, Cavendish was well documented in a pious Victorian biography by the Scottish chemist and Edinburgh technology professor George Wilson (1851), along with cleverly annotated collections of his electrical papers by James Clerk Maxwell (1879) and of his chemical papers by Edward Thorpe and his colleagues (1921). Better, Russell McCormmach and Christa Jungnickel had just produced a remarkable biography of Cavendish in 1996, and then published all his extant correspondence and his decisive manuscript on heat theory. Cavendish's position as pre-eminent exponent of precision experimentation, prudent development of Newtonian mechanics in novel areas of heat, electricity, pneumatics and chemistry, and thus a symbolic figure for the next generations of British gentlemen of science, seemed well established. There were just enough anecdotes of strangely silent seclusion to colour a stolid account of public service, ingenious instrumentation, and careful inquiry. In his own pursuit of telling judgments of Cavendish's character, David Philip Miller notices that the great chemist has recently been retrospectively diagnosed with Asperger's Syndrome, Wilson's biography seemingly providing Oliver Sacks with 'almost overwhelming' evidence for this judgment.

So far, so good. But when I turned to Wilson's book itself, and to the original *Dictionary of National Biography* entry, composed in the 1880s by the prolific mining engineer and science lecturer Robert Hunt, my serenity evaporated. I was unwittingly and inexorably drenched by the storms of the Water Controversy. Both Wilson and Hunt devoted the bulk of their work to this fight, vindicating Cavendish from charges by the admirers of James Watt of aristocratic corruption and robbery in a century of contests about rights to the discovery in the early 1780s that water is a compound of hydrogen and oxygen. Hunt confessed that "in the history of chemistry we do not find any discovery which has led to the same amount of angry discussion". In a chapter on the Water Controversy of over one hundred pages, matched by an appendix of twice that length, Wilson confirmed that it was astonishing that "a matter so simple as the question who first made a single chemical discovery should have been found so difficult of decision" and that "it is vain to attempt to lay down stringent rules for the adjustment of disputes as to the priority between discoverers in science".

It might therefore seem brave if not eccentric of Miller here to resuscitate such a vicious yet seemingly sterile dispute. As we would expect from the author of a pungent critique of the 'Sobel Effect' published in this journal two years ago, his is not a 'true story of how a lone genius solved the greatest scientific problem of his time'. He rather explains why and how such stories were told and ever judged true. Miller's interest is thus not entirely distant from Wilson's: rival assessments of claims to the discovery of water's composition reveal much of broader scientific, philosophical, political and technological enterprises in British society. Miller's success is in no small measure due to the fact, however, that he also approaches the controversy without some of the concerns of his predecessors. He refuses to resolve the dispute: 'maintaining neutrality is a strain', he concedes. The task requires careful navigation through the reefs of scientific historiography, and Miller proves an astute pilot. Since the minutiae of 1780s chemistry were documented decades later by highly partisan writers, it requires keen judgement to construct a comprehensible narrative of what then passed between Watt, Cavendish, and colleagues in Birmingham, London, and Paris.

In the laboratory projects and philosophical correspondence of those years, especially those of the Lunar Society of which Watt was a leading member, eudiometric spark experiments on air mixtures by another Lunar Man, Joseph Priestley, to check whether heat had weight, were developed by Cavendish in London to show that pure water is condensed from a proportionate mixture of dephlogisticated air, phlogiston deprived of water, and inflammable air, water bound to phlogiston. Meanwhile, James Watt told Priestley in spring 1783 that water is dephlogisticated air combined with inflammable air once they had lost their substantial latent heat. Cavendish denied there could be an elementary substance of heat, while Watt reckoned steam would turn into an air when its latent heat was lost. Antoine

Lavoisier learnt in summer 1783 via the London Royal Society of Cavendish's trials, and set out to repeat and extend them. The following year, in the midst of furious fights at the Royal Society about its President Joseph Banks' authority and politics – familiar turf for Miller, this – Cavendish had his report published, Watt's friends charged plagiarism, and in their turn Watt's views were printed in the Society's journal. This story was set out pugnaciously by Wilson and his contemporaries, then in all its details by the eminent chemistry historian J. R. Partington in 1962. It is on this basis, for example, that the new *Oxford DNB* entry on Watt judges that he 'nearly anticipated' Cavendish and Lavoisier in discovering the composition of water. Despite Watt's private grumbles about the pretensions of the financier Lavoisier and aristocratic Cavendish – 'rich men may do mean actions' – my entry on Cavendish reports simply that Watt was 'eventually persuaded of Cavendish's good will'.

Miller gives a condensed version of these events in just over five pages: the actions of the early 1780s are not here his prime concern. Instead of a history of high Enlightenment chemistry, he proposes rather a sociologically-informed history of the criteria of discovery in play when controversy was joined in earnest six decades later. Wilson's annotated bibliography of the fight skips straight from 1786 to 1839, the year in which the secretary of the French Academy of Sciences, the fiery republican François Arago, published a Watt eulogy whose 'dogmatic and partisan spirit', so Wilson judged, was 'equally detrimental to the interests of both claimants'. In postworthy epistles (recall that at this period the recipient paid for letters), popular biographies, public lectures at the British Association for the Advancement of Science, and elsewhere, long anonymous reviews in elite periodicals, and painstaking editions of correspondence and notebooks annotated with cunning erudition, Victorian lawyers, journalists and savants set forth the rival claims of the Scottish father of the steam engine and the English doyen of precision physical sciences. Miller does not ask who deserved to win but why they fought.

We are offered fascinating surveys of opinions found in chemical texts, lectures and dictionaries before 1839, and of at least one hundred chemistry textbooks published in Britain between 1840 and 1900. Attributions shifted unevenly but in the end firmly towards Cavendish. Miller has mastered the recondite literature of the controversy, especially those by so-called 'Wattites' who rallied to James Watt's son as standard bearer, such as the Scottish lawyers

Brougham and James Muirhead, and the pro-Cavendish campaigns of William Whewell, Vernon Harcourt and Wilson. He has also skilfully exploited protagonists' extant manuscripts to give remarkable access to the backstage of Victorian public science and spin. In this sense, *Discovering Water* takes an honoured placed alongside other recent microhistories of early Victorian scientific controversy, such as those of Jack Morrell, Martin Rudwick, Adrian Desmond and Jim Secord. Lessons taught by those historians are reaffirmed and amplified here: new and contested boundaries were drawn around the natural sciences and between disciplines; there was a significantly shaky relationship between the steam press, journal editors, scientific societies and gentlemanly specialists; the new workforces of science fought to set up authority and legitimacy; in the meantime north British men of science and Cambridge dons lambasted each other. For Brougham, Whewell was 'a superlative ass'; Wilson reportedly judged the Cambridge master as 'blundering as he is presumptuous'. Whewell's confrère George Peacock riposted that James Watt the younger's campaign was little more than pathological filial piety: 'Lord Brougham seems to have forgotten that much might be pardonable in the fondness of a son which would be highly reprehensible in one exercising the function of a judge'.

Miller's book convinces that almost all the issues which counted in 1840s sciences, especially their relation with industrial arts and experimental training, were at stake here. Wilson's tortuous role in the Controversy is exemplary. As an Edinburgh private chemistry lecturer he relied, so Miller urges, on Cantab patrons who might direct their students his way, so could not offend a fenland coterie battling for Cavendish's genteel purism. His political and scientific patron, Lyon Playfair, got Wilson the crucial directorship of the new Industrial Museum of Scotland, then enlisted Wilson's Cavendish in the ingeniously managed cause of pure science and industrial progress. Yet as a public scientist in the northern Athens, Wilson was also in sympathy with, often wooed by, the Wattites. Some of the finest historical analysis in Miller's book recounts the ultimately abortive attempts by Muirhead and his ilk to seduce Wilson into chemically expert support for Watt. These clubland drinking sessions and frustrating house-calls failed. But their details help Miller explain Wilson's tone adopted in the Cavendish biography that he published under the auspices of the eponymous chemical publishing club, the Cavendish Society, a gang Wilson himself would have preferred named after Robert Boyle. Here were slips between 'radically contextualist' interpretations of late eighteenth-century chemical texts and 'presentist scientific judgements' about whether these texts were right. Wilson set out to show what those chemists '*really meant* by such key terms as phlogiston, inflammable air, and air'. And Miller doesn't like it. Nor do I, much. The modern historian may not judge the chemists of the 1780s, but his philosophical loathing of such presentism and retrospection is unmistakable.

The evaluative component of Miller's program is indispensable to his book's aims, since he proposes to show how retrospective attributions 'create the discovery and discoverer', an approach that draws its strengths from finitist models of meaning and reference, from suggestions about the structure of discovery by T. S. Kuhn and sociological work by Augustine Brannigan. The epistemic stakes are high. Just as a philosopher such as Hilary Putnam, also reflecting on different construals of the word 'water', insisted that meanings are not to be found inside the head, so Miller's arguments tell against any psychological construal of the meanings of 'discovering water'. The tantalising reflection is that Miller feels incapable of judging past chemists, but sternly judges past historians, so requires an equally symmetrical account of the emergence of the archival-based and sensitively interpretative history he practises himself. Miller goes beyond sociological stories of discovery to a thorough historicisation of attribution criteria. There is thus a complementary story here, in which at the very moment when the modern sciences began to emerge in recognisable form, a second Scientific Revolution inaugurating social institutions characteristic of their secular power, including teaching labs, state funding, public shows, newsworthy journalism and industrial payoff, they also spawned expert historical and documentary commentary on their origins, aims, structure, and function. On this showing, the owl of Minerva spread its wings at dawn.

In one rather too brief remark, Miller points out how many comparable fights, involving the same protagonists, raged in the 1840s. This was, precisely, a moment of energetically forensic paperwork in the dusty libraries and storerooms of the sciences, especially in the Edinburgh of Sir Walter Scott and David Brewster, the Trinity College of Peacock and Whewell, and the busy secretaries' rooms of the Royal Society. Controversies then raged about Newton's fights with John Flamsteed, using documents whose existence was first announced at the British Association meeting, just as in the case of Watt vs. Cavendish. And as Arago launched the Water Controversy, so the fiercest polemics about Newton were resuscitated by the French academician Jean-Baptiste Biot's damning judgement of Newton's mental health, made available in English by Brougham's Useful Knowledge Society. As A. N. L. Munby and Richard Yeo have stressed, newfangled scientific biographies and documentary publications flowed in large numbers from the cheap print organs of the 1830s and 1840s. The deeply dodgy antiquarian James Halliwell, one of Peacock's more errant students, managed as a teenager to found the Historical Society of Science in 1839, before ripping off scientific papers from Trinity College library the following year and dumping them for a song on a Soho bookseller. The very same year, Halliwell joined in the Water Controversy by printing Watt's fascinating letter to Priestley of April 1783. The legalistic Muirhead used just such data in his longwinded case for Watt's priority. Wilson also noticed the creeping disease of priority disputes and institutional legalism. His prescription was to adopt prudent finitism about discovery criteria until the psychologists had cracked the discoverers' code: 'When we have learned all the laws to which Genius unconsciously works, we shall be able to bind it in fetters, but for the present we must regard each great discovery as teaching some of these laws, not as supplying a case which is to be decided by them'. Miller points acutely to the range of criteria in play here: Wattites tended to stress the similarity between Cavendish's work and that of their hero, then insisted on Watt's chronological priority, while the enemy were more struck by the contrast between the two men, and celebrated Cavendish's allegedly superior grip on chemical method. Whewell put the point pithily: Watt's hypothesis about material heat was 'by its very terms unsuited to the step which science then had to take'. Gentlemen of science, secure in Cambridge, knew just where science had to go next. Scottish engineers, even the best of them, simply didn't.

Miller's book has much more to offer than its official concern might suggest. 'Who can care ?', exclaimed A. R. Hall towards the end of his exhaustive account of the Calculus dispute. Miller's analysis of a different priority fight rarely prompts similar reflections. Because of its ingenious interweaving of erudite storytelling and sage epistemology, it instead hints at topics for much further work. Of the relation between the historicising of the lives of scientists and the institutionalisation of the new sciences of the 1830s and 1840s we still know far too little. At several points in Miller's analysis, the radical reorientation of chemical training and doctrine in the new teaching labs and classrooms of those years plays an important role in explaining protagonists' investments in the Water Controversy. Miller rightly avers that Cavendish's work was retrospectively judged a discovery not least because his work was judged exemplary of good chemical practice. That's how chemists' labours and chemists' histories began to interact. Such interactions matter, too, to canonical puzzles in our own philosophy of science. Thirty years ago Putnam introduced us to Twin Earth, a puzzling planet identical to our own save that its water's components, though not its water's properties, differ from ours. An anglophone Twin Earther would not mean the same thing as we do by 'water'. An implication of Putnam's anti-psychological argument was to suggest a fascinating division of linguistic labour, between those who 'recognise water' (all of us, seemingly) and those few who can 'distinguish water from liquids which superficially resembled water' (say, chemists). Jerry Fodor sums up: 'chemists do the heavy lifting, philosophers the heavy thinking' (Fodor, 2004). Putnam wanted sociolinguists to investigate this phenomenon of epistemic economy. Miller has now thought through it. The philosopher might be surprised by the historian's results. In 1750, so Putnam claimed, "chemistry was not developed" nor were there "any experts on water", and "it would have taken their scientific communities about 50 years to discover that they understood the term differently" (Putnam, 1975, pp. 223–229). The thought-experiment is absurdly premised but nonetheless telling. Miller finds chemistries and experts aplenty in 1750 just as in 1850, among the Lunar Men, their rivals and successors. Then he shows just how long it took to agree about watery differences, and he even begins to explain why. I wouldn't know quite how to incorporate all this into a new life of Cavendish. But Miller's telling contribution to a properly social account of translation, reference and meaning charts a step that has to be taken.

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Author's Response

By David Philip Miller

I'm much gratified by Jan Golinski and Simon Schaffer's positive evaluations of *Discovering Water* and thank them very much. That this microhistory of a Victorian controversy might be considered worthy to sit, or be read, alongside those written by Morrell, Rudwick, Desmond, Secord (and, I would aspire to add, Oldroyd) is very satisfying. This said, I trust that my reviewers won't think me churlish when I observe that by not giving more explicit attention to how I ought to be buried (as well as praised) these Cambridge men – uncharacteristically, if judged by historical standards – starve me of stuff to chew on in developing my response. I'm reduced to hunting out mild reproaches, blowing them up, perhaps into proportion, and defending myself against the result. However, I'm not complaining. Such treatment is much preferable to that accorded Edmund Burke by François Arago on Watt Jr's recommendation: 'crown your victim with flowers before immolating him', said the younger Watt. I'll settle quite happily for the floral tribute without the subsequent cremation. *Discovering Water* does indeed seek to operate on a number of levels. Though Golinski and Schaffer acknowledge the significances that I made explicit in the project, they also comment upon aspects of the work that broach historical, historiographical and epistemological issues that could perhaps have been taken further.

As both reviewers make clear, my resolve was to focus on the nineteenth-century water controversy and not to spend too much time or effort on describing, explaining, let alone attempting to arbitrate, the eighteenth-century contretemps. In the end, in fact, I expended a lot of effort in producing my seven-page summary of what happened in the 1780s, and also the contextual account that I give of both Watt's and Cavendish's route to water. On the face of it, my studied reticence about the first phase of the water controversy might be another eccentric aspect of the project. Most historians of chemistry are far more interested in tussles over the chemistry of water in the 1780s than they are in battles over the history of the chemistry of water in the 1840s. Since the book is liable to categorisation as history of chemistry my decision might seem a formula for dashed expectations. (Incidentally, I rejected a proposal to put chemical symbols on the cover by way of decoration as perhaps being further misleading in that direction). It is with some relief, then, that I find that Golinski, whose own interests lie mainly in history of science and chemistry prior to the nineteenth century, regards the book as being of value to historians of chemistry concerned with earlier periods. That it might serve in some ways as a cautionary tale about the thicket of interpretation that needs to be cleared away in order to get at the eighteenth-century situation had occurred to me. I now see that perhaps I should have made this clarification of how the 'gulf of misunderstanding' between Watt and Cavendish on the one hand, and their nineteenth-century champions and historians, on the other, was created, an additional explicit aim of the book. The history of British scientific culture in the mid-nineteenth century and the historiography of the chemical revolution are more closely related topics than they might appear.

Focusing on the nineteenth century, Schaffer expresses disappointment – I exaggerate in order to approximate the truth: he actually accuses me of brevity – that I don't delve deeper into the links between the water controversy and other related struggles raging in the 1840s, especially that concerning the reputation of Newton. Alongside the high scientific disputes there was mass distribution of biographical and documentary publications concerning science and scientific discoveries at this time – steam literature for steam reading. We are learning much more about the many layers of discourse about science in Victorian society through the recent work of James Secord (2000) on *Vestiges*, that of Geoffrey Cantor et al. (2004) on popular science during this period, the excellent study of evangelical science publishing by Aileen Fyfe (2004) and a range of other works. Much of this historical work, however, floats relatively free of a concern with the institutional development of the 'new sciences' or at least it has run ahead of our understanding of the institutions of science. When, for example, I tried to pursue my story in the direction of a closer linkage with the development and practices of the chemists, I failed to find recent relevant materials that would make that possible. The thought of pursuing that research into the archives was overwhelming and would have blown the book, already archivally 'heavy', out of proportion. So I made myself content with what I could do. This may be my deficiency (and I've certainly become aware of materials since that time that I wish I'd had earlier, such as Lundgren and Bensaude-Vincent (2000)), but I think that the flight of the field as a whole in recent decades (myself included) from avowedly institutional studies has something to do with my plight.

I think, incidentally, that this is an important general issue in the history of science. There are some signs that new efforts are being made to explore the interactions between the practices of science, or natural philosophy in earlier times, and modes of institutional practice (which is related to cultural practices but not exhausted by them). As that enterprise picks up, and scholars begin to revisit classic institutional sites of nineteenth-century science with new questions, we may also be able to better meet Schaffer's desideratum of a greater understanding of 'the relation between the historicizing of the lives of scientists and the institutionalisation of the new sciences of the 1830s and the 1840s'.

It would have been possible for me to pursue the lives and reputations of Watt and Cavendish deeper into the steam press and, indeed, into the second and third tiers of technical periodicals. This would be a productive thing to do but I didn't do it for a variety of reasons. One was quite practical – living 12,000 miles away from relevant sources, especially when they're fugitive, 'dilute' and unindexed, imposes some limitations. However, I also believed that so far as the 'attributional economy' of discovery was concerned the major reviews and the large numbers of chemistry textbooks that I consulted at the National Library of Scotland were more important. The pursuit of the lives of iconic scientists and engineers into other bodies of literature and other communities is worthwhile. I know, for example, that Christine MacLeod is doing work on Watt as a symbol for engineering unions. It would be interesting to know what, if anything, they thought of Watt and water. In my research I stumbled across interesting reactions to Cavendish in the evangelical periodical *Leisure Hour* that, while accepting his scientific eminence, lamented his lack of faith and painted an ambivalent picture of Cavendish as an extreme manifestation of, in George Levine's words, a human being 'dying to know' (Levine, 2002). More of this sort of work will help us to understand better the wider cultural framework in which the new sciences were institutionalised and the relationship of that process to specific historical contests such as the 'water question'.

Another implied criticism has to do with the contradictions involved in my rejection of presentism, both in the account of the water controversy and in my grumbles elsewhere about popular manifestations of it in the 'Sobel Effect' literature. This is indeed a real, and genuinely puzzling, issue for me. There is something contradictory in refusing, as I do, to carry contemporary scientific judgement into my analysis of the water controversy whilst at the same time beating nineteenth- and twentieth-century historians over the head with my current historiographical beliefs. I'm not entirely innocent of recent discussions of Whiggery, presentism and anachronism by Nicholas Jardine (2000), Nick Tosh (2003) and others. At one stage, my plan for the book had a final chapter in which I was to enter those discussions and test my history against them. I decided in the end not to include this precisely because I couldn't see how the subtleties of those arguments could be reflected in my historical practice, or perhaps it was *vice versa*. Of course, it is true that, in the final analysis, presentism is unavoidable. Any interest that the water controversy might now have is judged from a present perspective. Effective communication requires presentism if long-windedness is to be avoided. (The title of this Review Symposium – "The Story of H_2O?" – is a case in point. The question mark signals a cautious compromise between communicating effectively and being self-conscious about anachronism. The title would have been a little more accurate as "The Story of HO" given that none of the protagonists in *Discovering Water* represented water as we do. However, what was gained in accuracy would almost certainly have involved losses elsewhere, including perhaps receipt by the editor of too many complaints about sub-standard proof-reading!) It is also true that much of the huffing and puffing about Whiggery that goes on could specify its complaints more accurately. My problems with George Wilson's historical practice have mainly to do with anachronism, for example. In the end, avoiding the asymmetries introduced into historical explanation by scientific presentism has to sit, I believe, with a more relaxed attitude towards presentism in dealing with prior histories. Purism about reflexivity in these matters leads in my view to abandonment of historical explanation. There be demons, like actor–network theory.

Many of the issues at stake here are similar to those contended by analysts of contemporary scientific and technological controversies. I'm reminded of the exchange some years ago between Pam Scott et al. (1990), in one corner, and Harry Collins (1991) in the other, about the rights and wrongs of assuming the stance of the neutral analyst or embracing that of the confessed partisan participant when engaging with contemporary controversies. Collins was accused of inconsistency in relativising natural science but refusing to do the same for his own social science, for not abandoning the stance of the neutral expert. Scott, Richards and Martin also suggested that, given the tendency for the studiedly symmetrical analyst to be co-opted by one side or the other, it would be better to declare allegiance upfront. It strikes me as quite likely that my would-be symmetry of approach in *Discovering Water* will indeed be used by others to promote the historical 'cause' of James Watt against that of Cavendish. Treating in a reasonably symmetrical way what has before been seen as an uneven contest, or even a non-contest, is surely to favour the fortunes of the historical underdog. Nevertheless, I still hope that my account will advance understanding of how the controversy was conducted and what propelled, sustained and closed it, in a way that accounts preoccupied with finding the 'real discoverer' have failed to do.

Given my unwillingness to follow through the conundrums involved with presentism, except in a practical historical sense, it will not surprise the reader that I reserve judgement on whether I have, as Schaffer suggests, 'thought through' Putnam's famous Twin Earth example with its water$_1$ (H_2O) and its water$_2$ (XYZ). My use of the attributional model of discovery, based in finitism, and the story that I tell, does challenge some of the more cavalier historical statements that Putnam makes about chemistry in developing the Twin Earth argument. I won't pursue the 'meaning of 'meaning'' further than this, except to observe that I'm confident that my philosophical colleagues will find more of interest in the book than those who may have purchased it inadvertently, via the 'one click' facility on Amazon.com, believing it to be a primer on the art of dowsing!

Finally, we come to Schaffer's musing on what difference if any my history might make to his writing of the *Oxford DNB* entry on Cavendish. Were it possible, should that entry be revised in the light of all this? I think that perhaps it should. It may be true that Watt ceded the water discovery to Cavendish. However, if the thrust of *Discovering Water* is right, then the full meaning of that aspect of Cavendish's reputation was in significant part a product of the nineteenth century. For this reason, the water discovery was not only beyond the eccentric natural philosopher's grasp but also outside the 'Great Steamer's' gift.

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SURVEY REVIEW

MADNESS AND THE MONRO DYNASTY

Jonathan Andrews and Andrew Scull, *Undertaker of the Mind: John Monro and Mad-Doctoring in Eighteenth-Century England.* Berkeley: University of California Press, 2002. Pp xxii + 364. US$35.00 HB.
Jonathan Andrews and Andrew Scull, *Customers and Patrons of the Mad-Trade: The Management of Lunacy in Eighteenth-Century London*. Berkeley: University of California Press, 2003. Pp xvi + 209. US$44.95 HB.

By Jonathan Simon

John Monro is best known to historians of psychiatry as the conservative opponent of the progressive approach to understanding madness championed by William Battie in his *A Treatise on Madness* of 1758. In *Undertaker of the Mind*, Jonathan Andrews and Andrew Scull attempt to rehabilitate the image of this successful eighteenth-century mad doctor, one of three members of the same family that reigned over London's notorious Bethlem Hospital for over a century. Thus, John Monro (1715–1791) was the son of another visiting physician to Bethlem hospital, James Monro (the original target of Battie's criticisms), the name that his son John gave to one of his own sons who was destined to inherit the professional title as well. A Scottish Jacobite family, the Monros encountered a number of difficulties in achieving their prestigious standing in medical society, but, as Andrews and Scull ably show, profited fully from it, through their private practice.

In Roy Porter's *Mind Forg'd Manacles* (Penguin, 1987), which was doubtless a major inspiration for the pair of books under review (*Customers and Patrons of the Mad-Trade* is dedicated to him) we find the claim that: "Considering that the Monros ran Bethlem for 128 years, we know peculiarly little of their philosophy of insanity" (p. 128). Andrews and Scull have effectively responded to this chal-

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lenge by exploring the treatment philosophy and the therapeutic practices (dominated by bleeding) that were employed by Monro at Bethlem and elsewhere. Indeed, *Undertaker of the Mind* is far from a classic biography, as the authors have used a wide range of sources to meticulously reconstruct a number of the cases in which Monro was involved. Rather than just recounting the life of one notable mad doctor, the book aims to illuminate all aspects of madness as a medical condition and social phenomenon in eighteenth-century Britain. Thus, it includes, for example, a discussion of the ostentatious but often ill-adapted architecture of eighteenth-century charitable institutions for the insane, including Bethlem – the most famous of London's madhouses and seat of the Monros – built in 1675/6, as well as St. Lukes, a competing institution constructed using subscriptions in 1751, from which Battie was able to challenge the Monros.

Andrews and Scull also try to flesh out the elusive economics of private practice in the field, a notable feature being the large number of private institutions that sprang up in eighteenth-century London. Monro's own such venture, Brooke House in Hackney, clearly contributed greatly to the fortune that he was able to pass on to his son. Thus, one virtue of *Undertaker of the Mind* is that it includes a detailed exploration of a business that was often conducted in confidentiality, particularly when it concerned paying customers and family scandal. From clues in casebooks, court proceedings and government inquiries, the authors have attempted to reconstitute the importance and functioning of the numerous private institutions that offered temporary or more permanent care of the insane for a paying clientele. Through various public scandals that sometimes found issue in the law courts, they also evoke the ever-present threat of false imprisonment; a fantasy (and sometimes a reality) perpetually associated with such institutions, whether charitable asylums or private, for-profit enterprises.

Of course, the greatest challenge for any such history is to recover the experience of the patients themselves, rather than simply relying on institutional and professional pronouncements. This is particularly difficult for those stricken with madness, as their testimonies are always suspect. Andrews and Scull choose to relate a number of celebrated cases, such as that of Lord Ferrers (who brutally murdered Johnson, his steward), and the attempted regicide, Margaret Nicholson, who spent the rest of her days in Bethlem after lunging at George III with a dessert knife. These cases give an idea of the criminally insane as pathetic actors on an increasingly public stage of judicial inquisition and punishment. The authors also explore the testimony of the eloquent and effusive madman Alexander Cruden (Alexander the Corrector) who was not afraid to attack James Monro Senior's competence in print. Cruden's trajectory, with long-term stays in a variety of institutions, makes it all the more difficult to interpret his attacks on professional and personal acquaintances. Cruden is also used to illustrate the links that had become established between religious non-conformism and madness. This is a theme that Andrews and Scull are keen to explore, as it supports the implication of the medical profession in Anglican attacks on religious excess so closely associated with revolutionary tendencies. More broadly, the authors' goal is to trace the marks of new bourgeois patterns of consumption in the development of the eighteenth-century 'mad-trade', and the involvement of doctors specialising in nervous ailments in an increasingly difficult and strict policing of urban promiscuity.

The other book under review, *Customers and Patrons of the Mad-Trade*, is a complement to *Undertaker of the Mind*, in which Andrews and Scull have taken the opportunity to present a transcription of John Monro's private casebook from 1766, along with extensive explanatory notes and a 112-page introduction (that to a large degree uses material already presented in *Undertaker of the Mind*). This casebook is as remarkable a historical source as it is apparently rare, and it makes fascinating reading. It records a wide range of afflictions in brief notes that are quite evocative of Monro's daily contact with mental distress in and around London: "Mrs Harris from Hendon ... in a low melancholy way, troubled with bad & blasphemous thoughts which kept her from sleeping & made her very uneasy & unhappy" (C-46). Despite being relatively easy to read, these notes are often hard to interpret and it is difficult to avoid the temptation of seeing them as an expression of a universal human condition. They suggest stories of individual distress and domestic intrigue worthy of any soap opera, but the numerous reference to dreams as omens and religious overtones mark them out as distinctly pre-modern. The considerable research that has gone into the numerous explanatory notes for the cases related in this notebook means that they provide an indispensable context for the appreciation of the stories behind what are often only very brief notes. Indeed, both these books by Andrews and Scull are impressive in terms of the amount of detailed research that has gone into them, and, despite this attention to detail, and their joint authorship, they are both elegantly written.

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ESSAY REVIEW

CONCEPTUAL SPACES AND CREATIVE MINDS

Margaret A. Boden, *The Creative Mind: Myths and Mechanisms*. London and New York: Routledge, 2004. Pp. xiii + 344. £13.99 PB, £45.00 HB.

By Terry Dartnall

This is the second edition of *The Creative Mind* (hereafter TCM), which was originally published by Weidenfeld and Nicolson in 1990. This edition has a new 10-page introduction that outlines Boden's theory of creativity and a 17-page epilogue that describes some recent computational models of creativity. The main text remains the same except for some minor clarificatory changes and the inclusion, in Chapter 7, of some material on Douglas Hofstadter's COPYCAT program.

The programs described in the epilogue include JAPE, which generates low-key puns such as "What do you call a depressed train? A low-comotive", and "What do you call a strange market? A bizarre bazaar" (p. 305). Boden tells us about 'Letter Spirit', which is the latest and most ambitious project to come out of the Hofstadter/FARG stable at Indiana University, Bloomington. 'Letter Spirit' designs new fonts in the lower-case Roman alphabet. Boden says that it is a connectionist system. Hofstadter stresses that it is not a connectionist system. His complaint about connectionist systems is that once you have trained them up the processing whooshes through all in one go, without any of the compromise and give-and-take that characterises human creativity. It is true that 'Letter Spirit' has a low-level, stochastic architecture, like a connectionist system, but unlike a connectionist system it is highly modular. The decision-making is kicked around between its modules and the creative product emerges out of this. (Ideally, it emerges out of a resolution of tension between conflicting modules.)

Boden also tells us about David Cope's program 'Emmy', which generates music by (so to speak) decomposing, labelling and reassembling musical compositions. 'Emmy' can do this with composers

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as diverse as Bach, Mahler, Prokofiev, and Joplin, and she can combine styles as diverse as Baroque and Balinese. I did hear that she was at work on a Mahler opera (Mahler didn't write an opera, of course). Boden says that the music on Cope's CDs is played by human musicians. In fact the music on one of them (*Bach by Design*) is played by a computer, and it is predictably lifeless and dull. Boden also tells us about IMPROVISOR, which is a jazz program that can improvise in real time, the story-writing programs MINSTREL and BRUTUS, and genetic-algorithm programs that can generate infinitely many coloured images.

Routledge, the publishers of this edition, asked me to report on the viability of a second edition of TCM. I said that a new edition should include a chapter in which Boden responded to her critics. She has not done this, and I think it is a lost opportunity. She did the spadework in replying to a Peer Review of TCM in *Behavioral and Brain Sciences* (17:3, 1994) and in her Reply to Reviewers in *Artificial Intelligence* (79, 1995). In the preface to the new edition she says, "Because *The Creative Mind* was written for a general audience, I haven't detailed the many comments I've received since it first appeared" (p. ix). Well, that is a pity. The book would have been a lot more interesting if it had included such material, and by replying to her critics she might have shed light on some of the murkier aspects of her theory – especially what she means by a 'conceptual space'.

In what follows I shall outline Boden's theory of creativity as it appears in the new introduction. I shall then look for a more detailed account in the body of the book. Then I shall revisit my criticism of it in *Behavioral and Brain Sciences*, 17:3 and I shall supplement this criticism with material from Dartnall, ed., *Creativity, Cognition and Knowledge: An Interaction* (Praeger, Westport, 2002).

In the new introduction, Boden says that she distinguishes between combinatorial, exploratory and transformational creativity. Combinatorial creativity involves making "unfamiliar combinations of familiar ideas" (p. 3). Examples include "poetic imagery, collage in painting or textile art, and analogies." (*ibid.*) Exploratory creativity is where we explore a conceptual space, and transformational creativity occurs when we transform a conceptual space. She believes that computational concepts can help us to understand all three types of creativity.

The question is: what does Boden mean by a 'conceptual space'? In the new introduction she says it is a "structured style of thought" (p. 4), so that "someone who comes up with a new idea within that thinking style is being creative in the second, exploratory, sense" (*ibid.*). Transformational creativity involves someone thinking something which, "with respect to the conceptual spaces in their minds, they couldn't have thought before" (p. 6). They have to *transform* the structure of the space by changing the rules that govern it.

That still leaves us pretty much in the dark about what conceptual spaces are, so we have to look in the body of the book for the details. Even then it is quite difficult to pin the notion down. Part of the problem is that, rather than coming out and straightforwardly saying what a conceptual space is, Boden wants the nature of conceptual spaces to be revealed with respect to the puzzle alluded to above: how can we account for the fact that some sorts of creativity are *impossible* with respect to a particular framework of thought? Her answer is that a conceptual space is a space of structures generated by the rules of a generative system, so that some structures cannot be generated within the confines of a particular system. They "cannot be thought" within that system. In order for us to come to terms with them we have to transform the system so that it can generate the structures (or use another generative system, of course). Boden's favourite example is the way in which, by "passing from 'string' to 'ring', Kekulé created the possibility of a whole new science: aromatic chemistry (the study of the benzene derivates)" (p. 71). She says that the conceptual space of contemporary chemistry deemed ring-structures impossible, so that they could not have been thought within that framework (p. 63). I do not know enough about the history of chemistry to be able to say whether this is a good example. I suppose that another example is that within the confines of Newtonian physics it is unthinkable that light waves should bend: it is *logically* impossible for Newtonian physics to be true and for light waves to bend. Similarly, within the framework of Euclidean geometry it is *logically* impossible for the sum of the angles of a triangle to be other than 180 degrees.

For Boden, then, a conceptual space is defined by a generative system. "To justify calling an idea creative [in the interesting, transformational sense]… one must identify the generative principles with respect to which it is impossible" (p. 51). She goes on to say, "Computational concepts help us to specify generative principles clearly . . . computer modelling helps us to see what a set of generative principles *can* and *cannot* do" (p. 52).

Now to my criticism. A generative system generates a (usually non-finite) body of structures out of a finite set of elements through the application of a finite body of rules. A good example is a generative grammar. A generative grammar starts with a single, atomic symbol and generates strings of atomic symbols by rewriting one symbol at a time through the sequential application of rewrite rules. When the string contains only terminal symbols the symbols are replaced by words from a lexicon, giving us an interpreted string (usually a sentence). Boden uses generative grammars to illustrate what she means by a generative system (p. 49–52) and generative grammars feature throughout the book. She talks about English grammars (p. 48ff, p. 94, pp. 184–186), grammars of musical harmony (p. 101) and "shape grammars" that generate a space of buildings that adhere to a particular style – Palladian or Frank Lloyd Wright's Prairie Houses, for example (p. 309ff).

I want to know how this notion of a generative system can shed light on creative products such as paintings, poems and sculpture. Generative systems are atomistic. They consist of a set of atomic elements. It seems to me that some creative products cannot be usefully described as atomistic, and some are not atomistic at all. Let us take these in turn.

In my *Behavioral and Brain Sciences* Peer Review of TCM I say, "There is no significant sense in which Verdi's *Requiem*, or *Hamlet*, are combinations of previously existing elements or ideas." (p. 537). Although Verdi's *Requiem* consists of notes and *Hamlet* consists of words, so that they are structures in note-space and word-space, respectively, nothing is gained by such a characterisation. It does not tell us why they are creative, why they are original, why they can move us, and why they shed light on the human condition. In her response Boden says: "I do not understand why Dartnall thinks I believe Verdi's *Requiem* and *Hamlet* to be combinations of previously existing elements. They do involve combinatorial thinking, to generate the imagery and the literary or musical allusions. But, as I said repeatedly, music and plots are complex structures requiring exploratory-transformational creativity" (p. 559).

Well, *Hamlet* does belong to the conceptual space defined by English grammar and Verdi's *Requiem* does belong to the conceptual space defined by a permissible combination of musical notes. We agree, however, that this is not a helpful or informative thing to say. But if we are not dealing with these conceptual spaces, what conceptual spaces are we dealing with? What are the atomic elements of the spaces and what are the rules? What atomic elements are music and plots constructed out of and what are the rules of combination?

Now it is possible that Boden does not want an atomistic concept of a generative system (and hence of a conceptual space). In that case she should say so and try to provide a non-atomistic account – although it is not clear to me what this would look like. Chapter 5 is called "Concepts of Computation", so we would expect to find such an account here, if anywhere. We do not find it. In this chapter Boden talks about knowledge representation formalisms, such as semantic networks, frames, and scripts. Such formalism are notoriously "scruffy" (technical term!). They are put together in a rough-and-ready way to capture aspects of the way in which we seem to think (how we associate concepts, and the way in which we apparently 'chunk' knowledge together). They are not formalised, so they do not generate structures by applying rules to atomic elements. Recently, knowledge representation has moved away from scruffy formalisms to the 'neat' (technical term!) formalisms of logic. Logic, of course, is in keeping with the standard concept of a generative system, but now we are back to atomic elements again.

So far I have suggested that it is not helpful or informative to regard music and literature as structures in word-space or note-space. In *Creativity, Cognition and Knowledge. I argue that some creative products are not atomistic at all. When a sculptor chisels a form out of a block of marble or a potter shapes a pot out of a lump of clay they are not generating a product out of a set of elements. When we draw a face we do not assemble a set of elements, as we might put together a face out of an identikit of eyes, ears and noses. The features of the face emerge as we draw the face. The features are properties* of the face that do not exist independently of the face. There is no curve of the cheekbone that exists apart from the cheekbone: the curve is a property of the cheekbone, not a component or entity that we can store in an inner identikit. This is the point of Lewis Carroll's joke about the grin of the Cheshire Cat, which remains when everything else has faded away. The grin is a property of the cat that does not exist independently of the cat. I argue in the book that we generate the drawing of the face out of our *knowledge about faces*, and I suggest that this ability to generate products out of knowledge is an important epistemological issue that has been largely ignored. (If you think that we do draw faces by putting together eyes, ears and noses, like an identikit, ask yourself how you draw an eye. You construct it out of lines and curves, which are not stored entities. What would it mean to say that we store "the curve of an eye"?!)

So what have we got?

P1. Exploratory and transformational creativity involve the exploration and transformation of conceptual spaces.
P2. Conceptual spaces are generated by generative systems.
P3. Generative systems combine basic elements according to rules.
But P4. At least some creative products do not combine basic elements according to rules.

If P4 is true, either P1, P2 or P3 is false (possibly more than one of them, of course). P2 strikes me as a harmless definition of 'conceptual space', so it can probably stay. That puts the pressure on P1 and P3. Boden cannot abandon P1 without abandoning her theory, so she has to modify P3 (rather like a Lakatosian auxiliary hypothesis, which cops the arrow of *modus tollens* in order to protect the core of the theory). I have briefly looked at P3. It is not clear to me what a modified, non-atomistic account of a generative system would look like.

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LINKING *QIGONG* WITH PSYCHIATRY: AN ANTHROPOLOGICAL DIALOGUE ABOUT NORMALITIES

Nancy N. Chen, *Breathing Spaces: Qigong, Psychiatry, and Healing in China*. New York: Columbia University Press, 2003. Pp. xvi + 238. US$26.00 PB.

By Everett Zhang

The two words *qi* (vital energy or vapour) and *gong* (work and effect) came together to form the phrase '*qigong*' first in the Jin Period (265–420 CE). But the phrase did not refer to a systematic set of bodily techniques until, arguably, modern *qigong* was born in the 1950s, shortly after the founding of the People's Republic in 1949. In philosophy as well as in practice, modern *qigong* has been influenced by various traditions of Chinese religion and medicine. It enjoyed a dramatic rise in popularity at the end of the Cultural Revolution in the 1970s, and the enthusiasm expanded throughout the 1980s and 90s. This '*qigong* fever' was dampened only when the Chinese Government decided to crack down on the Falun Gong, one of the most powerful *qigong* movements, in the late 1990s.

Nancy Chen's *Breathing Spaces* is the most recent addition to the small body of ethnographic literature on *qigong* produced in Western academia. An earlier attempt is Elisabeth Hsu's meticulous ethnography, *The Transmission of Chinese Medicine* (1999). Hsu's book deals mainly with the intensified and somewhat secret process of curing specific illnesses in a *qigong* clinic. It considers the healing practice of transferring *qi* from the healer to the patient by conceptualising the flows of *qi* between the healer and the patient and between human and his/her surroundings into what the author calls 'body ecologic'. Chen's study, in contrast, focuses on *qigong* practice in public parks and on *qigong* as a routine of health cultivation. This focus enables her to investigate the public spatiality of the '*qigong*

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fever' – hence the title 'Breathing Spaces'. Hsu did her fieldwork in the city of Kunming, southwestern China, in the mid and late 1980s, whereas Chen did hers in Beijing and several coastal cities in the early 1990s.

For the specialists, Chen's study is an intervention in the debate on whether *qigong* deviation should be considered only as psychiatric disorder. *Qigong* deviation refers to a set of malignant reactions resulting from over-practise or misguided practise of *qigong*. Based on observations and interviews in psychiatric wards specialising in treating inpatients of *qigong* deviation, Chen investigates the significance of managing *qigong* deviation as a psychiatric disorder in broad medical and cultural contexts. In the global regime of psychiatry, the "overlapping and sometimes indistinguishable characteristics of *qigong* deviation and schizophrenia – chiefly hallucinations and delusions" (p. 100) leads to the subordination of the category of *qigong* deviation to that of a psychiatric disorder. Chen insists on taking local meanings of *qigong* deviation seriously and argues that despite the similarities between *qigong* deviation and certain mental illnesses in psychiatry, they represent two different sets of disorders. To bolster this view, Chen surveys multiple meanings of *qigong* possession, which is a state of trance induced by *qigong* techniques. *Qigong* possession shares with many healing rituals documented in anthropological studies the potent experience of relocating and transforming the body as well as the self. It may function as an expression and articulation of marginality, and thus has social and political implications. However, a person in trance may also slip out of control and end up developing symptoms of *qigong* deviation. These multiple meanings cannot be reduced to simple psychiatric illness.

Given both the potential and danger of *qigong* practice, it is important to understand what the norms are, so that a cultural space of healing practice for *qigong* can be justified. The task of defining '*qigong* norms' is not easy, however. Publications in Chinese on *qigong* are usually based on *qigong* masters' own narratives and/or the guided testimonies of practitioners; they are hardly reliable. It seems to me that a necessary step is to record the gamut of experiences of practitioners in many walks of life. Chen's ethnographic work points in the right direction. The book includes vivid sketches of ordinary people's experiences in searching for 'breathing spaces' in public parks. Chen also describes her own brief but adventurous experience of receiving *qi*. These are commendable attempts to document what is actually going on with the body when practicing *qigong*. Such investigations also encourage careful consideration of the practitioners' life worlds and life histories, which cannot be separated from *qigong* experiences.

Because of its close attention to the comparison between *qigong* deviation and psychiatric disorder, Chen's ethnography examines mainly the negative effects of the mysterious power of *qigong*. I suggest that with its clinical success in managing *qigong* deviation, psychiatry may also be used to uncover the positive potential of healing and health cultivation of *qigong*. The point of entry for this investigation might be a comparative analysis of the two different sets of norms – the psychiatric and the *qigong* – for a healthy person. To achieve the *qigong* norms demands tremendous bodily as well as mental discipline. Similarly, many personal and institutional disciplines are required in order to maintain the psychological norms of reason and rationality. If so, then 'practising' reason and rationality always contains the possibility of deviation, just as there is always the risk of going astray in *qigong* practice. This kind of comparison may lead us to better understand not only deviations, but also norms in psychiatry and in *qigong*. Chen's pioneering work invites us to venture into this uncharted territory. The ultimate goal is perhaps to uncover the mysterious power of *qigong* on the one hand and the efficacy of psychiatry, which is often no less mysterious, on the other.

Any productive dialogue between psychiatry and *qigong* must consider body politics beyond the individual body and personal experience. Chen's research highlights two social and political forces that have influenced the status of *qigong* as a popular health practice and the relationship between *qigong* and psychiatry. One force is what she calls the 'mandate of science', which is connected to the modernisation project of the state. Although the Chinese state promoted research on *qigong* as a 'Chinese science', the *qigong* fever has always attracted much criticism and suspicion from the scientific community. Both attackers and defenders of *qigong* mobilised scientific discourse; the defenders, for example, have incorporated biology and even information science into their discourse. The momentum of the battle has been swinging one way or the other over the past 20 years.

The other force is the state. Chen discusses 'breathing spaces' in relation to the state, with particular focus on the case of the Falun Gong. The rise of the Falun Gong and the Chinese Government's crack-down on the movement can be seen as two faces of a complex history of the relationship between the socialist state and *qigong*. The state's support was an important condition for the reinventing of *qigong* in the 1950s and the *qigong* fever in the 1980s. However, when a *qigong* group gathered a large following and formed a collective social force, the socialist state began to fear that its authority was being threatened. The state claimed itself as a legitimate representation and institution of the 'mandate of science'. Its clash with the mass movement under the 'mandate of *qi*' led to the crackdown on the Falun Gong. Scholars have characterised this event as the struggle for civil society or a millennialism that fed on the discontents of the marginalised in a consumer society. Chen leans toward the latter view. It is indeed hard to deny that the movement has gathered force from the widespread resentment toward rampant corruption and increasing social inequality. The movement also expresses a shared desire to cultivate the body in a logic different from that of consumerism. Any effort to explain the Falun Gong movement must take into account the impulse of the Chinese state to maintain its sovereign power over the unorthodox, the oppositional, and any collective use of bodies by asserting its conviction in 'science' – a conviction that can lead to violence when not grounded in democracy.

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ANALYSIS OF AN ASYLUM

Steven Cherry, *Mental Health Care in Modern England. The Norfolk Asylum/St Andrews Hospital c. 1810–1998*. Woodbridge: The Boydell Press, 2003. Pp. xii + 335. US$75.00 £45.00 HB

By Joseph Melling

In the past three decades the lunatic asylum has become one of the most studied institutions in history, with detailed accounts of individual establishments regularly published. As one of the first public facilities for the mad funded by the ratepayers of an English county, the Norfolk Asylum opened its doors before the Battle of Waterloo was fought. The magistrates of most English and Welsh counties did not follow suit until the famous Lunacy Acts of 1845 were passed. Cherry offers a careful scholarly assessment of the evolution of this service from its origins in the early 19th century to its closure at the end of the twentieth. The more original and interesting chapters are those dealing with the development of 'modern' treatment after the formation of the National Health Service in 1948 and the steady transition towards community care, which entailed the contraction of a institution which had expanded relentlessly in earlier decades. The study documents the actual growth of annual admissions in the years immediately following the 1959 Mental Health Act as shorter periods of residence and more effective psychotropic medication became available. This explains the paradox that admissions remained high and often increased after 1965 but the resident population decreased along with available beds in such hospitals. Students of psychiatry as well as social historians of madness will find a mass of interesting material, including the interviews which the author has conducted with former staff and patients at St Andrew's Hospital.

The purpose of these interviews are indicated in the opening pages where Cherry explains his aims as uncovering where possible 'the

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experience of its patients and staff' as well as locating the growth of this institution within what he (following Andrew Scull) terms 'global histories' of insanity and the provision made for containing it. The interpretation offered for the growth of the asylum is one in which a plurality of forces and influences are at work, ranging from powerful elites to popular concern and from Poor Law authorities and estranged families to medical experts and psychiatric social workers. In its careful detail and cautious conclusions the text leans more towards Kathleen Jones's crisp empiricism than the challenging claims of Michel Foucault and Andrew Scull, though there are few points at which the author directly challenges 'global' narratives. One rare moment of disagreement with earlier accounts is evident in the conclusions where the author suggests that this reviewer has exaggerated the role of families and friends in the admission and discharge of patients to the asylum, though this seems to arise from a misunderstanding of the argument rather than a fundamental difference of interpretation.

The most difficult task facing any historian of such an institution is to recompose the 'experience' of those who lived and worked inside its walls. This is a commendable but possibly a utopian ambition for historians of insanity. For the great bulk of the records that survive from the eighteenth, nineteenth and early twentieth centuries are primarily the materials produced by medical personnel, legal and clerical authorities, and occasional diaries and personal testaments that individual sufferers felt compelled or enabled to write about their experience of the mad house. The practical barriers to detailed reconstruction are apparent in Cherry's acknowledgement of the limitations of the records available for military personnel who were suffering from shell-shock during First World War. Even when apparently abundant statistics are available, the asylum historian is often compelled to present miniature cameos of individuals as a method for illustrating a particular condition or trend within the patient population. Such sources needs to be deconstructed not only in terms of the situation of the authors and the constraints within which they composed the various texts that provide the raw material for historians, but also in terms of the cultures of class, gender and geography that shaped the expectations of those speaking. Researchers into private as well as public asylums in the Victorian and Edwardian decades may also recognise the distinctive genres of expertise and literary composition that coloured the letters and evidence of people who viewed the asylum in the shadow of the sensational novels of the Brontes, Wilkie Collins, or Henry James at different moments when issues such as false imprisonment, married women's property rights, or syphilitic infection were being debated in relation to the incidence of insanity. In such contexts the 'experience' of the resident of an asylum or even of the long-term patient of modern mental hospitals will necessarily be interpreted through a series of cultural and intellectual filters by the reader as well as the historian presenting such evidence on the page.

This study succeeds admirably in providing us with a careful and considered narrative of the development of an important county asylum from the late Regency years until our own time. There are effective and well-organised explanations of the ways in which this establishment coped with the changing legislative framework that governed the care of the insane, as well as tangible details of the impact of the new medication available from the 1950s. It remains an open question whether individual asylum histories framed in this way can seriously challenge 'global' histories unless the latter are themselves interrogated in a sustained and consistent fashion, which would require a rather different book. We are given many valuable glimpses of the recorded experiences of individual patients and the staff who cared for them although the more demanding reader will expect some guidance as to how our own perspective on the long history of institutional care may be located in the changing models and genres of writing on madness. For it is only too apparent that the facilities of community care have failed to resolve many of the more intractable problems of attitudes towards mental illness which Victorian psychiatrists documented in the mid-nineteenth century.

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SENSIBLE STRUCTURALISM?

Charles S. Chihara, *A Structural Account of Mathematics*. Oxford: Oxford University Press, 2004. Pp. xiv + 380. US$70.00 HB.

By Stewart Shapiro

In contemporary philosophy of mathematics, *nominalism* is the view that abstract objects, like numbers, sets, points, and functions do not exist. For more than 30 years, Charles Chihara has been a steady advocate of nominalism. Although the present work is substantially self-contained, it is a sequel to his earlier *Constructibility and Mathematical Existence* (Oxford University Press, 1990). That book proposes to imitate the structure of simple type theory in semantics: sentences with one free variable play the role of classes, and the relation of satisfaction plays the role of membership. The theory has variables that range over physical sentence *tokens* – hunks of ink, sound waves, acts of waving flags, and the like. The language has a sort of variable that ranges over open-sentence tokens that can be satisfied by ordinary objects, a sort of variable that ranges over open-sentence tokens that can be satisfied by open-sentence tokens that themselves can be satisfied by ordinary objects, and so on up the hierarchy. Chihara invokes a 'constructibility quantifier'. If x is a variable and P a predicate, then a formula CxPx is read 'it is possible to construct an x such that Px'. The development or reconstruction of mathematics then follows that of simple type theory. The role of cardinal numbers is played by certain second-level open sentences: the number n corresponds to any open sentence that is satisfied by an open sentence S if and only if S is satisfied by exactly n ground level objects.

The essential primitives of Chihara's system, then, are the constructibility quantifier and the satisfaction relation. In his books, Chihara says a lot about the former, by way of philosophical motivation, a formal semantics via possible worlds, and inference rules.

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We are given less on the satisfaction relation. There are at least *prima facie* issues concerning interpreting satisfaction as a relation involving tokens. The same token can have different meanings if it is used in different languages, or even in the same language, due to ambiguity. I presume that a nominalist does not want to introduce meanings, as abstract objects, or even as equivalence classes of tokens.

The present book covers a lot of ground, and all of it is interesting. It contains replies to published criticisms of the earlier book, arguing that in each and every case, the critics either brought shallow, unconvincing arguments or misrepresented Chihara's view. In addition, much of the book is devoted to extensive and sharp criticisms of just about every major philosophy of mathematics in the literature, including the work of writers, such as Geoffrey Hellman and Hartry Field, who propose competing nominalistic accounts of mathematics. The realist structuralist views of Michael Resnik and myself come in for especially heavy commentary, sometimes invoking contentious positions from general metaphysics. Some of the criticisms of my own position are thoughtful and fair. The community of realists is indebted to the present book for showing places where further articulation, modification, and nuance are needed.

What is the relationship between a nominalistic reconstruction of mathematics and either pure mathematics itself or the mathematics applied in science? John Burgess and Gideon Rosen (*A Subject with no Object*, Oxford University Press, 1997, §§IA, IIIC) propose a dilemma for an advocate of any reconstructive program. Does the nominalist propose that we should all use the reconstruction instead of standard mathematics? This is a *revolutionary* claim. Or does the nominalist claim instead that the reconstruction gives the *meaning* of actual mathematics, or at least the mathematics used in science? This *hermeneutic* nominalist thus makes an empirical claim about mathematical and scientific sentences. Neither option is palatable. But then what does the nominalist have in mind, concerning his or her reconstruction of mathematics?

As far as I know, Chihara is the only nominalist to respond to the dilemma in detail. He claims that the purpose of his nominalistic reconstruction is to undermine the so-called indispensability argument in favour of the existence of mathematical objects. W.V.O. Quine and Hilary Putnam famously point out that the scientist *uses* mathematics in developing her account of the natural world, and this mathematics has variables that range over abstract objects. They then claim that it is dishonest to disavow in philosophy what one accepts in one's best account of the world. A hermeneutic nominalist can give a straightforward response to the indispensability argument. She holds that mathematics is *true*, and thus usable in science, without entailing the existence of mathematical objects. But Chihara rejects the hermeneutic approach. We are told, over and over, that he has no detailed views about what mathematical assertions actually mean. His only claim is that if mathematical statements are understood "literally and platonistically", they are false. This applies to statements of pure mathematics and mixed statements like 'the force due to gravity exerted on two objects varies inversely with the square of the distance between them'. If such statements entail the existence of abstract objects, then they are false, but, beyond that, Chihara shows no interest in their actual meaning.

Chihara argues that we do not need to sustain a hermeneutic claim to undermine the indispensability argument. Since the scientist could have used the ontologically parsimonious constructibility theory instead of standard mathematics, we need not conclude from the use of mathematics in science that mathematical objects exist, or even that the assertions of mathematics are true.

I am not sure that the main theme of this book allows Chihara to be so sanguine concerning semantic matters. At the outset, he provides a nice sketch of our goals:

> [P]hilosophers of mathematics ... seek to produce a coherent overall general account of the nature of mathematics (where by 'mathematics' I mean the *actual mathematics practiced and developed by current mathematicians*) – one that is consistent not only with our present-day theoretical and scientific views about the world ... but also with what we know about how our mastery of mathematics is acquired and tested. (p. 6, my emphasis)

Suppose that Chihara is right that science could have proceeded using the constructibility theory instead of the usual mathematics, and thus that the usual mathematics is, in some sense, not indispensable to science. But the constructibility theory is not the mathematics that is 'practised and developed by current mathematicians'. We are still lacking an account of how *mathematics* fits into the big picture. Indeed, we still do not have the promised general account of the nature of mathematics. We have instead an account of a possible surrogate.

Perhaps the main innovative theme of the present book is its account of the applications of mathematics, which draws on some insights of structuralism, while demurring from a structuralist philosophy. Chihara says, several times, that he is out to *validate* accepted mathematical reasoning. He correctly notes that an ''adequate view of mathematics should facilitate obtaining insights into the role mathematics plays in our theorizing about the physical world and about valid scientific reasoning'' (p. 229). He discusses a number of examples of applications, providing varying levels of detail along the way. The simplest of these is an inference whose premises are 'there are five dimes on table A at time t', 'there are seven quarters on table A at time t', 'a coin is on table A at time t only if it is a dime or a quarter', 'nothing is both a dime on table A at time t and a quarter on table A at time t'. The conclusion of the argument is ''There are twelve coins on table A at time t'' (p. 230). Chihara shows how to render the premises and conclusion in the constructibility theory. The counterparts of the premises, together with the constructibility analogue of the 'pure' statement '7 + 5', do logically entail the counterpart of the conclusion.

How does this 'validate' the ordinary reasoning, which does not go via the constructability theory? The conclusion of the reconstructed argument is the statement that the open sentence 'x is a coin on table A at time t' satisfies a 'twelve attribute' (the latter being a possible open sentence). Since this is not the conclusion of the unreconstructed argument, how does it validate the reasoning of *that* argument?

Chihara claims instead that ''as a native speaker of English'', he is ''in a position to know that each of the ... premises [of the argument] is true iff its' rendering into the constructibility theory 'is true''' (p. 231). Call a statement in the form

'$\mathbf{\Phi}$ is true if and only the constructibility rendering of $\mathbf{\Phi}$ is true'

a 'Basic Biconditional'. In a sense, the truth of the relevant Basic Biconditionals, together with the truth of the constructibility theory, would validate the reasoning in the given argument, showing it to be truth preserving.

I agree that there is some intuitive plausibility for something in the neighborhood of the Basic Biconditionals. A realist might very well accept them (perhaps wondering which is the chicken and which the egg). A hermeneutic nominalist might also accept them. But these folk have views concerning the meaning of the premises and conclusion in the given argument; their acceptance of the Basic Biconditionals is not based on simple competence with English and the constructibility theory. In places, Chihara concedes that it may be that, if understood literally, statements of pure mathematics do imply the existence of abstract objects. The same may also hold of the mathematised statements of science and ordinary language. Suppose, for example, that the conclusion of the unreconstructed argument above does entail the existence of the number 12 (as, for example, Frege believed). Or suppose that the more sophisticated statements of mathematised physics, such as the inverse-square principle, do entail the existence of real numbers. Then, as a nominalist, Chihara must hold that some of the key Basic Biconditionals – empirical premises used in applications – are in fact *false*.

I agree that Chihara need not claim that his constructibility theory gives the true and complete meaning of the statements of pure and applied mathematics. To some extent, he can demur from a detailed hermeneutic project. However, in order to 'validate' ordinary reasoning in applied mathematics the way he does, I submit that Chihara must claim that mixed statements that occur in applications are themselves literally true. That is what the Basic Biconditionals require. To go on to sanction nominalism, Chihara must also claim that the truth conditions of these statements do not entail the existence of abstract objects like numbers. This is where the realist will make her challenge. Unless there is some indication of what the actual truth conditions for the mathematised statements are, she need not feel threatened.

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DRUGS AND GLOBALISATION

Arthur A. Daemmrich, *Pharmacopolitics: Drug Regulation in the United States and Germany*. Chapel Hill: University of North Carolina Press, 2004. Pp. xiii + 203. US$34.95 HB.

By Nicolas Rasmussen

In this book Arthur Daemmrich asks how the 'therapeutic cultures' of different nations have shaped, and continue to affect, the assessment of drug safety and efficacy. This carefully structured, informative, and commendably compact book approaches the issue by comparing pharmaceutical approval processes in the United States and Germany since the Second World War, specifically examining five representative drugs spanning the second half of the 20th century: the antibiotic Terramycin, the sedative Thalidomide, the heart medicine Propanolol, the anti-cancer cytokine Interleukin-2, and the AIDS drug Indinavir. In all five cases the author tries to characterise the respective roles of the medical profession, government regulators, and the manufacturer in the processes by which these drugs were allowed onto the market (or disallowed), and for what medical uses. Framing the study unobtrusively in terms of actor–network theory, the book begins by describing how the governments of both countries acquired authority over pre-market approval of pharmaceuticals from medical bodies around 1940, how that authority evolved since the War, and also how professional bodies in medicine maintained a regulatory function – particularly when it came to monitoring untoward drug reactions once drugs are on the market ('pharmacovigilance'). The book paints a convincing picture of how physicians in Germany retained the cultural authority to guard the interests of the patient and decide on the appropriate use even of dangerous drugs, whereas in the United States this 'patriarchal' model of the doctor–patient relationship was abandoned in favour of a consumer model in which patients need to be

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protected from industry by the government, and enabled to make their own informed consumption choices (particularly since the era of AIDS activism). Perhaps more could have been done to explain why there was this widening difference in the authority retained by the medical profession in the US and Germany; my guess would be the failure of the US Government to ensure universal health care after the War, which inhibited long-term doctor–patient relationships due to uncertain private coverage arrangements, and ultimately eroded physician autonomy in the age of 'managed care'. But whatever the reason, the differing therapeutic cultures and how they coloured construction of regulatory procedures and assessment is clear.

Pfizer's Terramycin, the first drug discussed, is a nice example of the 'broad-spectrum' antibiotics introduced in the 1950s. Marketed not only for infectious diseases where cheaper and safer penicillin was unsuitable, but also for what would today seem an outrageous range of conditions, drugs like this were meant to be used by physicians essentially as a cure-all. Broad spectrum antibiotics were used in this extremely 'broad' way in Europe as well as North America, although in the particular case of Terramycin the drug was in short enough supply in Germany that there it was used more appropriately than others. The second case, the sedative Thalidomide, illustrates more interesting, qualitative differences between the national 'therapeutic cultures'. Invented by a small indigenous firm, the drug was the top-selling sedative in Germany by the end of the 1950s, but (famously) was never approved for US sale. Dow–Merrell, the US firm licensing the drug for the American market, pressured the US Food and Drug Adminstration (FDA) after their initial application of 1960 was rejected for inadequate safety testing in animals, meanwhile distributing 'experimental' samples of the drug very widely in the US. In Germany, where the line between experimental and official use was still less distinct because government regulation was weaker and the medical profession retained more of its traditional prerogatives to experiment upon patients, the drug had entered general use with less formal testing. But at the same time that Thalidomide was filtering into the US market via experimental samples and FDA approval seemed inevitable, German medical researchers were connecting the drug to an outbreak of phocomelia, a tragic birth defect in which babies had stunted, flipper-like limbs. Surprisingly, these unfortunate events led to strengthening of FDA regulatory power, but not that of Federal German regulators (who would seem to have needed reinforcing more). Germans interpreted the outcome as showing that medicine was self-correcting without state interference, while Americans saw this as illustrating the need for strict controls on drug firms.

The third case, the beta-blocker Propanolol, offers perhaps the most convincing account of two distinct 'therapeutic cultures' – as opposed to mere laws and regulations – in the two nations. Indeed, the disease for which this British heart drug was intended differed between the nations: in Germany angina pain was understood as a sign of *Herzinsuffizienz* (heart insufficiency), whereas in the US angina was not attributed directly to faulty heart action but to a secondary condition of the chest wall. The drug, with other beta-blockers, entered German practice easily in the 1960s, largely on the basis of subjective case reports, whereas the FDA required comparatively rigid double-blind controlled trials (particularly since slowing the heart was not an obviously rational therapy for angina in the US intellectual context). Eventually, it was approved for patients whose angina did not respond to older medications.

The final two cases discussed, the recombinant DNA cytokine Interleukin, introduced as an anti-cancer drug in the 1980s, and the anti-viral drug Indinavir, introduced for AIDS treatment in the 1990s, throw interesting light on more recent changes in clinical trial design to accommodate the demands of patients, particularly in the US. Essentially, the FDA had to relax rigidly 'scientific' demands for double-blind trials with patients taking no other medications, because these were perceived not only as an obstacle to drug firms but also to desperately sick people seeking access to experimental drugs. To me the story of these two recent drugs reveals more about differences in the formal regulatory frameworks in the two nations than about medical culture although Daemmrich makes a good point that, much more than Europeans, American patients have become impatient(s), demanding and untrusting consumers (a sound response, I think, to the unhealthy subjugation of medicine in the US to the health insurance as well as pharmaceutical industries). One gets the impression that American doctors might prefer to have the sort of jobs German doctors enjoy but are frustrated by the way corporate interests have shaped the 'therapeutic culture' of the United States, although Dammrich does not venture so far as this.

My main complaint is not about what the book does, but what it does not do. The characterisation of the drug companies as actors in all these cases does not match the depth with which the author treats medical and regulatory actors (with the partial exception of Thalidomide, in which case lawsuits brought corporate activities to light).

This is not surprising, as drug companies are notoriously secretive and seldom allow access even to very old internal correspondence, making it extremely difficult for the historian to explore what was going on within them. Nevertheless, it is disappointing that in this study we are often left with only the official story offered by drug companies to regulators and the general public. And understanding the drug companies as actors is politically crucial at the moment for a number of reasons. For instance, with their massive influence on the US Government (with by far the largest force of lobbyists and lobbying expenditure in Washington) pharmaceutical firms are currently attempting to impose obscenely high drug prices on people of all nations under the hypocritical banner of 'free trade' – at the expense of national health insurance systems and therefore lives everywhere. (This is hypocritical given that R&D of US drug firms is massively subsidised via transfer of Government-funded life science to private hands, and also that the entire industry only exists thanks to Federal regulation, which has saved pharmaceuticals from total market failure since the early twentieth century. The industry is a creature of protectionism and regulation.)

Or for a case more familiar to Americans, one may cite the travesty the drug industry lobby recently made of Medicare 'reform', turning a public medical insurance program for the disadvantaged into rank corporate welfare. Daemmrich does not delve into the wider policy issues raised by the drug industry's aggressive role in the moulding of government, the medical profession, and patient–activist groups. However, as he points out in his epilogue, differences in drug evaluation procedures among nations – the precise topic of this book – has great political currency in itself. Specifically, multinational drug firms are pushing to 'harmonise' pharmaceutical regulatory procedures worldwide through government trade bureaucracies, so that companies will only have to organise one set of pharmacological and clinical tests in order to gain approval to market new drugs in all developed nations.

While undoubtedly such 'harmonisation' offers great gains in efficiency for drug companies, as in most forms of globalisation the gains to big business come at the expense of almost everyone else. In this case, political autonomy (that is, democracy), cultural identity, and life itself all stand to suffer. This may seem an alarmist view to take over the technicalities of making procedures and forms similar in North American, European, and Japanese drug regulatory bureaucracies. The problem is that standards of safety have always been something that nations – that is doctors and other citizens (i.e. their patients) – were free to decide for themselves; but now this decision is being stripped away for the convenience of multinational corporations. Furthermore, the pharmaceutical industry now has the opportunity, by influencing international trade authorities engaged in this 'harmonisation', to alter safety standards in its favour. As John Abraham has shown, levelling-down is exactly what is happening: whatever safety standard is lowest among the nations engaged in 'harmonisation' tends to become the international standard. Moreover, medicine can differ greatly across cultures: doctors interact with patients differently, medicine plays different political and cultural roles, health is understood differently, and diseases are defined differently, as Dammrich's discussion of angina illustrates (and as historians and anthropologists of medicine have long known). Making drug safety testing uniform across nations requires flattening all these cultural differences for corporate convenience. Alas, one would hardly be aware of any of these pressing issues from reading this book. It merely concludes that differences in 'therapeutic cultures' will not be 'harmonised' away very quickly, too cautiously refraining from critique or any deeper analysis. Nevertheless I would recommend this book highly, especially for readers already versed to some extent in the ways of the pharmaceutical business.

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THE PURSUIT OF THE RIEMANN HYPOTHESIS

John Derbyshire, *Prime Obsession: Bernhard Riemann and the Greatest Unsolved Problem in Mathematics*. Washington, DC: Joseph Henry Press, 2003. Pp. xvi + 422. £19.95 HB.

By Mark Colyvan

With Fermat's Last Theorem finally disposed of by Andrew Wiles in 1994, it's only natural that popular attention should turn to arguably the most outstanding unsolved problem in mathematics: the Riemann Hypothesis. Unlike Fermat's Last Theorem, however, the Riemann Hypothesis requires quite a bit of mathematical background even to understand what it says. And of course both require a great deal of background in order to understand their significance.

The Riemann Hypothesis was first articulated by Bernhard Riemann in an address to the Berlin Academy in 1859. The address was called "On the Number of Prime Numbers Less Than a Given Quantity" and among the many interesting results and methods contained in that paper was Riemann's famous hypothesis: all non-trivial zeros of the zeta function, $\zeta(s) = \sum_{n=1} n^{-s}$, have real part 1/2. Although the zeta function as stated and considered as a real-valued function is defined only for $s > 1$, it can be suitably extended. It can, as a matter of fact, be extended to have as its domain all the complex numbers (numbers of the form $x + yi$, where x and y are real numbers and $i = \sqrt{-1}$) with the exception of $1 + 0i$ (at which point the zeta function is undefined). This extended zeta function takes the value zero for infinitely many complex numbers. For instance, all the negative even integers are zeros of the zeta function. These, however, are the trivial zeros. The Riemann Hypothesis is thus the conjecture that all the other zeros (and there are also infinitely many of them) have the form $1/2 + yi$. This hypothesis is of crucial importance in analytic number theory. The zeta function is very closely related to

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the prime counting function $\pi(N)$ (which is the number of prime numbers less than or equal to some natural number N). In fact, the zeta function 'encodes' important information about the distribution of primes, and the location of the non-trivial zeros of the zeta function are crucial in all of this.

In *Prime Obsession*, John Derbyshire attempts to bring the Riemann Hypothesis within the grasp of the general popular science reader. Very little by way of prior mathematical knowledge is assumed. Derbyshire gently and patiently leads the reader through the mathematics needed. I should add that a great deal of this mathematics – basic calculus, a little complex analysis, elementary matrix algebra, and the like – will be familiar to many readers of this journal. But for the general reader, this stuff needs to be there, and (for the most part) Derbyshire pitches it at just the right level.

He resists the temptation to go deeper than necessary into the many fascinating areas of mathematics he surveys. (The temptation is a little too great at a couple of points, but Derbyshire can hardly be blamed for indulging just a little in complex analysis, for instance. This is one of the most beautiful areas in all of human enquiry.) But this is not a mathematics text; it is clearly a popular science book. And as such it skips over the more difficult mathematics. The reader is often asked to trust Derbyshire on various matters. Again this is perfectly understandable. In fact, what is impressive is how infrequently the reader is asked to do this. The patient reader will come away from this book with a very good idea of what the Riemann Hypothesis is about and why mathematicians get so excited about it. That, in my view, is a huge reward for the modest effort required.

Derbyshire also tries to give one a sense of the importance of the Riemann Hypothesis by sketching its relationship to other results in analytic number theory (results such as Prime Number Theorem: $\pi(N) \sim N/\ln N$) and even applications further afield – in quantum mechanics, for instance (see Chapter 18). There is also a nice overview of some of the recent work on solving the problem. But this is not a book just about the Riemann Hypothesis. Derbyshire also provides a great deal of historical background on some of the main characters involved in the story: Leonhard Euler, Carl Friedrich Gauss, David Hilbert, Lejeune Dirichlet, Richard Dedekind, Jacques Hadamard, J.E. Littlewood, G.H. Hardy, Bernhard Riemann (of course), and many others. The book consists of alternating chapters on the mathematics and historical background. This approach works very well. The obvious trap with such an approach is that the mathematical story and the historical story may unfold at different rates (as indeed they do) and that this will lead to continuity problems. The latter is not the case here though. Derbyshire has obviously invested a great deal of effort into getting the alternating chapter structure to hang together and, in particular, into finding appropriate segues from one chapter to the next. This, I should add, was done without compromising the natural pace of the various stories.

Less successful, I thought, were some other concessions to the popular science genre. For instance, while the example of a 12-h clock to illustrate modular arithmetic is helpful, the continued reference to modular arithmetic as 'clock arithmetic' I found annoying. Any reader interested enough to read a book like this and patient enough to work through the mathematics presented I think is capable of understanding modular arithmetic (and have it referred to by its rightful name). Also Derbyshire's rather silly clock notation for rings (p. 268) I found distracting. And while on the topic of minor annoyances, I found the repeated reference to the Euler Product Formula, $\sum_n n^{-s} = \Pi_p(1 - p^{-s})^{-1}$, as 'The Golden Key' a little sensationalist and unnecessary. Still, as I've pointed out, the book is intended for a popular audience, and perhaps some readers appreciate such attempts at making the mathematics friendlier.

These minor complaints aside, I enjoyed reading Prime Obsession. Although the book is popular in style, it does a very good job of laying out most of the technical background required to understand one of the most important (if not the most important) open questions in mathematics. Derbyshire is to be congratulated for making such important mathematics accessible to non-specialists.

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BAROQUE SCIENCE[1]

David Freedberg, *The Eye of the Lynx: Galileo, His Friends, and the Beginnings of Modern Natural History*. Chicago and London: University of Chicago Press, 2003. Pp. xii + 513. US$30.00 PB.

By Michael Hill

The *Eye of the Lynx* is a study of the illustrated texts written by members of the Academy of Linceans, founded by Francesco Cesi (1585–1630) in central Italy in 1603. Freedberg says in the first chapter that his study began when "in 1986, in a cupboard in the Windsor Castle, I came across hundreds of the finest natural historical drawings I had ever seen" (p. 15). Subsequent research revealed an abundance of further manuscripts and published books of Lincean natural history. The lynx has a famously sharp eye, and most of the namesake Academy's work was produced with the aid of the telescope and microscope: new technology that had expanded immeasurably, more or less overnight, the macro and microcosm of creation. The research of the Linceans seemed to Freedberg to be on one level an heroic attempt to catalogue everything that could be known about the non-human world. Order and explanation were the problem: the detail was so overwhelming that Freedberg set his mind to discovering what epistemological paradigm it might illuminate. The book retains this duality, both a meticulous, at times fastidious, document of the Linceans' achievement, and an impressive account of an early episode of the scientific revolution, where the data gathered in the spirit and with the technology of the new empiricism eventually

[1]Editor's note: due to an administrative error a review of this work by Paula Findlen was published in the last issue as part of a survey review. I'd like to apologise to Professors Findlen and Hill but I'm sure our readers will appreciate both reviews.

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became almost indecipherable, however long it was interpreted within the existing world-view.

The book has four parts. The first is a lengthy introduction to the illustrated (mostly little known) texts under discussion and to the principal Linceans – the founder Cesi, his friend Francesco Stelluti, the secretary Anastasio De Filiis, the semi-tragic Johannes Henkius, and of course Galileo, a later addition to the Academy in 1611. The Linceans wanted to find out about nature and they could not rely on ancient theories: "it was crucial to make practical experiments and firsthand observations" (p. 68). Part Two discusses the astronomical observations of Galileo, their subsequent controversy, and their advocacy by the Linceans. The personality of Galileo, his scintillating observations, and his legal fate, cannot help but make this the most exciting part of the book. It is not, however, the longest nor the most important. That is Part Three, consisting of a vast amount of new material on studies of bees, plants, the new found flora and fauna of Mexico, and fossils. The final part is a lengthy summation of the limits of Lincean research, how their exquisitely pictured observations raised anomalies that could not be explained. Obviously Galileo is not the book's main subject, as the sub-title would have it, and it is odd that a publication of this standard resorts to such a cheap trick. It should be Cesi's name on the cover, as he was the driving force of the Linceans and is the most frequently mentioned individual. Freedberg, however, is less concerned with personalities than facts and ideas.

The central theme is the priority of visual evidence, and about how what we see can only take us so far towards scientific knowledge. The denouement of many episodes seems that of failure; from the local failure to convince Church authorities about the truth of heliocentrism to the global one of the inability of visual description to record something definitive about things. Ultimately, the book is about the frustration of facts outgrowing theories. Freedberg's material includes the most precise drawings imaginable, executed by high-class artists, who worked under the supervision of natural historians (that is, scientists) convinced that appearance could reveal inner workings. For them the eye was the mind's most powerful tool (on this I found it peculiar that Leonardo da Vinci was not discussed, for his drawings, including dissections, of plants and animals seem to me an important precedent). Physiognomy, the science of reading individual temperament from external features, was an important paradigm, and

Giacomo Della Porta, author of *De Physiognomia Humana* of 1586, emerges as something of a father figure to the Linceans. Yet in retrospect Della Porta stands at the end of a tradition, achieving fame just before the accumulation of factual anomalies was set to overwhelm the science his compendium had systematised. In 1628, William Harvey's study of blood would undermine the tenets of Galenic medicine critical to physiognomy; more conclusively, in 1649 René Descartes opposed himself to physiognomy's authorities, while emptying physiology of astrology and the philosophical 'chain of being' implicit in the older tradition of literature represented by Della Porta. A more mechanistic theory was the answer. The Linceans' work was thus pursued under a fading interpretative light. It is a story that could have been told by Thomas Kuhn, whom Freedberg acknowledges as one of his intellectual guides, along with Foucault, Koyré, and Feyerabend.

A telling analysis is made of *De florum cultura* (1633) by Giovanni Battista Ferrari, not a member of the Academy but closely associated with its methods and aims. In this text the same bella-donna lily is classified as two separate plants, because the examples the author had analysed were of different hues. Colour was a criterion of classification. Texture and shape were others. And why should they not be? Divining the character of something from its appearance was self-evidently legitimate, and colour and form were well understood in a culture accustomed to theorising visual properties. Somewhat more bizarre seems to have been the reliance on *rhetorical* categories as a means of plant classification, as in Ferrari's discussion (he was a professor of rhetoric in addition to being a horticulturalist) of the Chinese rose, whose colours change in accordance with the poetic sequence of sweet and useful. Yet this was more than quaint 'science' – Ferrari adapted his categories when he discovered they were unable to withstand microscopic evidence. He had to go inside the reproductive organs. Freedberg writes 'Having realized that he could not rely on pure externals for his classification, Ferrari had decided to look deeper within; and so, putting his microscope to use, turns to discuss its seeds … As if perfectly exemplifying what Foucault called the age of *mathesis*, Ferrari begins to count. He opens a seed-pod of the Chinese rose, and counts its seeds. He finds exactly 163, divided into six compartments. And with the patient task of counting over, Ferrari realizes that he has to look more closely at the structure of each seed …, (p. 46).

In Part Three, Freedberg considers the phallicised depictions of plants in another manuscript in the same terms, as a vague understanding of the critical importance of sex and genitalia in biological classification. Freedberg offers a classic summary of empirical research. "They begin with the joy of discovery [and] the plenitude of nature ... But in the end ... the Linceans [had] to come to terms with ... the unmanageability of abundance and the chaos of nature itself ... Cesi felt the need to find the order of nature, and to devise a system for conveying it. In this way he and his fellow Linceans were motivated by the desire to understand fertility as well as by the drive to classify" (p. 244). On the one hand, *De florum cultura* and such work seem anachronistic, but on the other the reproductive basis of modern classification stands in the distance, heir to the Lincean discoveries.

Freedberg is exceptionally good at setting science in its cultural context; perhaps it is his training as an art historian that makes him alert to the connection of patronage and propaganda. The sense that Roman baroque art was first and foremost a strident 'celebration' of the papal Church is so commonplace it barely requires further demonstration. Yet historians of science might still find it interesting that one of the Linceans' most impressive projects, three texts on bees published in 1625, which includes astonishingly detailed microscopic analysis, was directly related to the desire to secure the favour of the Barberini papal family, whose insignia were the same insects. The wisdom, chastity, benignity, and even fortitude (their hirsute necks were like a lion's mane!) of bees were extolled with nauseating ingenuity. The justification was Galileo's delicate position – the Linceans had to prostrate themselves before the papacy if Galileo was to avoid further censure. Of course, they failed. At this point in history, the ignorance of the Church was wilful and obdurate.

The Eye of the Lynx may soon be regarded as a classic of its kind. Freedberg is an acknowledged master of the social history of art; his understanding of 17th-century astronomy, biology, and palaeontology is no less convincing. Rarely does one find an art historian engaging with the history of science, except sententiously, and Freedberg could stand comparison with Erwin Panofsky (who wrote a short book on Galileo and the visual arts in 1954) or Martin Kemp. My only criticism is that he can take too long to make his point. Yet even this could be a virtue. Freedberg writes like an urbane empiricist, drawing his conclusions with care. His book makes a genuine contribution to knowledge.

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SEX AND SEXOLOGY IN IMPERIAL JAPAN

Sabine Frühstück, *Colonizing Sex: Sexology and Social Control in Modern Japan*. Berkeley: University of California Press, 2003. Pp. 257. US$19.95 PB.

By Aya Ezawa

Sexuality became an important topic of research as well as a subject of social control in early 20th-century Japan. While scholarly interest in sex was not new, sexual knowledge took on distinctively new features. *Colonizing Sex* traces how the rise of the modern state during the Meiji era (1868–1912) came with an intensified interest in sexuality and sexual practices of the population. The rise of a new health regime linked sexuality, health and the prosperity of the nation, and led to the increasing subjugation of the population to scientific scrutiny, bodily disciplining and mass regulation. Taking a Foucauldian approach, Sabine Frühstück examines the history of discourses about sex, illuminating their deployment in the surveillance and administration of the sexuality of men, women, and children. Based on a close reading of debates among reformers, sexologists, and activist movements, as well as analysis of social surveys, popular media and advertisements, *Colonizing Sex* provides a rich account of shifting sexual discourses and power relations, which should be of interest to scholars in science studies as well as those with general interests in modern Japan.

Colonizing Sex shows how discourses of sex created configurations of sexual knowledge that continue to inform discourses of sexuality in postwar Japan. The sexual science that emerged in the early 20th century was distinctive in its search for precision and empirical knowledge, drawing from developments in other fields of medicine and science. It produced new standards of normalcy and appropriate sexual behaviour that became a basis for later policies and practices. Sex research

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thereby not only legitimated its status as a scientific discipline, but also established a link between social problems and sexual behavior.

Frühstück examines the production of knowledge about sex in a range of different yet interrelated sites. The first part of her discussion provides a fascinating account of how state and empire building came with an increased concern with the health as well as sexual practices. With the establishment of military conscription, soldiers were subjected to physical examinations and new standards of hygiene aimed at improving the health and strength of the imperial army. Venereal diseases were no minor issue at the time. During the Russo–Japanese war, for instance, 1387 men were killed in battle, 2066 were wounded and an astonishing 2012 fell victim to venereal disease. To protect soldiers' health, military brothels were established in form of so-called 'comfort stations' and prostitutes were subjected to mandatory health examinations. Children, as future subjects and soldiers, likewise, came under the gaze of research, propelled by a discourse, which emphasised the need for the measurement, assessment, and theorisation of children's health. Masturbation and children's sexual desires in particular, were feared as leading to social problems and instability. A desire to refashion subjects and strengthen the empire thus led to an increasing quest for knowledge, surveillance and control of sexuality, aimed at protecting soldiers' health.

By the 1920s, sexual knowledge spread to the masses through newspapers, sex journals, and public lectures, and sexologists began to conduct sex surveys, which documented the sexual experiences and practices of the masses in unusual detail. Masturbation, it was found, was a nearly universal experience, and hence could not constitute a behavioural abnormality. Sexologists became active in legitimating sexology as a science that not merely dealt with sexual pathology, but created sexual knowledge that served as a key to the liberation and enlightenment of the population. Frühstück's analysis shows in great detail how sexual issues from masturbation and sex education to birth control became subject to competing positions, which sought to resist or extend the dominant discourse about sex.

Whereas the 1920s were characterised by the spread of discourses of sex into the public, the rise of militarism and eugenic thought in the 1930s led to the suppression of the topic and its disappearance from the public sphere. Sexology journals were subjected to severe censorship, public lectures were interrupted, and in 1937, the last of several sex research journals closed its doors. Despite these changes, the basic configurations of sexual knowledge remained in place in postwar Japan. As Frühstück shows in the epilogue, the measurement and assessment of sexual behavior, as well as standards of normalcy continue to shape discourses about sex in postwar Japan.

The development of sexology in the early decades of the century is a captivating example of the creation, circulation, and colonisation of sexual knowledge and practices in Japan. Frühstück's close analysis of debates among sexologists, survey results and voices of ordinary people in popular journals provides a rare view of sexual discourses and behavior at the time. She also carefully traces resistance among the general populace to new technologies of surveillance and control, as well as diversity and difference among scholars and activists involved in the field of sexual research. Together, her analysis combines theoretical depth with a powerful empirical study that will enrich not only our knowledge of Japan but also discussions of sex, power, and knowledge.

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PHARMACEUTICAL INNOVATION: TOO IMPORTANT FOR THE PRIVATE SECTOR?

Merrill Goozner, *The $800 Million Pill: The Truth behind the Cost of New Drugs*. Berkeley: University of California Press, 2004. Pp. 297. US$24.95 HB.

By Takuji Hara

The process and price of new drug development is not widely understood, even among pharmaceutical industry insiders. In this book, Merrill Goozner, a former Chief Economics Correspondent at the *Chicago Tribune*, provides us with a simple explanation of this enterprise, filling a major gap in the literature. This does not mean that his book is a full description, nor an impartial narrative. However, the importance of the contribution is unquestionable, since it is the first accessible book-length study on current-day pharmaceutical research and development.

It is often reported that people in the United States go to Canada in order to buy brand-name medicines at cheaper prices, whereas people in low income countries demand that pharmaceutical companies allow them to make and buy AIDS drugs regardless of patent protection and royalties. Health insurers and governments are now extremely sensitive to the cost–benefit balance of new drugs. The pharmaceutical industry, on the contrary, asserts that without the high price of their newer products it would be impossible for them to remain innovative because it costs more than US$800 million to discover and develop a new drug. Accurate costing of pharmaceutical research and development is crucial to these issues and Goozner's book aims to do just that, assessing the industry's claim that approximately US$800 million is necessary to develop a new drug. In fact, he spends a much larger portion of the book describing the drug discovery and development process (Chapters 1–8) than discussing the cost (Chapters 9–10, plus several pages elsewhere).

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The descriptive portion covers the development of erythropoietin (the blood-forming hormone by which Amgen became a paradigm for success in biotechnology), orphan drugs developed by Genzyme and TKT, DNA sequencing machines (which brought a big fortune to Applied Biosystems in the era of the genome races), AIDS drugs, anticancer drugs, anti-peptic ulcer drugs, and Cox-2 inhibitors. In particular, the description of the development process of AIDS drugs is thickest, filling three chapters. However, these descriptions are not simply an overview of drug research and development; Goozner also endeavours to highlight how researchers in universities, government research institutions, and private companies supported by public funds contributed greatly to the basic research and the early stages of development. He argues that none of these drugs are genuinely invented by pharmaceutical firms: the industry's claim that they have created these drugs is baseless.

Goozner's explanation is clearly based on a simplistic view of science, insensitive to the ambiguity and uncertainty of both biomedical knowledge and the sociological processes behind its production. His account makes us feel as if there were little risk of failure when private companies set out to turn public research into practical new therapies. He also seems to assume that companies distort 'true' science created by academics. However, consider that when James Black, a corporate researcher, endeavoured to find histamine H_2 antagonists that could reduce ulcers, most academics in the 1960s did not believe the relationship between histamine and acid secretion (to take one example). Goozner treats such cases only insufficiently. In general, far too little attention goes to the work conducted inside the pharmaceutical industry by corporate researchers, in contrast to research work outsourced by drug firms to academia. As a consequence, this book includes little about the efforts towards the optimisation of drugs, the development of production process, and the vast number of failed projects – all of which are essential to introducing a new drug, and all of which cost companies a great deal. These deficiencies undermine the robustness of his argument.

Despite such drawbacks, the book includes much that is sociologically interesting about drug innovation. For example, the development of AIDS drugs is fascinating, due to the complex interaction among various actors including public research institutions, pharmaceutical companies, governments, activists, medical practitioners, and other groups. It also discusses important institutional matters such as patents, research subsidies, 'compassionate use' programmes, drug price regulation schemes, lobbying, and international politics. The account describing how a company that was publicly subsidised by the government for the early stage development of an AIDS drug cunningly changed the compound so that they could evade the royalty to the government can be seen as the essence of this book: the industry has a tendency to hide public money in their creation of new drugs. Pharmaceutical companies' reluctance to seek combination therapies is another problem of profit-driven drug development. For example, Roche asked for approval of Saquinavir, their protease inhibitor, as a monotherapy in 1995, though there had already been mounting evidence against monotherapy by the time. It seemed to be out of the question for the company to develop combination therapies with new drugs developed by rival companies, even if such therapies might be better for curing people.

Goozner also accuses pharmaceutical companies of developing many imitative 'me-too' drugs and pouring huge money into their clinical trials in an effort just to show some minimal advantage over older drugs to justify the effort. He argues that most of the claimed cost of new drugs in reality goes to this kind of research, the real purpose of which is commercial rather than therapeutic. The accounts of the development of Prilosec by AstraZeneca and of Cox-2 inhibitors are very stimulating. Merck's Vioxx, one such drug, has recently been withdrawn from the market because of its side effects. If the merits of the drug were really marginal, why did it sell \$2.5 billion a year? Goozner's answer is simple: because the company invested enormous amount of money into promotion activities many of which were dressed up as 'clinical trials'.

In Chapter 9, Goozner depicts the political controversy over the cost of drug development. From the taxpayer's perspective, the industry's estimation neglects the contribution of taxpayer-funded research and includes the cost of clinical trials of which the purpose is promotion rather than therapeutic advance. From the industrial side, the critics ignore the opportunity cost of capital and wrongly assume the possibility of one-size-fills-all drugs. The classification of data and the definitions of categories are also objects of dispute. Unfortunately, standing firmly on the taxpayer's side, Goozner's analysis of this controversy is insufficient, with little original evidence. This somewhat weakens the book's power of criticism.

Goozner provides some prescriptions for change in Chapter 10. They include government-funded clinical trials to investigate the cost–benefit balance of new drugs, governmental support for the generation and diffusion of information regarding the best clinical use of existing drugs, an 'independent' (i.e. not funded by the pharmaceutical industry) institute investigating the economics of new medicines, the change in FDA regulatory process for new drug approvals, the modification of patent laws to remove excessive intellectual property obstructions for drug research, and the creation of new organisations for drug development, that is, a network including non-profit drug developers and specialised outsourcing companies.

Although Goozner's prescriptions are worth serious consideration, there are at least two fundamental questions. First, is government-driven or non-profit organisation really more efficient than private companies? Why shouldn't advantage be taken of the vigorous driving force towards cost down, innovation, and good services that competition among profit-seeking companies provides? In principle, markets exclude worthless products. From the perspective of economic theory, the problem seems to be insufficient knowledge and information for the market to function efficiently, not the pursuit of profits *per se*. Therefore, I am not convinced of the idea that the creation of a non-profit network organisation for drug development is a good solution. The second question is whether 'me-too' drugs are really devoid of any value. Some 'me-too' drugs actually have distinct features (fewer side effects, longer acting, easier to use, etc.) compared with existing drugs in the same class. These features may be trivial for academic scientists who are on the frontiers of science, but they can be blessings for particular patients or their carers. After all, innovation is not the same as scientific advance. In addition, from a managerial point of view, imitation-based drug development is important to manage risk, carry business forward, and build organisational capability. Major innovation does not happen so often, and private companies cannot keep solvent only by seeking them. If pharmaceutical companies cannot run businesses for a long time, the supply of existing drugs may face uncertainty, and this may risk the lives of patients. Imitation is also an excellent opportunity for organisations to accumulate capabilities, which may support major innovations in future. Therefore, therapeutically beneficial 'me-too' drugs seem to be worth developing, though the pursuit of radically new drugs for great therapeutic leaps is, of course, important as well.

In sum, Goozner's *The $800 Million Pill* is an excellent work for anyone unfamiliar with the modern pharmaceutical innovation to understand its process and problems. It is not a complete account. Despite its journalistic flair, it is lacking in analyses of social process, especially inside private organisations. Its interpretation of phenomena tends to be one-sided, and devoid of sufficient reflexivity. The discussion on the cost of drug research and development, which constitutes the subtitle of the book, is not very rigorous. However, attracting more interest in the issue is probably more important than these flaws. In any case, this book is one of the best introductions to the issue of modern pharmaceutical innovation and its costs.

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LIFESTYLE AND THE STRUGGLE FOR SURVIVAL

Michael Hau, *The Cult of Health and Beauty in Germany: A Social History, 1890–1930*. Chicago and London: University of Chicago Press, 2003. Pp. ix + 286. US$53.00 HB US$22 PB.

By Matthew Jeffries

In his classic novel *The Magic Mountain*, Thomas Mann memorably chose an Alpine sanatorium as the setting for his allegorical examination of Europe's sick society in the early twentieth century. Arriving at the International Sanatorium Berghof for a three-week stay, young Hans Castorp is at first amazed and amused by the range of treatments on offer: '"What! You lie out on your balcony at night, in the damp?" he asked, his voice shaking . . . "I'm perfectly worn out with laughing", he said, and breathed through his mouth. "You've told me such a lot of crazy stuff – that about the psycho-analysis was the last straw".' There is a 'lot of crazy stuff' in Michael Hau's engaging study of German health and lifestyle reformers too, although as with Mann's novel, the mood turns much more sombre before the end. We learn, for instance, of Louis Kuhne, a chronically sick carpenter's journeyman from Saxony, who 'healed himself' with natural therapies and became a wealthy and successful 'health entrepreneur', with a Leipzig clinic and a thriving mail-order business selling his patented steam bath tubs. His particular speciality was the *Reibesitzbad*, which he recommended especially to women. It provided a repeated and gentle washing of the genitals, and was intended to "expel toxins by stimulating the body's life force at the place where all the important nerves of the body were thought to converge" (p. 112). Kuhne also believed that disease always affected bodily form, so one could diagnose (and even predict) diseases by reading a patient's facial expression.

While Kuhne was recognized as a quack by many contemporaries, the same could not be said of Carl Heinrich Stratz, a respected

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gynaecologist, who became better known as an author of lavishly illustrated books on the female body (read, not surprisingly, mostly by men). For Stratz, beauty was the expression of a scientific and divine law governing the proportions of healthy individuals, although it only applied to the most 'advanced' races. The good news for women was that the 'normal ideal body' could be achieved by any civilised person (*Kulturmensch*), provided they adopted a hygienic lifestyle; the bad news was that if they were in long-term employment, their bodies "would become square, their facial expressions sharp and severe, and their figure and movements masculinized" (p. 70). Such arguments were not surprisingly dismissed by feminist lifestyle reformers, but they did not dispute the 'degeneracy' of contemporary women: the blame, however, lay with men and the corset. One such feminist reformer, the vegetarian activist Klara Ebert, argued that only independent and enlightened women would be able to produce healthy offspring, which also led her to favour selective breeding, as the only way to save society from Nietzsche's 'many too many' (*den Vielzuvielen*). Another militant vegetarian introduced by Hau was Rudolf Franck, who 'deplored the sentimental compassion of his contemporaries', and who attacked modern doctors for their attempts to prolong life at all costs, 'even if that meant artificial feeding and oxygen supply'. Franck's solution was brutally simple: "one could get rid of degenerates by employing such devices as electric chairs, which would induce a quick and painless death" (p. 119).

It is to Michael Hau's credit that he resists the temptation to dwell for too long on the quacks and cranks, prophets and pioneers, who made up Germany's lifestyle reform milieu. One could write a very long book indeed on these fascinating characters, but Hau's volume is instead notable for its brevity: in less than three hundred paperback pages he offers sharp, balanced judgements on a wide range of topics, many of which have already been the focus of much longer studies. Inevitably, perhaps, this becomes the book's great strength and its principal weakness. Teachers and scholars looking for a handy and well informed introduction to debates on health and hygiene, gender and race in early twentieth-century Germany, will find this book an ideal companion. Hau's prose is clear and refreshingly free of jargon, despite a willing engagement with the theories of Bakhtin, Bourdieu, Foucault, and Gilman. It also strives hard – perhaps too hard – to avoid cheap and obvious connections to a later period of German history, arguing that the *Volksgemeinschaft* forged by the Nazis was "quite different" from the "utopian communitarian visions of many life reformers before 1933'' (p. 8). This is true, but some may feel that by appearing to deny any continuity between the radical racial hygiene practised in the Third Reich and earlier eugenicist discourses, the author is over-compensating for the teleological perspective adopted elsewhere. In fact, of course, Hau is well aware of the continuities, but his admirable desire to offer a succinct and sensitive appraisal of lifestyle reformers 'in their own terms' means that potentially profitable lines of inquiry are not pursued. This criticism could also be levelled at other areas of the book, where its large and complex topics – lifestyle reform, naturism, eugenics and racial hygiene – are not always well served by the volume's brevity. For the most part, however, the author's efforts to synthesise and simplify are impressive, and deserve credit rather than reproach.

Hau, who lectures in Modern European History at Monash University in Australia, argues that Germans in late-nineteenth and early-twentieth centuries ''increasingly defined their personal problems in medical terms, described them in medical language, and understood them in a medical framework'' (p. 3). The new awareness of the importance of health, or 'body-consciousness', manifested itself not only in the clash between orthodox 'scientific' medicine and the proponents of 'alternative' therapies, but also in the rise of bodybuilding and naturism; in panics over venereal disease or gender relations; in the discourse on national efficiency; and in debates on euthanasia and eugenics. Such conflicts, he suggests, should not be viewed as a clash between modern and anti-modern forces, but rather as one of the inherent contradictions within Detlev Peukert's crisis years of 'classical modernity'.

One reason why lifestyle reform became such an attractive proposition to middle-class Europeans around 1900 – and maybe why it remains so in the era of globalization – is that it allowed for agency: although people could not change their personal circumstances ('the unhappy intimate relationships, the failed social aspirations, the frustrated career expectations'), they could still control and discipline their bodies. ''Through the control of their bodies'', Hau suggests, ''they hoped to regain the fitness that would enable them to succeed again in the perceived struggle for survival'' (p. 15). Of course, the concept of *Lebensreform* was sufficiently vague to mean ''different things to people with different social experiences'' (p. 32), and Hau comes up with some convincing explanations as to why ''the educated middle classes held different ideas about the importance of physical culture for the development of harmonious and cultivated personalities than did those from the lower middle class" (p. 44). Indeed, Hau's attention to class, as well as race and gender, is a welcome feature of his book, which combines some of the best virtues of both social and cultural history, and therefore deserves wider recognition than its title might initially command. Of course, it is never likely to attract as many readers as Mann's *Magic Mountain*, but it may help to explain why Hans Castorp's stay at the Berghof sanatorium was eventually to last some seven years.

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WIELDING THE SYRINGE OF EMPIRE

Ming-cheng M. Lo, *Doctors within Borders: Profession, Ethnicity, and Modernity in Colonial Taiwan*. Berkeley: University of California Press, 2002. Pp. xvii + 239. US$19.95 PB.

By Paul D. Barclay

Doctors within Borders, the first publication in a University of California Press monograph series titled ''Colonialisms'', explores the interlocking forces of modernity, ethnicity, and profession through a study of 'native physicians' in colonised Taiwan. In this wide-ranging book, Professor Lo sets before herself two tasks: augmenting theoretical approaches to the sociological study of professions; and rescuing the voices of early twentieth-century Taiwanese from oblivion. The dominant modernisation paradigm used to assess 'Japan's impact on Taiwan', whether critical or laudatory of Japan's colonial projects, has emphasised abstract forces and large units-of-analysis at the expense of individual agency and local detail. Lo addresses this imbalance by examining how medical education, licensing, and practice under a regime of 'scientific colonialism' enabled an ambitious, privileged and conflicted cohort of Taiwanese elites to alternately fight against and further the expansion of Great Imperial Japan. As Lo demonstrates, professionalisation as a force in Taiwanese history was deeply conditioned by the vicissitudes of ethnic and class struggles. Therefore, the author challenges us to conceptualise professionalization, undoubtedly a major force in global modernity, as a site-specific phenomenon with multiple configurations, many of them deeply intertwined with colonial pasts.

For Taiwanese subjects, medical school was a socially prestigious, academically competitive, and financially remunerative path to upward mobility under Japanese colonial rule. When the Japanese annexed the island in 1895 (as a result of the Sino–Japanese War), the

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new Government General, outnumbered as it was, laboured to co-opt Taiwan's old land-holding/literati elite (gentry) through the distribution of minor government posts and cultural awards. Lo emphasises that Japan's most influential colonial administrator, Gotô Shinpei (Civil Administrator 1898–1906), used medical and biological metaphors to justify and implement a regime of colonial modernity in Taiwan. Specifically, Gotô viewed the diffusion of modern medicine in Taiwan as a way to display Japan's colonial prowess to the outside world and to cement the loyalty of colonial subjects to their new overlords. As part of its program of co-option and 'scientific colonialism', the Japanese Government built an island-wide system of hospitals and a sanitary infrastructure on Taiwan; concomitantly, it opened a medical school for the training of 'native physicians'.

During the 1920s, when the first generation of Japanese-trained professionals came of age on Taiwan, two strands of civic activism emerged on the island in the relatively liberal atmosphere of 'Taishô Democracy'. One branch was Marxist and called for independence and wealth redistribution. The 'progressive' wing, on the other hand, sought rights equal to Japanese citizens by petitioning for Home Rule. Politically, Taiwanese doctors implicitly acknowledged the empire's legitimacy by couching demands in the language of greater participation in the Japanese polity. On another level, the activist-physicians were subversives, arguing for a separate parliament based on Taiwan's status as a sociologically unique entity. Economically, physicians benefited more than most Taiwanese under Japanese rule. Yet, they suffered official and informal discrimination *vis-à-vis* Japanese in almost every facet of life. Therefore, Taiwanese doctors occupied a position 'in-between' colonizer and colonized along multiple axes (p. 52). The consequences of these in-between 'structural locations and ambivalent social identities' changed radically from the early 1920s through 1945, as the empire itself underwent a series of profound transformations.

Lo organises the heart of the analysis around a standard chronology of Japanese imperialism, dividing her narrative into three temporal units. The 1920–1931 period, Lo's first, saw the emergence of 'National Physicians' in the context of a relatively robust Taiwanese civil society. Lo highlights several of what she terms 'structural contradictions' to bring state directives, professional initiatives, and ethnic concerns into play as interrelated factors in the formation of policy and identity. Japanese colonial authorities decreed an ethnically segregated two-track medical infrastructure, but were frustrated by Japanese physicians whose professional ethos demanded integration of medical schools and hospitals. Doctors developed a Taiwanese consciousness by practising medicine among a disenfranchised and violently abused population of fellow ethnic subalterns. Yet, these same doctors internalised a 'cosmopolitan' medical discourse that regarded their ethnic traditions as 'local' and 'backwards'. Another source of tension: Taiwanese doctors' incomes as private physicians rested on a fee structure that put their services out of reach of the average colonial subject, placing their financial aspirations as members of an occupational group at odds with the general welfare of their fellow Taiwanese.

As the wealthiest and most socially prestigious members of the new educated indigenous elite (in distinction to the old landed gentry or merchants), the physicians articulated Taiwanese ethnic consciousness through leadership and patronage of cultural, journalistic, and outreach activities while using their professional platform to denounce police brutality and the Government General's profitable though morally bankrupt opium monopoly. They also sought to contribute to the 'health of the nation' by providing discounted service to financially disadvantaged Taiwanese. Lo argues that political activism and ethnic identification among doctors flourished under conditions that permitted a modicum of freedom in the arenas of publication and assembly. But as this atmosphere changed in response to metropolitan directives, the native physicians' conflicted structural positions and hybrid identities left openings for radically new forms of affiliation.

Lo calls the years between the Manchurian Incident and the full-scale assault on mainland China, 1931–1936, "The Years of Public Demobilization". As the Japanese state heightened its sense of national mission in the face of perceived foreign encirclement and Chinese nationalism, the Government General in Taiwan clamped down on freedoms of press and assembly, banning progressive civic organisations. Starting earlier, but accelerating after 1930, Taiwan became the pivot of the Japanese medical empire as a centre for the study of tropical medicine, bringing the medical community in Taiwan under much closer government scrutiny and regulation. This period is transitional for Lo, characterised by Taiwanese physicians' struggles to tack between imperial, Taiwanese, Chinese, and Japanese identities in the absence of public forums in which to forge a collective political voice.

The third and final period, bounded by the Marco Polo Bridge Incident and Japan's defeat in the Pacific War, 1937–1945, is titled

"Medical Modernists". This period is known for Japanese policies centred on *kôminka*, or 'imperialisation'. In Korea and Taiwan, colonial subjects were increasingly forced to assimilate language, naming practices, and even religious worship to Tokyo-defined norms through a variety of coercive policies. As higher-echelon Japanese *colons* were called home and abroad to build an empire in China and beyond, Taiwanese physicians moved up the ladders of responsibility, respectability, and income on the island. Cutoff and lured away from sources of Taiwanese or Chinese ethnic identification, as agents of expansionist Japan in programs to spread modern medicine throughout the East-Asian Co-Prosperity Sphere, Taiwanese doctors began to internalise the universalising discourses of empire and cosmopolitan medicine.

Lo thus charts the long journey from a period in the 1920s when professional identity in the Taiwanese medical community took on a strongly ethnic and moderately anti-colonial cast through the early 1940s, when Taiwanese physicians began to identify primarily with their fellow professionals and with the goals of empire. Interestingly, in interviews conducted long after the War, Taiwanese physicians remembered their academic ascents up the colonial meritocratic scale as a central facet of their experience under Japanese rule, evincing little explicit awareness of the marked inequalities and forms of discrimination/segregation that characterised life in Taiwan from 1895 to 1945.

In her last empirical chapter, Lo compares Japan's medical mission in Taiwan to Japanese imperial medicine on the mainland in the 1930s. Through a discussion of the *dôjinkai* or 'benevolent societies', Lo points out marked differences, according to place and time, in the enactment of Japanese 'scientific colonialism' in Asia. On China's mainland, Japanese colonial officials showed little interest in diffusing medical knowledge through a native elite or instilling civilisation amongst a backward populace. Rather, Japan built hospitals and offered free exams to compete with Western mission hospitals for the hearts and minds of Chinese in the treaty ports while they pioneered vast public vaccination programs in the country to prepare areas of China for colonial penetration. Through an inventive and adroit use of archival, journalistic, and oral sources, Lo points up some of the most crass hypocrisies of Japanese colonial medicine as practised on the mainland. The situation on Taiwan, which consumes most of the book, was more complex, and will yield many rewards to the careful reader. Lo herself is forced to admit that by the end, Japanese colonial medicine had won the admiration or at least grudging respect of Taiwanese high and low for improving health throughout the island. At the same time, Lo's careful and well-documented analysis recovers the poignant and conflicted voices of the colonised to allow readers to gauge for themselves the cost of this 'colonial success story'.

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CYBERNETIC ORIGINS

David A. Mindell, *Between Human and Machine: Feedback, Control, and Computing before Cybernetics*. Baltimore and London: The Johns Hopkins Press, 2002. Pp. xvii + 439. US$46.00 HB.

By Andrew Pickering

I approached this book as a historian of cybernetics, and I read it, at least on the first pass, as a very interesting historical critique and attempted revision of the standard story of the origins of the field. Historians have, for obvious reasons, seen Wiener's famous book, *Cybernetics* (Cambridge, MA, 1948), as a key point of entry: it was Wiener who came up with the field's name around 1947, and this popular book first brought it to the attention of the educated public.

Over the past decade or so, however, this origin story has come under attack. Wiener did not create the field from nothing; his book was a synthesis of the work of many hands, in the US and in Europe; and the trajectory of much of this work did not follow Wiener's. These revisions, in effect, change the subject, deflecting attention away from Wiener as cybernetics' prime-mover. But in *Between Human and Machine*, David Mindell takes on Wiener himself. The subtitle of Wiener's 1948 book was *Control and Communication in the Animal and the Machine*, and Mindell puts Wiener in his place by offering us an admirable history of control and communication engineering in the US from roughly 1916 to 1945, from World War I to World War II. Four substantive chapters deal with disparate threads of the interwar story, covering the development of fire-control systems for naval guns, aircraft control systems such as automatic pilots, feedback amplifiers for telephone networks, and analogue computers and feedback theory. Two transitional chapters then lead into four further chapters on developments in World War II in which these lines of work were both taken further and integrated with one another in, for example, work on anti-aircraft gunnery.

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These ten chapters are the heart of Mindell's book, and they are exemplary works in the history of science and technology. Mindell's writing is resolutely focussed on the technology and how it works, resolutely material – there are many wonderful photographs of the machines themselves and diagrams of their functioning. He does not shy away from important technological or theoretical details (which means that some passages can be daunting, but in general he does an excellent job of explanation). The writing is also resolutely social, situating the work Mindell discusses in particular places, companies, academic laboratories or branches of the US military, and exploring the relations between these institutions.

There is no way here to mention all of the topics covered in *Between Human and Machine*, but a glance at the first historical chapter can indicate Mindell's style and interests, and the way it serves to undercut a Wiener-centric history of cybernetics. The topic is naval control systems, especially fire-control systems, which addressed the problem of accurately aiming heavy guns at a moving target (another ship) at long range, from a platform that was itself moving on a rolling sea. As Mindell tells it, the solution to this problem was a construct built from humans and machines (hence the title of his book) that became ever more complex and sophisticated from WWI and into WWII. The sensory input to this construct was provided by human observers, determining the range and bearing of the enemy via optical instruments. This information was then fed, mechanically or, later, electrically, to a plotting room deep in the ship. There it was entered, either manually, by human pointer-followers, or, later, automatically, into a device called a range-keeper, also referred to as a computer (an analogue one, as we would now say). And the job of this computer was prediction: calculating where the enemy ship would be at the time it took a shell to reach it. Current bearings of the target ship would be fed repeatedly into the range-finder until its predictions came into line with later observations, at which point the predicted bearings would be transmitted to the guns for firing – again, either manually or, later, automatically via servo-control devices.

As Mindell notes, one can recognise here one of those human/nonhuman hybrid systems of distributed cognition that Edwin Hutchins described and analysed so well in *Cognition in the Wild* (Cambridge, MA, 1995). More specifically, anyone interested in the history of cybernetics would also recognise all of the elements of Wiener's problematic in anti-aircraft fire control, 25 years *avant la lettre*: questions of how to track moving targets, how to transmit data, how to move quickly from data to prediction, how to deal with data that are inevitably uncertain and noisy, how to move from prediction to firing a gun, how to integrate all of the human and nonhuman elements in such a system, Even the pathologies of feedback systems that Wiener discussed in abstract terms in *Cybernetics* were already evident as practical problems in naval fire control in WWI. 'Hunting,' for example – the tendency of feedback systems to oscillate around some desired setting – impressed itself on engineers in real and important material settings: a fire-control system that has the guns continually swinging back and forth and never coming to rest on their targets is a pretty useless system in warfare.

Thus one reading of *Between Human and Machine* – as a detailed and very complex prehistory of Wiener's cybernetics. The book tells us something new: that Wiener's work on fire control was not a *sui generis* origin for cybernetics, but should rather be seen as just one contribution to much wider continuing traditions of research, development and theory in the fields of control and communication engineering, This is a very interesting and stimulating idea. And yet there are two problems with it. One is that the phrase 'before cybernetics' in Mindell's title invites the reader to think of Wiener's work as the *summum* of the various pre-War engineering traditions that this book dissects so nicely – as if everything at last came together with Wiener and then headed off in a new direction because of that. Mindell is aware that his title and framing of the book seem to point in this direction – "It is tempting to call the story that follows a prehistory of cybernetics" (p. 6) – and he argues explicitly and quite rightly against it: the book is not "a mere corrective to Wiener's origin story" (p. 7). On the one hand, Mindell shows that Wiener's work was a minor and, in fact, unsuccessful part of a much larger research effort into anti-aircraft gunnery sponsored by the NDRC – the National Defense Research Committee – in the run-up to the US entry into WWII. On the other, he shows that wartime developments in control and communication engineering led to many major technological achievements that owed no debt to Wiener whatsoever: the Whirlwind digital electronic computer, the SAGE air defence system, numerically controlled machine tools, global positioning systems, the SABRE computerised airline ticketing system, and more. From this perspective, Wiener was a blip – *albeit* an important one – in the history that Mindell tells.

There is, however, another problem in reading this story as a contextualisation of Wiener that Mindell seems less aware of. One thread of his argument is that Wiener covered his tracks in *Cybernetics*. By not mentioning the engineering and theoretical work that Mindell's book is about, Wiener obscured a constitutive debt to that work. I would be happy if that were the case, but Mindell did not quite persuade me of it. Mindell wants to insist that "The omissions [in *Cybernetics*] are striking. Wiener must have been aware of his predecessors ... " (p. 286). I worry about that 'must have' in an otherwise meticulously documented book, especially when a few pages earlier Mindell quotes Warren Weaver, the head of the NDRC's fire-control Section D-2, as noting in September 1942 that Wiener and Bigelow "have gaily started out on a series of visits to military establishments, without itinerary, without any authorizations, and without any knowledge as to whether the people they want to see (in case they know whom they want to see) are or are not available. ... This item should be filed under 'innocents abroad'" (p. 281) – a phrase that Mindell adapts as his subhead on the page before: 'Innocence Abroad.'

'Innocents abroad' – perhaps Wiener was not so much covering his tracks as displaying a genuine ignorance in *Cybernetics*; perhaps, conversely, the prior achievements of these branches of engineering were not as important to the emergence of cybernetics as Mindell would like to think. Perhaps we still need to look elsewhere to see where Wiener was coming from – to his well known generalising and interdisciplinary tendencies (as manifest in his collaboration with the physiologist, Arturo Rosenblueth), and to his background as a mathematician: the lasting technical product of Wiener's foray into gun control was the mathemetical analysis presented in his restricted 1942 report on *The Extrapolation, Interpolation, and Smoothing of Stationary Time Series with Engineering Application*, rehearsed for the public in mind-boggling technical detail in *Cybernetics* itself.

This is not to detract from the virtues of *Between Human and Machine*. It remains an exemplary work in the history of technology, even if, in the end, Mindell fails to connect its substance convincingly into the history of cybernetics.

But I want to close by noting that Mindell's thesis on the origins of cybernetics might have more in its favour than he succeeds in showing. Remarkably enough, despite Mindell's twenty-four page bibliography, it appears that I have read something he hasn't – Norbert Wiener's novel, *The Tempter* (New York, 1959). And the plot of this novel revolves precisely around the interwar careers of two control engineers, one in industry, the other in a university. Of course, one cannot read back Wiener's state of knowledge in the early 1940s from a book he published years later. But one can certainly read this book as an acknowledgement by Wiener of some of the pre-WWII work that Mindell documents for us. It would be a fascinating exercise to go back to *The Tempter* and correlate its fictionalised people, events and institutions with their historical equivalents. I am sure one could get quite far. The only reason I have not done so for this review is that, as a novel, *The Tempter* is the most startlingly boring book I have ever read.

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REVIEWS

STORIES ABOUT THE TECHNOLOGICAL REMAKING OF THE WORLD

David E. Nye, *America as Second Creation: Technology and Narratives of New Beginnings*. Cambridge and London: MIT Press, 2003. Pp. 371. US$40 HB, $17.95 PB.

By Georgine Clarsen

David E. Nye, a professor of American Studies at Odense University in Denmark, is not afraid of grand narratives. Since 1990 he has produced an impressive number of books analysing the broad sweep of technological progress during the nineteenth and twentieth centuries in the United States. Nye builds on the work of his teacher Leo Marx in emphasising the centrality of technology to American national identity. Like Marx, he argues that in the United States the power of the machine was as much moral and political as it was technical. In *Electrifying America: Social Meanings of a New Technology* (MIT, 1990), Nye invoked the 'electrical sublime', a concept he further developed in *American Technological Sublime* (MIT, 1994), broadening it to include canals, railroads, dams, factories, bridges and skyscrapers. The studies traced how during the nineteenth century and early twentieth century, Americans extended a European notion of the sublime from the wonders of nature to the marvels of human technology. In contrast to the old world, Americans were quick to adopt an optimistic stance towards technologies, embracing them as instruments of democratic egalitarianism that not only enhanced people's material lives, but also improved their morals. As Nye tells it, the history of the American technological sublime had a 'rise and fall' trajectory, in which the celebration of technology declined in the second half of the twentieth century, in part through a fear of the destructive power of nuclear weaponry. It has been replaced by the far

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less grand 'consumer sublime', exemplified by spectacles of consumption like Las Vegas or the IMAX theatre at the Grand Canyon.

In his latest book, *America as Second Creation: Technology and Narratives of New Beginnings*, Nye revisits much of the same territory as his earlier studies, but from a somewhat different direction. Rather than organising his material around the perceptual notion of the sublime, Nye concentrates on the rhetorical practices by which technologies were incorporated into national life. Drawing on the work of Hayden White and Pierre Macherey, he considers the narratives that surrounded technologies during the nineteenth century as stories that enabled Americans to feel at home in their 'new' world. Key to his analysis is the notion of 'second creation', a term he takes from an anonymous article in the *New Englander and Yale Review* of 1843, in which human creativity was said to bring into completion the 'scant' and 'rudiment' beauty of nature. The first creation was God's, but it was a creation whose perfection was latent – it was unorganised, unfinished, and waiting to be improved by the hand of man (and of course here we are quite literally meaning 'man'). Nye argues that in the telling and retelling of second-creation stories, Americans in the nineteenth century presented radical transformations of the landscape as tales of inexorable progress, in which men's actions completed the order that lay dormant within God's creation. The narratives endorsed the white invaders' transformation of a space they imagined to be untouched and waiting to be brought into life, and reinforced the belief that Americans were a people distinct from those of the old world.

Nye's analysis constitutes a fresh take on the old doctrine of manifest destiny, where the American mission is conceived not in the familiar political, religious or ideological terms of creating and populating an empire of liberty, but as much more materialist project, in which technology is placed centre-stage. Or more correctly, it is not the technologies themselves that are the heart of his analysis, but the stories that have been told about them. Colonial narratives conceived of America in biblical and religious terms as the Promised Land, and second-creation stories did not emerge until a generation after the American Revolution, when former colonies were able to view themselves as distinct from their European origins. And although post-Revolutionary second-creation narratives have not entirely disappeared from American national life, according to Nye they had lost much of their power by the early twentieth century – 1910 to be precise.

Selected for close scrutiny are five key technologies – the axe, the mill, the canal, the railroad and the irrigation dam – which Nye declares most shaped American's understanding of their place in the new world during the course of the nineteenth century. The axe was a narrative of the solitary pioneer. It encroached on millions of acres of forest, translating surveyor's lines into forest clearings. At the centre of each clearing, of course, stood the nation's birthplace – a log cabin. Mills sprinkled along watercourses were the seeds of new communities. They promised a pastoral and democratic industrialisation that would avoid the 'satanic mills' of British coal-based manufacturing. Canals and railroads were national technologies, inserting individuals into an integrated market, and fostering massive growth in remote regions. Irrigation opened marginal land to full use, promising to turn millions of acres of 'wasteland' into productive land that was perhaps even superior to that of the east.

All this is familiar territory, eminently readable and well pitched to undergraduate teaching. But Nye does more than reproduce the standard narratives that surrounded these five technologies. His original contribution lies in drawing out four enabling assumptions that underpin those second-creation narratives, and hanging on them a multiplicity of counter narratives to the dominant stories. The four assumptions he identifies represented profound shifts from European perceptions, as well as from those of Native Americans. The most important was that organic and traditional systems of dividing up land were abandoned in favour of an abstract grid that imposed a geometrical order on the landscape. Centrally administered, the grid ignored the specificities of place, working to erase knowledge of prior ownership, and declaring that the land was a vacant commodity, standardised and waiting for exploitation. With this redefinition of place into space came three other foundational shifts – from regulated prices based on the idea of a just price, to free markets; from a psychology of scarcity such as that espoused by Malthus and Ricardo, to a confidence in natural abundance; and from a universe subject to mysterious spiritual forces, to a Newtonian order that was predictable and animated by forces assumed to be plentiful and expanding.

Nye weaves these unstated assumptions underpinning second-creation stories into a meta-narrative, which unifies the five technologies into a coherent trajectory of imagined progress. Each technology is more powerful than the one before it; they move from muscle power to mechanisation; from individual to corporate effort;

they follow each other in chronological order; and travel sequentially across the landscape from east to west. It is a meta-narrative that Nye admits was a common theme of American history texts right up until the 1970s, but his purpose is to make it less viable by opening spaces for counter-narratives to the dominant stories. He structures his book as a series of oppositions, with one chapter devoted to the standard second-creation narrative and the following to the counter-narratives expressed by those whom new technology disturbed or displaced, such as Native Americans, unionists, small farmers, women and early environmentalists. Nye demonstrates how each counter-narrative, although muted in comparison to dominant narratives, began to emerge almost simultaneously with them. The land was stolen; surveyors' grids denied local specificity; free markets had been shown to be vulnerable to fraud, monopolies and corruption; resource abundance had proved illusory; and following the development of the theory of thermodynamics, energy was entropic rather than unlimited and expanding.

In presenting these narratives, Nye draws upon all the familiar names in nineteenth century American technological discourse – Emerson, de Tocqueville, Hawthorne, Twain, Whitman, Olmstead, Thoreau, Turner, Faulkner, Melville, as well as a catalogue of boosters, engineers, government reports, newspaper proprietors, fiction writers, and figures of popular culture. The multiple narratives presented by Nye are seductive and beguiling stories, rich in their research and references, but they leave a sense of unease. For the counter-narratives, as Nye acknowledges, formed a less powerful tradition than the foundation stories they attacked. They were based on detailed local knowledge rather than general principals, and largely spoke to the interests of those on the margins of the national project who had little control over the technologies of development or the media that celebrated them. It remained much more compelling for Americans to believe that their second-creation was in harmony with God's creation – that land had been 'lying idle', rivers were 'lazy', and their waters 'wasted' as the discharged into sea.

Nye's stated intention is to encourage Americans to reject second-creation stories outright by revising the assumptions that underpin them, and giving up fantasies of human domination of the natural world. In their place he would have us imagine technological narratives based on stewardship, which recognise environmental limits and our co-dependency on the natural world. But like the counter narratives of the nineteenth century, Nye's 'stories about stories about the technological remaking of the world' are less than compelling as long as they remain rarified and divorced from concrete social relations. In his accounts women, African-Americans, Hispanics, Native Americans and struggling farmers are inclined to be shadowy, generic figures. His book will work towards achieving his stated intention to the extent that it encourages a proliferation of detailed empirical studies that bring to life the counter-narratives, and show that things might (still) be otherwise.

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THE FIRST MAN ON THE PILL

Nelly Oudshoorn, *The Male Pill: A Biography of a Technology in the Making*. Duke University Press, Durham, NC, 2003. Pp. xi + 306. US$21.95 PB.

By Ivan Crozier

For many years now there has been talk about a contraceptive pill for men. This proposed pill has often graced the pages of magazines as a revolution in human reproductive biology, a re-dressing of the power balance that gendered The Pill as a female technology. It has been feted as a way of men taking responsibility for family planning, and a share of the risks associated with taking a regular dose of chemicals. The question remains, after 30 years of fanfares, why can I not go to my doctor and have it prescribed as an everyday pharmaceutical? It is the process leading up to this situation – the development, testing, politics, publicising, and changing cultural attitude towards the male pill – that Nelly Oudshoorn addresses in her outstanding book on its development.

The first important change that put the need for a male contraceptive pill on the international map was political. It was also non-Western. In both China and India there was a sustained effort to develop a male equivalent of a regular and reversible method of family planning that fitted with the cultural needs of the users as well as the political climate of population limitation. The search for regular and reversible male contraception also became an issue for the World Health Organization (WHO), which set up a task force – the Human Reproduction Program (HRP) – to address issues of population control in conjunction with the invigorated interest in family planning by various funding organisations. The WHO's role in this project was one of organisation, bringing together various international researchers and institutions from India, Australia, Europe, the USA, and Asia. It set up multi-centre testing, co-ordinated research and called regular meetings to disseminate findings.

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Two particularly important issues relate to the WHO's co-ordination of the search for the male pill: the science behind the development of the drug, and the relationship with the pharmaceutical industry which would eventually need to take over production and distribution of the product – and thus incur risks associated with the development of new drugs.

One of the key issues facing scientists was testing for success of the male pill. This question revolved around the criteria of what is a 'good' pill. In China, a 'good' test was one on which 99.89% of users did not get their partners pregnant. But other problems associated with the pill which were not deemed problematic in one system were not appropriate for the West. Problems such as diarrhoea, heart failure and sterility were contra-indicators of a successful drug. Later problems that would be faced by reproductive scientists included a loss or lowering of male libido, and an inability to get an erection. Both of these problems were significant, for as one of Oudshoorn's respondents put it, "men don't take the Pill to refrain from sex" (p. 107). More complex work in the 1990s found that the use of androgens would raise libido while not affecting the lowered sperm production offered by hormones that worked as a contraception. Further, there was much debate about what the successful performance indicators should be: the WHO resisted the notion that pregnancy would be a good indicator, for it placed an unacceptable burden on the non-user of the drug. Other options that could indicate a working contraception included lowered sperm mobility and decreased sperm production, but these were occasionally associated with the problems of decreased sexual performance which could be argued to defeat the purpose of a male pill – cultural issues of masculinity had to be taken into account as well as chemical performance if the pill was to become a viable option for drug companies to take up.

Various other factors contributed to the general resistance of large corporations to jump on the male pill road show. While the development of the female oral contraception in the 1950s meant that advantage could be taken of 'passive' groups – such as Puerto Rican women who were able to be used as a test population for the drug – testing and distribution of pharmaceuticals is nowadays a much harder venture. Huge investments and care must be taken in order to avoid mass torts/class actions. In other words, risk is not only there for the person taking the drug, but for the company providing it. Degrees of certainty – medical and market – are necessary before a new drug is taken on by the industry.

Other important influences on the development of the male pill came from Feminist critiques of the politics of the female pill. Feminists had long complained that women all too often became the repository for chemicals associated with various health problems. Sharing the risk with men was not, however, their only concern. Male contraceptive pills could possibly disempower women by diminishing control of their fertility. Such shifts in reproductive responsibility also raised issues of trust as a man who failed to take the pill would not suffer the same consequences of pregnancy, or choices about abortion, as a woman.

Oudshoorn has also identified new social movements involved in raising male awareness as significant factors in the re-thinking of contraceptive practices. For a considerable time, men have been identified as the worst of all possible health-care users. Culturally, men are less likely to ask for health advice, to go for regular check-ups, and to seek information on new products. These issues have changed somewhat, however, with both the general acceptance of male sexual problems that was brought about with the introduction of Viagra. Further, wide discussion of HIV has made sexual health a more prominent issue for men. Men's clinics, which have opened up in large cities, have become spaces for the development of issues germane to the cultural changes necessary for introducing male contraceptives. These spaces have challenged men's ideas about their bodies and about medical intervention, making them more akin to the established feminine conceptions of medically mediated bodies. This has involved a series of developments of male cultural conceptions rather than a wholesale feminisation of masculine notions of good health. For a male contraceptive to be developed, subtle changes have to take place concerning male bodies and masculinity. Thus it has often been in men's clinics that volunteers for trials of various male contraceptive compounds have been recruited.

One final site of contention over the male pill has been in the media. Newspapers and magazines have been constantly floating the idea that men will soon be on the pill. These same sources have been one site for doctors to access a market of potential users who could be incorporated into various long-term trials. But more significantly – and very well analysed by Oudshoorn – the potential users of the male pill were 'tested' in these same newspapers. "The testing in the media focused almost exclusively on the cultural feasibility of the new technology" (p. 205). Of particular note was the way that the idea of men subjecting themselves to an injection was addressed in the media.

It was suggested that men would not be able to remember a pill every morning, but also that they would be wary of having a regular injection in their bum: "users were primarily represented as over-sensitive, unreliable men who would never use a contraceptive injection" (p. 206). The press therefore had two distinct impacts upon the development of male contraception: it circulated information about the new technologies, while also reinforcing cultural stereotypes about patterns of use.

Oudshoorn's approach to the above problems is well grounded in STS theory. She has adopted a version of Social Construction of Technology (SCOT) which she has melded together with the parts of Actor Network Theory (ANT) that have appealed to her. This SCOT/ANT hybrid allows her to attend to issues such as the 'path-creation' of technology while also elaborating upon networks of experts, institutions, social groups, techniques, and relationships between technologies and their users. Oudshoorn draws upon human action as constitutive of technological paths and networks; her brilliant empirical work, summarised above, allows us to see how the certain struggles around the development of a contraceptive pill for men relied on these human and non-human factors.

Beyond both SCOT and ANT, Oudshoorn emphasises cultural aspects in addition to the social. She focuses upon the way that factors such as masculinity need to be addressed to fully position any developments within male contraceptive technologies. This emphasis is drawn partially from Judith Butler's well-known work on gender performance. To adapt a new contraceptive method to a different gender involves learning to perform certain masculine roles differently, to learn a new repertoire that involves hitherto feminine scripts, such as taking a pill each morning, or going to the doctor regularly to discuss contraception and be injected. In this, men desiring to take a male contraceptive pill have to re-evaluate their practices in the light of the new technologies available. As Oudshoorn puts it, "The development of new contraceptives for men thus requires the destabilization of conventionalized performances of masculinity" (p. 16). It is worth noting that Oudshoorn does not rely heavily upon Butler beyond stating her affiliation. While the language of 'performance' is present, this is not a work that draws upon re-worked Austinian concepts. Rather, Oudshoorn enriches her SCOT/ANT hybrid with her understanding of culture as performed and local. In this, she offers more to this detailed, empirical narrative than any work that allies itself with Judith Butler that I have seen.

When combined with her outstanding empirical research, Oudshoorn's biography of the male pill is a brilliant example of applied STS theory and deserves a wide readership.

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MODERNIST MIND GAMES

Alex Owen, *The Place of Enchantment: British Occultism and the Culture of the Modern*. Chicago: Chicago University Press, 2004. Pp. xiv + 355. US$30/£21 HB.

By Ivan Crozier

The occult is a problem that has not been given deserved attention in the history of ideas in the Modernist period (1880s to 1930s). While we may, on hearing the word occult, conjure up images of witches' covens dancing on bald mountains, or darkly-clad students blaming the non-appearance of essays on the vanishing of their favourite black cat (true story), the fact remains that occult practices were very much a part of the Modernist movement. Indeed, the occult has many affinities with things like the birth of psychology, the development of Modern religious sensibilities, and the critique of hard scientistic attitudes. Alex Owen's marvellous new book does much to unveil the occult as a part of this changing world view.

The occult became popular as a point between evangelical religion and the hard-core of scientific naturalism, both characteristic of late-Victorian Britain. Its entry was made possible by a widespread interest in spiritualism – in the form of mediumship. Spiritualism meshed well with existing Christian belief: both held ideas about an afterlife, and, to some extent, that it is possible to receive messages from this spiritual arena (whether by stone tablets or bodily take-overs). In time, those interested in other areas of the occult, especially in magic and Eastern religions, moved further from Christian spiritualism. They held a sceptical attitude towards organised religion, and towards matters of faith in general. But they were likewise wary of the materialism espoused by John Tyndall and other scientists. That said, the spiritualists adopted aspects of Modern science to their own interests: especially the 'experimentalism' that underlay scientific

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scepticism, and the 'naturalist' approach towards the mind – treating it as an object that was worthy of careful and intense study. Such an attitude was not the province of occultists alone. At the same time as this, the Society for Psychical Research at Cambridge became interested in many of the same issues, taking spiritualists to task in numerous debates.

Various features of the occult tie-in closely with other movements associated with Modernism: conscious and unconscious exploration of the mind; an extension of ideas of personality; an interest in other religious practices. The Bohemian life also had much to offer occultists, particularly drug experimentation and sexual freedom. All of these interests were often shared by those involved with the nascent psychoanalytical movement, and Owen shows the cross-fertilisation and resistances between these two groups adeptly.

Occult practices eventually became highly structured. There was a rigid structure of progression for the neophyte, based on scholarship, memory, and commitment. There was a high level of secrecy surrounding the distribution of knowledge, and a series of orders of attainment that separated the hierarchy into three circles. Code names were used, mostly in Latin. Initiation rites were practised, a secret vocabulary was developed, and messages were written in cipher. In all, it was everything one could wish for in a secret occult society of the High Victorian period. But Owen's analysis of this organisation goes far beyond a list of names and roles. It includes an exploration of challenging gender roles and sexual mores encouraged by the occultist movement. Exploring aspects of sexuality in particular was, for those so inclined, a means to further spiritual enlightenment.

The most compelling aspect of Owen's analysis is her adherence to the symmetry principle. She does not hold up science – or even psychology – as a yardstick against which to measure the successes or failures of occultism. Nor is this book an investigation of the author's beliefs or own cultish modes; rather, it is a thick description of various practices that were compelling for many great thinkers of the Modernist period. As such, we are shown with great detail the kind of investigations that were employed by occultists, and are introduced to their beliefs in a sensitive way. This approach draws the best from Owen. While her cultural and intellectual contexts and her analysis of social factors such as sex and gender are sound but unremarkable, her main thrust in this book – the chapters on 'Modern Enchantment and the Consciousness of Self', 'Occult Reality and the Fictionalizing mind' and 'Aleister Crowley in the Desert' – are other-worldly in their excellence.

'Modern Enchantment and the Consciousness of Self' allows the reader to see what occultists actually do. To this point of the book, we knew that they dabbled with various forms of self-reflection and deep introspection. It turns out that a number of means are possible and the point of these researches was to follow the disappearance of the self as a transcendental entity. The Lockean self dissolved as the medium took off on a bout of astral travel, for which all of the study and incantation had prepared him. This personality research was fundamental to the work of occultists. It was not a simple indulgence of pleasure, like a hedonist taking ecstasy before the wonders of *Le nozze di Figaro*; it was serious experiment that would unveil the secrets of the universe.

The premier English occultist was Aleister Crowley, the intelligent, educated, bisexual, womanising, drug-taking figurehead of the occult as it moved from the Golden Dawn into truly terrifying domains of a new organisation: the Magical Order of the Silver Star. Crowley has kept a cult following today. He wrote various documents that vividly describe his experiences in the occult (*The Book of Law*, 1904; *Diary of a Drug Fiend*, 1922; *Magic in Theory and Practice*, 1929; and the posthumous *Confessions of Aleister Crowley*, 1969). These are drawn upon powerfully by Owen. The chapter on Aleister Crowley's time in the desert was like reading a Paul Bowles novel rather than a history book: it has all of the horror, the angst, the searching. Described is a 1909 journey into the Algerian Sahara in the search for enlightenment and experience. To quote Owen, "The episode ... marks the point at which Crowley crossed the Rubicon in a number of senses, but the experiment was not straightforwardly self-serving, as much of his magical work was to become. Nor did it represent simply the indulgence of an exoticized and outlawed sexuality. What happened in the desert was the result of a serious, if misguided, attempt to access and explore a centuries-old magical system, and it represented an intense personal investment in the pursuit of magical knowledge." (p. 187). The crux of the book, for this reader, was the moment where Crowley and his acolyte, Cambridge poet Victor Neuburg, map out a magical space in the sand and summon forth beings from the astral plane. In Crowley's words, "I sacrificed myself. The fire of the all-seeing sun smote down upon the altar, consuming every particle of my personality" (p. 198). He experienced voices, apparitions, bodily take-overs, and a confrontation with "Choronzon, the mighty devil that inhabiteth the outermost Abyss" (p. 199). Crowley and Neuburg entered into a struggle, leading to brutal sex in the desert sands: Neuburg sodomised Crowley as "a homosexual rite offered to the god Pan" (p. 198). Owen captures these moments lucidly, showing with true scholarship the extent to which occultists devoted themselves to their art.

The aftermath of this experience is Crowley's descent into popular icon. He is famed for his adoption of the name 'The Beast 666'. He held wildly elaborate 'sex magick' orgies at his Italian villa, from which he was expelled by the fascists (see p. 218 for details). He died, lonely, in a boarding house in Hastings in 1947. After his death, he made the cover of The Beatles' *Sgt Pepper's Lonely Hearts Club Band* (1967; second from the left in the back row), and was voted number 73 of the top 100 Britons in a recent poll. But the Crowley given us by Owen is not simply a freak who smoked cannabis and conducted orgies under the motto 'Do What Thou Wilt'. Rather, Crowley is understood as the most extreme of a rational set of actors in search for a deeper understanding of human nature.

To draw home the point that this book is not merely a celebration of occult excess or experimentation, the epilogue remarkably situates the main issues of the occult in the history of the "dilemmas of modern existence" (p. 238). *The Place of Enchantment* is primarily concerned with the investigation of subjectivity, and the location of an occult subjectivity among the newly emerging forms of human sciences that focused on such issues: psychology, sociology, psychoanalysis. The occult in Owen's study opened up a specific knowledge of the human subject by stressing experience and mental awareness, and by creating a regime through which these may be accessed and understood. It is the reliance upon reason and not faith that makes this project Modernist – just as the sociological, the psychoanalytic and the psychological subject were established through the application of a specific reasoning to experience. Like other Modernist human sciences, the occult required a new model of investigation. It required a new ontology, a new audience. By reflecting on the occult, Owen helps us to reflect on the development of the human sciences in the period of reconstruction that affected most significantly the newly emerging forms of subjectivity that the West would experience for a century to follow. The result is a book of profound insight and relevance to the history of the human sciences.

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SCIENCE LEGITIMISED?

Francis Remedios, *Legitimizing Scientific Knowledge: An Introduction to Steve Fuller's Social Epistemology*. Lanham and Oxford: Lexington Books, 2003. Pp. xii + 143. US$55.00 HB.

By Piet Strydom

As founding editor of a journal and author of numerous books and articles dealing with social epistemology, Steve Fuller – although only in his mid-forties – has already succeeded in making a significant contribution to both philosophy and the social sciences. A book length introduction to his work, therefore, does not come as a surprise. As the first such venture, Francis Remedios' *Legitimizing Scientific Knowledge* does not disappoint. It brings clearly to the fore those features of Fuller's social epistemology which make it both unique among analytical philosophers and attractive to many a European philosopher and social scientist – its reflexively modern, humanistic, normative, political, democratic, and critical orientation.

Having spent some ten years studying the literature on epistemology, philosophy of science and social epistemology, Remedios is able to locate Fuller in the contemporary social epistemological field of contestation in a clear and comprehensible manner, fitting to an introductory work. By the same token, valuable light is shed also on different varieties of contemporary naturalism. Fuller's 'politically oriented social epistemology' emerges well profiled as being flanked by David Bloor and Barry Barnes' 'interest-oriented social epistemology', on the one hand, and Philip Kitcher and Alvin Goldman's versions of 'truth-oriented social epistemology', on the other. Considering the formal importance of this triangular relationship, however, the reader is left wondering why the structure of the book does not reflect it more strongly: Why is the treatment of Kitcher

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submerged in the text and Goldman banned to the conclusion rather than the truth-oriented position being given its own chapter relatively early in the book?

Remedios' overall framing, which explains the title of the book, is his most important achievement for it makes possible not only an immanent but also a transcendent understanding of contemporary social epistemology. Since Kant, traditional philosophy sought to provide a strictly epistemological legitimation of science, but as a consequence of Thomas Kuhn's questioning of the basis of the standards of scientific rationality and knowledge, a wide-ranging debate erupted in the philosophy of science about this central issue. The 'legitimation project' – as Remedios calls it – to which this debate gave rise is the philosophical attempt to fill the resulting vacuum. The general response, which attracted naturalists in particular, took the form of a socialisation of epistemology in the sense of shifting the emphasis to the social dimension of knowledge and the processes whereby it is produced. From this common platform then emerged the different competing versions of social epistemology. In the foreword to the book, Fuller actually applauds Remedios' framing of the issue for allowing the contrasting of both his political position with interest- and truth-oriented social epistemologies and his materialism with postmodern philosophies of science which reject the legitimation project, such as Joseph Rouse's.

Over and above its hermeneutic disclosing power, however, this framing simultaneously exposes the limits of both Remedios' account and Fuller's approach. Throughout the book, it keeps the critical reader aware of the fact that whereas Fuller claims that his social epistemology is materialist in a sense close to Marxism, the material conditions of the processes of scientific knowledge production are never dealt with. Both Fuller and Remedios consistently understand the "crisis in the legitimation of science[,]. . . probably the biggest problem . . . facing our society today" (p. x), as having been created by Kuhn. Yet it is well known for decades that the 'legitimation crisis' (Jürgen Habermas, *Legitimation Crisis*, London, 1976) that critical social scientists and concerned citizens have in mind in the case of science can be traced to the negative consequences and side-effects of scientific knowledge, especially in the form of the ecological crisis. For all his failings, Robert Merton knew already that science would become a serious object of study only once it had become a social problem. In the light of these observations, Fuller's objection to disinterested, normatively anaemic approaches devoid of any practical significance applies to his own politically oriented social epistemology: "we're just spinning our wheels" (cited p. 73). This problem undoubtedly raises a question also about Fuller's understanding of his own eliminativism, i.e. the manner in which he proposes to absorb science in society.

Remedios gives Fuller's work an overwhelmingly sympathetic treatment, mostly keeping to the letter of the philosophical debates, but he does point out certain problems along the way. Perhaps the most important is Fuller's position on the governance of science which, based on a retrieval of the old distinction between the contexts of discovery and justification, separates privately funded basic research from state-funded testing and distribution of privately produced knowledge. While Remedios registers the impossibility of regulating morally reprehensible science under such a dispensation, he neglects to probe deeper into Fuller's 'civic republican' political theory. Would an alternative political theory, say a deliberative democratic theory which by no means excludes participation yet mitigates the excesses of republicanism (e.g., Sheila Benhabib in Craig Calhoun, *Habermas and the Public Sphere*, Cambridge, 1992; Jürgen Habermas, *The Inclusion of the Other*, Cambridge, 1994), not be a more appropriate and sensitive guideline? On other occasions, however, Remedios' sympathetic and literal approach leads him to gloss over apparent problems in Fuller's social epistemology. One of these is Fuller's eclecticism of combining a constructivist conception of the natural sciences with a realist approach to the social sciences within the framework of a new unified metascience. Particularly problematic here is the concurrent reduction of the social sciences to a nomological sociologism that few contemporary social scientists, and by no means only anti-naturalists, anti-realists, interpretativists and postmodernists, would find unacceptable. In my view, a more promising direction is a pragmatic realism, rather than a sheer instrumentalism, grounded in a weak naturalism which sees cognitive forms arising from natural historical processes open to natural scientific study, as setting broad parameters for practices within the framework of sociocultural forms of life organised in their own communicative terms (see my *Risk, Environment and Society*, Buckingham, 2002).

Another significant problem passed over by Remedios, the lack of an adequate social theory in Fuller, is signalled by various indicators, but particularly by debates appearing spurious and at times even somewhat lame. Remedios reviews and elaborates on debates which by their entanglement in traditional dualisms display a disregard for social theoretic possibilities beyond such a cul-de-sac.

They include the debates about whether Fuller defends a 'minimal' or a 'maximal' social epistemology, and about whether Fuller's knowledge policy analysis represents 'thin' (i.e., purposive-rational *à la* Elster) or 'thick' (i.e., psychologically rich *à la* Geertz) agency. Obviously lacking in both cases is a social theory that can account for the dynamic formation of structures, from the micro- to the macro-level, making possible the building up, articulation, development, institutionalisation, diffusion and employment of knowledge. It is noteworthy that Fuller himself attaches high value to what could be considered cognitively important matters such as processes of knowledge production, the need to know our knowledge processes better, how knowledge comes to be socially accepted knowledge, and whether and how 'people collectively learn'. In the same vein, he also substitutes Kuhn's paradigm with the social movement model drawn from cognitive sociology. This suggests that a possible solution lies in what may be called cognitive social theory. In its contemporary version, the latter covers a wide range of issues which contain resources allowing one to steer clear of debilitating traditional figures of thought – e.g., dynamic cognitive processes (Niklas Luhmann, *Die Wissenschaft der Gesellschaft*, Frankfurt, 1992; Jürgen Habermas, *Truth and Justification*, Cambridge, 2003), categorisation or classification and cognition in action (Anni Borzeix, Alban Bouvier and Patrick Pharo, *Sociologie et connaissance,* Paris, 1998), cognitive structures of different levels and scope (Strydom, *Discourse and Knowledge,* Liverpool, 2000), sociocultural cognitive orders (Gerhard Schulze, *Die Erlebnisgesellschaft*, Frankfurt, 1992), discourse or collective argumentation and collective learning (Max Miller, *Kollektive Lernprozesse*, Frankfurt, 1986), generalisation or the diffusion of collective representations or cognitive structures (Ron Eyerman and Andrew Jameson, *Social Movements: A Cognitive Approach*, Cambridge, 1991), orders of justification conceived as pragmatic regimes of action (Luc Boltanski and Laurent Thévenot, *De la justification*, Paris, 1991) or cognitive rule regimes embracing descriptive, prescriptive and evaluative rules (Klaus Eder, *The Social Construction of Nature*, London, 1996), and so forth. Yet Fuller consistently objects, with support from Remedios, that the cognitive approach commits the fallacies of 'decomposition' and 'division'. Presupposing the individual/collective and cognitive/social distinctions, however, this is an unmistakable traditionalist response, which falls short of available social theoretic solutions.

In sum, then, Remedios has rendered students and scholars alike a commendable service by mapping the social epistemological field and providing grist for the philosophical mill. It does not detract from this achievement to point out that his copy-and-paste style, which certainly does not match the linguistic, argumentative and evaluative dexterity of Fuller, induces a reading experience at times invaded by a feeling of repetition, which is paralleled in turn by some conspicuous copyediting and production failures (e.g. pp. 13 and 80).

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A VIEW OF THE EMERGENCE OF GEOLOGY AS A NEW SCIENCE

Martin J.S. Rudwick, *The New Science of Geology: Studies in the Earth Sciences in the Age of Revolution*. Aldershot and Burlington: Ashgate, Variorum Collected Studies Series CS 789, 2004. Pp. xvii + 163. £59.50 HB.

By David Oldroyd

Martin Rudwick is, by my book, the scholar who has made the most significant contribution to the study of the history of geoscience since it emerged as a recognisable (albeit small) field in the second half of the twentieth century. Starting as a palaeontologist at the Sedgwick Museum in Cambridge, he was handily placed to the then emerging History and Philosophy of Science department and began to interest himself in its work, first giving lectures there on the history of palaeontology and eventually committing himself to a full-time career as a geohistorian. Since that time, he has held chairs at the Free University in Amsterdam, Princeton, and San Diego, and having returned to England he is now a research associate at his former department in Cambridge.

In 2003, I had occasion to write an essay on the historiography of nineteenth-century geoscience, and it was remarkable how many of the most innovative ideas and approaches to this historiography came from Rudwick's work. To a considerable extent, he dominated the field. Now we have the first of a pair of Ashgate Variorum books, this one presenting fourteen of his papers. The sister volume is scheduled for publication in 2005. We are also looking forward with keen anticipation to the appearance of his matured thoughts on the question of the origin of geology as a science, and in particular the role of Georges Cuvier in its emergence as an *historical* science. (This will appear as a Chicago University Press book, and I assume that it

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will both synthesise and expand upon the work published in the volume under discussion here.)

Rudwick has long been interested in the question of 'visual imagery' in geoscience, its role in the cognitive processes of geologists, and the manner of presentation of their ideas; also the social formations of geoscientists. Such matters are well to the fore in his Variorum I, but there are also contributions on the history of ideas about time-scales, with material showing how Ussher-type chronologies gave way to modern ideas of geological time and history. Rudwick shows, for example, how figures such as Jean-André de Luc (who was pilloried in Charles Gillispie's paradigmatically whiggish *Genesis and Geology*, 1951 – a nonetheless interesting and influential book) in fact gave interpretations of the processes of weathering and deposition that make much sense to us today, despite being situated within the context of a 'binary' history of ante- and post-diluvian epochs. In this, as for other issues, Rudwick is always an opponent of whiggery and historiographical anachronism.

However, I suppose the principal thesis that Rudwick would wish to uphold, so far as his Variorum I reveals, is that *Cuvier* – though later called a 'catastrophist' and sometimes represented as unscientific by Lyellians – was the pivotal figure in the emergence of geology as an historical science, not just because of his commanding position in French scientific life but because he 'historicised' geology, and showed how one could read the Earth's history from the 'records' left in the rocks, or its 'archives'. (Gabriel Gohau in France has also made much of the notion of 'geo-archives'.) Seen in this light, Cuvier's work is presented as the key to the emergence of *geology*, as opposed to the earlier 'theories of the Earth', which Rudwick correctly regards as substantially different cognitive enterprises.

In support of his thesis, Rudwick gives considerable attention to the aforementioned matter of 'visual imagery'. He tracks the changes from eighteenth-century 'geognostic' maps to the early nineteenth-century stratal maps and sections, which conveyed both structural and historical information. (They revealed the historical order of deposition, and could also be linked to the climatic and physical conditions that obtained at different places in the world at different times.) William Smith has often been credited with being the progenitor of such maps, but, as Rudwick cogently points out, Smith did not advance conceptually to the production of a geohistorical map of Britain. Rather, his map (1815) provided a means for the 'reading' (interpretation) of the three-dimensional structure of the country (aided by his sections, produced in symbiotic association with his maps). Rudwick maintains that Smith's maps and sections were 'geognostic' in character, in keeping with the tradition of mining engineers and surveyors of which Smith was a part, rather than geohistorical productions, such as the map of the Paris 'basin' of Cuvier and Brongniart (1808/1811).

In support of this thesis, Rudwick points out (in a footnote to the work of Rachel Laudan) that in the keys to his maps Smith sometimes shaded his colour boxes more intensely at the top, and sometimes more at the bottom. This may suggest that, while his well-known method of shading assisted the depiction of the three-dimensional geological structure of Britain, the temporal order was less important to him than it was for Cuvier and Brongniart, with their concern for the 'conditions of existence' at the time of the strata's deposition (fresh-water, marine, or whatever).

However, against this one may now point to a geological map of part of Derbyshire produced for Joseph Banks by Smith's friend John Farey, also from the surveying and engineering community. Dated 1812/1813, it was found by Hugh Torrens in California, of all places and has been reproduced by him in: R.E.R. Banks *et al.* (eds), *Joseph Banks: A Global Picture*, 1994. It raises some questions about the distinction between Smithian and Cuvierian maps that Rudwick sought to make back in 1976, for Farey's production can hardly be distinguished from many mid or late nineteenth-century maps. On the other hand, at the time of its compilation it was represented as a 'mineralogical map', and Farey is known to have consulted the Cuvier and Brongniart map and memoir (1811) by 1812. Moreover, Farey was unable to get his map published in Britain. In any case, I would not claim that Farey's interests were really geohistorical in the sense of Cuvier (and Rudwick).

Passing on from that issue, I find what Rudwick has to say about geological illustration intensely interesting. He discusses the techniques of metal engraving, lithography, and woodcuts, and the kinds of information that could be conveyed with the different technologies (e.g., through the use of various kinds of shading). More than this, with Rudwick as our guide, we can follow the preparation of Cuvier's memoir illustrations, from his extraction of fossils or dissection of specimens, their drawing, and the transformation of his drawings by professional engravers into published pictures (with mirror-image reversals in the process). On occasions, different scales were used for the depiction of different fossil types, so that their homologies were more evident when their representations appeared in the same plate.

Moreover, the representations of fossil vertebrates' skeletons could be brought together as "proxy specimens" (IX, 57) within the pages of a book or books, and thence by their publication and distribution they became "mobile" (*ibid.*). Cuvier's knowledge – produced from the work of his collectors, his own hands as a dissector and artist, the engravers, and the publishers – became disseminated, and to a considerable extent was accepted as authoritative. Thus Rudwick pays attention to both the 'internal' cognitive processes of the comparative anatomist, and the technical, social, and economic processes whereby his knowledge was transferred to other workers. Cuvier's books, Rudwick suggests, became a "paper museum of fossil bones" (part of title of Paper IX).

In keeping with his visual imagery interest, Rudwick's papers are themselves generously illustrated, with items from major or minor 'geo-texts', and some ingenious diagrams of the author's own design: for example, one that represents the development of concepts of geo-time or geo-history; a diagram representing the "emergence and historical development of the visual language of geology" (V 178); and another showing the "social and cognitive topography of geology in the 1830s (XIII 197).

The reader will find insights into the history of geology on almost every page, prompting this reviewer to say to himself (in an unoriginal fashion) "how stupid not to have thought of *that*". So: many historians of science will turn to the anthology with satisfaction and profit for a conspectus of the author's work over the years (plus one essay original to Variorum I). We look forward to Variorum II in 2005, which will presumably focus on Lyell and Darwin. Also in 2005 there will be the Chicago *magnum opus* on the origin of geology as an historical science, with – I imagine – Cuvier presented as the key figure. But that raises a query, already prompted by Variorum I: if Cuvier was *the* major figure in early French (or world) geology, why were the founding fathers of the *Société géologique de France* (Constant Prévost, Ami Boué, Jules Desnoyers), which was established in 1830, anti-Cuvierians?

But this need not alarm Rudwick. He would, I think, simply say that there was more to Cuvier than his 'catastrophism'. The question of thinking about the Earth's history in a manner analogous to human history, using 'archives' both for geohistorical work and for human historiography, in order to comprehend what happened in the past and former conditions of existence, was the mark of the new science of geology.

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DEFEATING THE INTELLIGENT ZOMBIE

Niall Shanks, *God, the Devil, and Darwin: A Critique of Intelligent Design Theory*. New York: Oxford University Press 2004. Pp. 296. US$29.95 HB.

By John S. Wilkins

T.H. Huxley once said "life is too short to occupy oneself with the slaying of the slain more than once". But what about the Undead? Zombie arguments that pop up again and again long after the *coup de grace* has been delivered on the battlefield? The so-called Intelligent Design (ID) movement reinvents Paley and denies Darwin once again, and yet, as Niall Shanks shows here, this movement adds nothing of interest or worth. It is truly a zombie argument.

It is a sad reflection on the state of public knowledge of science that the intelligent design movement should need a book like this one, so long after the beast was slain. But still, it continues to be asserted and sold, if not by scientists, then by those with a religious aim and a political agenda. Others have discussed the politics and the history of the 'wedge' that the intelligent design advocates are trying to insert into public discourse. The virtue of this book is that it delivers a fairly complete account of the *ideas*, at a level that an intelligent non-specialist can approach with no more than slight effort.

However, one may fairly ask if giving such intellectual recognition to a zombie argument is to provide it unwarranted kudos and standing. Proponents of ID in biology, or IDevotees as I call them, take the slightest recognition by the academic community as evidence they are shaking the foundations of Darwinism, when in reality they are little more than a sideshow to the main carnival of science. Still, this book is needed, despite some very good recent publications, as a resource to offer to an honest and relatively intelligent student or friend who might be thinking there is something to ID but who lacks the resources needed to see it for the politically-motivated attempt to gull the rubes that it is.

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Shanks begins with an excellent review of the history of the Argument from Design, from Aristotle and Cicero, through Aquinas' Five Ways version, and through to the influence of the mechanistic metaphor used by Harvey and Descartes, and the design implications of that metaphor. Briefly reviewing Newton's use of intelligent design explanations, he turns to Paley, the last and perhaps definitive of the Arguers from Design. Shanks discusses the well-known watch on the heath, before turning to Kant's notion of teleology, Hume's counter arguments to natural religion, and Adam Smith's notion of self-organisation in economic markets.

The second chapter covers Darwin's views on religion, morality and design, his relation to Lyellian uniformitarianism, and of course, the role and logic of natural selection. A section covering the development of evolutionary theory after Darwin, and the nature of evolving genes, is less satisfactory. Shanks unfortunately refers to genes as 'bits of information', which I consider plays into the hands of the IDevotees, in particular William Dembski, but this is a judgement call – after all, Dawkins also does this. Interestingly Shanks refers to a G–C and A–T base pair as the 'bit', which is new to me. Previous attempts to give an information-theoretic account of genes tend to discuss the information content of codons. Although so far as it goes this discussion, which includes the nature of species, is correct enough, I wonder at its direct relevance to the naïve reader. There is no requirement, for example, that one adopt Templeton's cohesion concept of species in order to defend evolutionary biology against ID. The discussion of genes as machines, however, ties in neatly to the previous chapter.

The third chapter is where the rubber hits the road. Here Shanks begins by discussing the ordinary creationist misunderstandings of thermodynamics in what is perhaps the best popular but un-dumbed introduction to the topic I have read. This chapter alone makes the book worthwhile, and is more widely applicable than just the context of creationism or ID. An excellent discussion of Bénard cells as an example of self-organization of complex structures rounds it out.

The fourth chapter covers the claim made by IDevotees that science as practised by 'naturalists' excludes the action of non-natural causes by fiat. Shanks explains the difference between *methodological naturalism* and *philosophical* or *metaphysical naturalism*, and shows how the ID position is analogous if not directly derivative of the vitalism of old. He considers an actual attempt to show that a supernatural cause – prayer – has beneficial causal powers in medical recuperation, and argues that the case has not been demonstrated, but is not ruled out by good science. He also shows clearly that statistical significance is something Christian apologetes do not understand in this case.

The fifth chapter covers Michael Behe's claim that natural selection cannot produce 'irreducible complexity', and William Dembski's claim that it cannot produce 'specified complexity'. Demonstrating some theoretical and actual ways that it clearly *can*, Shanks undercuts the foundations of ID by using the work of A.G. Cairns-Smith a decade earlier than Behe's book, in which Cairns-Smith posed, and resolved, the irreducible complexity problem before ID ever got hold of it. The watch returns here, and the so-called 'analogy' between watches and living systems is effectively demolished. SETI, a favourite 'example' of IDevotees, is also turned to better use.

In the next chapter, Shanks turns to more general, cosmological, arguments for design, in particular those of the Anthropic Principle of Barrow and Tipler. The opening sentence of this chapter, in which the authors of the book of *Genesis* are referred to as 'ancient Middle Eastern sheepherders' is unnecessarily off-putting for the Christian or Jewish reader. The authors were more probably urban scholars in sophisticated Babylon than shepherds. But overall the chapter traces the threads of this recent pop-philosophy 'challenge' admirably, and the conclusion, that anthropic coincidences do not force us to a conclusion of design, is well argued.

The book is to be commended, particularly to undergraduates who may be discussing ID in the context of the philosophy of biology. It is, in itself, a polemic work, but Shanks wears his convictions on his sleeve and that too is a useful discussion point. Despite being an atheist, Shanks concludes that no intelligent believer needs to believe in intelligent design to the exclusion of modern biology or cosmology.

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ALFRED RUSSEL WALLACE, MAN OF THE HOUR

Ross A. Slotten, *The Heretic in Darwin's Court: The Life of Alfred Russel Wallace*. New York: Columbia University Press, 2004. Pp. viii + 602. US$39.50 HB.

By Peder Anker

With this book, Ross A. Slotten has made a valuable contribution to the growing body of literature about Alfred Russel Wallace (1823–1913). It is a likable and solid biography covering his life from early childhood to death, with an emphasis on the later part of his life. The strength of this book lies in its rich use of primary and archival resources, while the methodological aspect of historical research is less emphasised.

The aim of the book is to undermine "the myth of a lone scientist ultimately triumphing over universal opposition", which dominates historiography about Charles Darwin (p. 1). Though few serious historians of the period believe in this myth, it serves as a nice point of departure in discussing the importance of Wallace. Four major themes emerge in Slotten's analysis of his life.

First, Slotten places Wallace's work in view of class relations within British society. Wallace emerges as an interloper struggling in an uphill battle for acceptance within the upper-class society he was not born into. In the end Wallace chose to identify with his own class, which serves as an explanation for his controversial social as well as spiritualist views. Wallace's defence of spiritualism, Slotten argues, was ultimately a defence of a belief system within his own social class, and his socialism became a defence of his own social group.

Second, Slotten focuses on the importance of Wallace's scientific work for the emergence of the new evolutionary biological paradigm. Following Thomas Kuhn, he argues that Wallace was of equal importance to Darwin in introducing evolutionary principles to

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biology. Even though they would not see eye to eye on the relevance of the evolutionary paradigm in understanding human evolution, Wallace nevertheless deserves credit for his co-discovery of evolutionary mechanisms.

Third, the book shows how Wallace tried to reconcile religion and science. He was reacting to the excessive materialism, which his revolutionary ideas had wrought. Wallace's belief in spiritualism was not only a defence of an intellectual movement of the lower classes, but it also promised a more religiously informed path for biological research.

Finally, Slotten takes the reader through the fieldwork of a natural scientist deeply imbedded in the tradition of botanical collecting in the tropical part of the world. He shows how Wallace arrived at his conclusions from studying plants and animals in the field rather than in the laboratory.

What is perhaps most interesting in this book is how important it was to have a patronage network to succeed in the long run as a scientist. Wallace was, despite his initial scientific success, unable to secure a steady income due to his social and scientific missteps. Socially he was unable to capitalise on his co-discovery of evolutionary mechanisms with Darwin, and scientifically he lost credibility as a believer in spiritualism. Slotten has done a fine job in untangling the difficult financial side of Wallace's life, and its importance for his later scientific work and ideas.

At least four biographies of Wallace have reached the book market in recent years. The first was Peter Raby's *Alfred Russel Wallace* (Princeton, 2001), which pays special attention to the early part of his life. It was followed by Michael Shermer's *In Darwin's Shadow* (Oxford, 2002), which focuses on proving the validity of scientific biographical methodology. The next biography was Martin Fichman's *An Elusive Victorian* (Chicago, 2003), which reads Wallace's scientific work in view of his political and social views. Slotten's book represents the latest culmination in this interest in Wallace. In addition one can also find several republications of primary sources and Wallace readers designed for students. All of this research into the life and work of Wallace raises the question why he, after years of neglect, suddenly emerges as a key scientist worthy of such intense historical investigation. The attention cannot be explained by the discovery of exciting new primary resources. Nor has it anything to do with a jubilee or an anniversary. Instead these new biographies may be understood in view of recent focus among historians on the importance of scientists working on the margins of the scientific mainstream. Together, these books show that even though Darwin was a key figure bringing forth the 'Darwinian Revolution', he was not the only one. Wallace also played an important part in making the shift toward evolutionary biology possible.

Slotten has done an excellent job in presenting the details of Wallace's life. With four competing biographies in the bookstore, one can only hope that this publication represents the grand finale of current Wallace biographies. It is a fine piece of scholarship and well worth the read.

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ART, BODY, SCIENCE

Pamela H. Smith, *The Body of the Artisan: Art and Experience in the Scientific Revolution*. Chicago: University of Chicago Press, 2004. Pp. x + 367. US$35.00 HB.

By Sean Gurd

The significance of the body in recent scholarship of the early modern period can hardly be overestimated. Since Leonard Barkan's flagship work from 1975, *Nature's Work of Art: The Human Body as Image of the World* (New Haven), there have appeared a great number of studies in literary and art-history, which focus on bodily experience, the representation, and the means of control of the body. Pamela Smith brings this large and important body of knowledge to bear on science studies in a way that is remarkable and entirely successful. This is intelligent, historically sophisticated interdisciplinary research at its unmistakable best.

The Body of the Artisan argues that seventeenth-century approaches to natural philosophy drew inspiration and authority from a long tradition of practical knowledge originating not in the studies of philosophers and humanists, but rather in the workshops of craftspeople and artists. As early as the fourteenth century, the artisans of Smith's title had a sense of craft that emphasised embodied, material interaction with nature as a means of producing both beautiful objects and technical knowledge. For the ensuing two centuries these 'artisanal epistemologies' remained outside the elite centres of learning, but in the seventeenth century they began to achieve new legitimacy with the growth of modern modes of scientific inquiry. Smith traces the interactions between artisanal knowledge and the more canonical, recognised sources of natural philosophy through three successive periods and *milieux* (fifteenth-century Flanders, sixteenth-century southern Germany, and seventeenth-century Amsterdam and Leiden).

The most striking aspect of this book is Smith's ability to draw frequent and compelling connections between the 'arts' and the

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'sciences'; of course her point is that such a distinction, at least before the seventeenth century, is not entirely *à propos*. Dürer and Paracelsus are shown to have had much in common, as did Palissy and Agricola. Smith is very successful at showing to what degree the culture of what might otherwise be called 'aesthetics' – painting, sculpture, casting – and the culture of 'early science' – alchemy, early chemistry – had much in common. In the seventeenth century, however, as these practises and attitudes to knowledge began to acquire new prestige and were promulgated by members of the elite, an unease about their origins began to set in. In the end, modern science both embraces and strives to disown the 'embodied epistemology' it inherited from men like Dürer, Rembrandt, and Palissy. The division between the 'arts' and the 'sciences' (quite a different kind of division from the dialectical divisions of knowledge characteristic of high scholasticism) thus appears to be a reflex of social anxiety over the artisanal sources of modern science's legitimacy.

The climax of the book is a detailed reading of the house and possessions of the Dutch physician and natural philosopher Franciscus dele Boë, *Latine* Sylvius (1614–1672). In addition to being a practising physician, Sylvius was one of those responsible for the institutionalisation of experimental science. A professor of medicine at Leiden, he was a major force behind the founding of the first chemical laboratory in a European faculty of medicine. But Sylvius was also a major art collector. By the time of his death his collection of late sixteenth- and early-seventeenth-century paintings was one of the largest in Leiden. Smith works her way through this treasury with patience and a careful, historically disciplined eye, pausing to engage in extended discussions of individual pieces, drawing out their connections to other works in the collection and to broader artistic, scientific, and cultural trends. For Smith, Sylvius's collection reflects an affinity with the artisans from whom his science drew; but at the same time much of his art subtly *critiques* the powers of the senses, which are also seen as a source of great ill (for the passions are excited *via* the senses). In her hands, Sylvius' house becomes an important document in the history of science, a place where the tensions and contradictions arising from empiricism's origins were inscribed in the language of visual art.

Smith offers a wide historical perspective on the history of early modern science: she sees silver workers, painters and entrepreneurial apothecaries as part of the broader system of knowledge exchange and production that led to the emergence of early scientific thought.

But in order to argue, as she successfully does, that there are important connections between artisanal and alchemical epistemologies, Smith needs to be able to move seamlessly and comfortably between art and philosophy, between insightful readings of philosophical texts and in-depth ecphrases of paintings. Her abilities are nicely displayed on the pages of *The Body of the Artisan*. This work should be taken as a model of the interdisciplinarity that characterises the best humanistic research.

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HOLDING THE UNIVERSE BY THE THUMB

Gerard L'E. Turner, *Renaissance Astrolabes and their Makers*. Aldershot and Burlington, VT: Ashgate, 2003 (Variorum Collected Studies Series, 766). Pp. xii + 294. £65 HB.

By Julian Holland

The astrolabe was the embodiment of astronomical knowledge in Medieval and Renaissance Europe. Astrolabes continued to be made and used in the Islamic world until the last century or so but reached their peak of complexity and finesse in Europe in the Renaissance.

The astrolabe is effectively a two-dimensional representation of the universe, like a celestial sphere squashed flat. A number of prominent and easily recognised stars, often thirty or so, are marked on a *rete*, a net or framework, mounted over a stereographic projection of the universe as seen from a particular latitude. Generally there are a number of latitude plates so the astrolabe can be used at different latitudes. The *rete* and plates are mounted in the *mater*, a circular disc with a raised rim divided into 360° and a suspension ring at the top. The various elements of the astrolabe are joined by a central pin, enabling the *rete*, a rule at the front and an alidade at the back to rotate. Astrolabes may be marked with other features such as a shadow square and calendrical information. The instrument could be used for telling the time by observation of the sun or stars. In addition it could be used for making astronomical calculations.

Documentary evidence for technology before the seventeenth century is limited. The physical artifacts are the principal source of information. Much can be extracted by an examination of the physical evidence. Over the last decade or so, Gerard Turner has applied a variety of techniques to extend our knowledge of the production and use of astrolabes.

Renaissance Astrolabes and their Makers brings together twelve papers published in a variety of places between 1993 and 2001. Five

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of the papers were written with a co-author. They are organised to present a logical sequence rather than being ordered by date of publication. The first paper on 'Later Medieval and Renaissance Instruments' sets the scene. By the sixteenth century a variety of instruments were in use for astronomy, navigation, and surveying, and the craft skills required for their manufacture were forming into specialised workshops. In the following papers a recurrent theme is that individual instruments cannot be studied effectively in isolation.

All known astrolabes are being incorporated into an International Checklist (IC) first published by Sharon Gibbs, Janice Henderson and Derek de Solla Price in 1973. This provides a framework for documenting the thousand or so known European and Islamic astrolabes. An unsigned astrolabe of puzzling character (IC 3042) is the subject of Turner's second paper. It bears lettering of two distinct styles which has led to diverse estimates of its age. It was at one time thought to date from the tenth century and came to be known as the 'Carolingian' astrolabe. Others thought it a fake. Turner uses various approaches to conclude that it probably dates from the twelfth or thirteenth century, that it was made by a scholar rather than a craftsman, and that some of the lettering was added at a later date.

In assessing the historical significance of an astrolabe it is useful to be able to date it and so set it into a social and intellectual context. Only a small portion of European astrolabes before 1500 are marked with dates. It had been thought that the date of the Sun's entry into the First Point of Aries (the Spring Equinox) as marked on astrolabes could be used to determine their dates. This feature gradually changes over the millennia. By using the First Point of Aries to calculate the presumed date of twenty astrolabes bearing inscribed dates, Turner shows in the third paper that this technique has no validity. This is a good demonstration of the value of comparative analysis of astrolabes. It also shows the importance of examining a host of constructional details for evaluating astrolabes rather than relying on one technique.

The subject of the fourth paper, an astrolabe (IC 640) presented by Regiomontanus to Cardinal Bessarion in Rome in 1462, remained in Italy until it came into the hands of Dr William Somerville, who gave it to Sir John Herschel in England in the mid-nineteenth century. It was studied in the 1950s and exhibited for many years by the National Maritime Museum in London, while remaining in private ownership. It is only much more recently that it has been possible to place it in context with a group of German astrolabes of the fifteenth century.

An astrolabe of 1537 by Georg Hartman(n) of Nuremberg (IC 262) that appears to have belonged to Galileo is the subject of the next brief paper. This astrolabe can be determined to have been in Florence at an early date because of a plate for the latitude of 43 degrees. This is in addition to the three plates engraved for six latitudes original to the instrument. An examination of the punch marks of this extra plate indicates it came from the Florentine workshop identified as that of Giovan Battista Giusti. Although virtually nothing is known of Giusti, the application of this technique has identified more than twenty instruments, most unsigned, as coming from his workshop, which are treated in the sixth paper. This paper is not limited to astrolabes and again shows the value of critical examination of potentially related instruments in widely scattered locations.

One of the Giusti items is another supplementary 43-degree plate made to fit one of the finest astrolabes ever made (IC 490). This has been confidently attributed to the great cartographer Gerard Mercator. This astrolabe, which seems to have arrived in Florence in conjunction with the elevation of Cosimo de'Medici as the first Grand Duke of Tuscany in 1569, is treated in detail in the following paper, and allied with two more Mercator astrolabes, one bearing his monogram (IC 4608), in the eighth paper.

The concluding papers cover a Tudor astrolabe by Thomas Gemini (IC 425) closely related to an astrological disc produced by Mercator; an astrolabe by Erasmus Habermel; an Elizabethan silver globe by Charles Whitwell; and a cautionary tale about the persistence and consequences of an erroneous date in the scholarly literature.

The sequence of papers thus draws the reader on with a powerful cumulative effect. As befits the study of objects, all the papers are illustrated. The papers are mostly reprinted as they appeared in the original publications. This may account for some variability in the adequacy of the illustrations in showing detailed features of punch marks and engravings discussed in the text. One paper refers to a colour plate not reproduced here and other coloured illustrations in the original are reproduced in black and white. Given the number of instruments discussed in the twelve papers, though, the extent of illustration is very generous.

As a 'Variorum' edition, these papers come from a range of publications. In many cases the assiduous user of academic libraries could track down the original components of such a volume. In this case it is especially welcome to have the papers brought together. Some come from less available sources, including an auction catalogue. Two of the papers have been reset, one being a revised version of the auction catalogue entry. Apart from these the original pagination has been retained enabling citations to be made consistent with original publication. Curiously, a paper originally published in *Annals of Science* has a silently interpolated footnote. The volume also has a preface and an index.

For students of the early modern period unfamiliar with astrolabes and the other types of scientific instrument produced at the time, this book offers many interesting insights. These astrolabes tell us about astronomical knowledge, methods of metalworking, relationships of patronage, the transmission of ideas, the divisions of time and other factors of social and economic life that are often complementary to such written records as survive. While the reader will need to turn to other sources for a straightforward account of the way in which astrolabes were used, *Renaissance Astrolabes and their Makers* opens the way to a richer understanding of Renaissance Europe and to further detailed examination of its material culture of science.

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REVIEWS

THE MAN OF SCIENCE

Paul White*, Thomas Huxley: Making the ''Man of Science''.* Cambridge: Cambridge University Press, 2003. Pp. 205. £16.95 PB.

By Sally Shuttleworth

Taking his cue from the literary scholar Stephen Greenblatt's work on Renaissance 'self-fashioning', White explores the ways in which Thomas Huxley sought to define himself as a 'man of science'. As late as 1894, Huxley had written to the editor of the newly-founded magazine *Science Gossip* to complain about the use of the 'vulgar' term 'scientist', which to his eyes had connotations of mere technological practitioner, and did not carry the moral gravity or sense of social responsibility of his preferred term, ''man of science'' (p. 1). White's book focuses on the construction of Huxley's scientific identity, and his engagement with a range of cultural and social networks. Building on previous work in this area, particularly Adrian Desmond's major biography of 1998, he brings a new orientation, stressing continuities rather than conflict, and questioning the widely-held belief that Huxley's career epitomised that of a new breed: the professional scientist.

The book is divided into thematic sections, dealing with gender ideology, his relations with Darwin and Owen, Science as Culture, religion and educational activities, and his social campaigns of the last decades. These map, very roughly, onto a chronology of his career, but such divisions inevitably cause some problems in the arguments. The first section, ''Science at Home'', is perhaps the least convincing, although it draws on some fascinating material in Huxley's correspondence with his future wife, Henrietta Heathorn. White tends to assert his argument, in this case that Huxley identified ''scientific work with the pure and often feminized domestic sphere'' (p. 8), long before offering substantiating evidence. Whilst it is clear that Huxley did wish to distance himself from the corruption of

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market-based work, his model for his own labours seems to have been more that of the gentleman scholar than feminine domesticity. Class is addressed in the following chapter, but unfortunately it is separated off from the gender-based analysis offered here: the complex interweaving of class and gender issues needs some exploration. There are indeed some intriguing passages quoted, including "man is as clay in the hands of the father – woman" (p. 19), but the imagery and nuances of the language tend to remain unexplored. Huxley continued to invoke highly gendered language throughout his career, as in his opposition to William Booth's emotional rhetoric as a "dwarfing of manhood" (p. 137), and his constant grouping together of parsons and women as opponents of science, but regrettably considerations of gender are largely confined in this work to the first chapter.

'Gentlemen of Science? Debates over Manners and Institutions' offers a very helpful reconsideration of Huxley's relations with the scientific community, focusing primarily on his interactions with Darwin and Owen. Owen himself is rescued from the savage portrait, painted by Huxley, of a man corrupted by power and self-interest, and his early support for the younger man, which Huxley omitted from his own accounts, is emphasised. Huxley's attacks on Owen and lionising of Darwin should be seen, White suggests, less as personality sketches and more as "prescriptions for scientific manners and institutions" (p. 60). White draws out the irony of the fact that Darwin, celebrated by Huxley for his aloofness from institutional structures, was in reality made deeply uneasy by the public (ungentlemanly) manners of his supporter. The chapter helpfully underscores the ways in which Huxley's attacks on Owen and supportive reframing of Darwin were linked to his role in shifting the focus of scientific practice from the museum to the laboratory.

Literary anthologies for the nineteenth century now frequently carry extracts from Huxley's science writing, in addition to the classic exerts usually given which profile his exchanges with Arnold, as forerunners of the twentieth-century 'two cultures' debates of Leavis and Snow. In 'Science as Culture' White does not offer a 'literary' reading of any of Huxley's texts, but he does rewrite our understanding of the Huxley–Arnold exchanges, stressing less their antagonism than their shared agenda in opposing denominationalism and *laissez faire* approaches to education. Their alliance is highlighted symbolically by Huxley's invitation to Arnold to speak at the 1868 meeting of the Geological Society of London, at which Huxley was to be installed as president (p. 88). Whilst continuing to criticise each other's position publicly, both offered, White suggests, variants upon a 'one culture' model of education produced from within the elite environs of the Athenaeum Club. Ideas of scientific imagination, he argues, also played a role in creating this mutually confirming domain of 'Science and Literature', where science could draw on the cultural authority of literature. The argument here is rather sketchy, however, and could do with more precision of language ("literary imagination with its fancy for the concrete" [p. 96], for example, unwittingly and confusingly evokes Coleridge's classic distinction between imagination and fancy).

The strongest section of the book focuses on Huxley's relations to the religious establishment, and his educational activities. So powerful is his image as the 'devil's disciple' it comes as something of a shock to trace his close relations with liberal Anglicans such as Charles Kingsley, and to hear that as an elected member of the London School Board he had proposed that the Bible should be taught as part of "religious culture" (p. 125). (He afterwards rather marred this effect by proposing that only passages approved by the Board be employed, to avoid the selection of ethically or scientifically unsuitable material). White outlines the forms of professional accommodation practised by liberal clergy and scientists in their interactions, suggesting at one point that for Huxley and Kingsley, agnosticism became a "form of sociability" (p. 119). In stressing the non-threatening nature of Huxley's adoption of religious forms, however, White rather overstates his case. Did Huxley create his provocatively titled *Lay Sermons* in a spirit of conciliation? The outcries against Huxley's materialism receive virtually no airing in this chapter.

The final section on 'Darkest England' and Huxley's interventions in the social debates of the 1880s and 1890s offers a very different picture. Here, in his scathing attacks on William Booth and the Salvation Army, on Gladstone's policies of Home Rule, and on proposals for land nationalisation, Huxley is portrayed as a violently polemical figure, but also an unsuccessful one, who has lost touch with his audiences and started to identify with the commercial and industrial system he had earlier attacked. The rise of a labour press, and a new culture focused on workers' concerns led, White suggests, to Huxley the self-appointed moral reformer and friend of the working classes being castigated as a member of the selfish ruling classes. Although White is right to suggest that the social and cultural climate was changing, he probably overstates both the shift in Huxley's views and his placement in public perceptions. The onslaught on Booth, for example, partakes of the spirit of his earlier indictment of Comte's despotic system as 'Catholicism minus Christianity', whilst the attack on Huxley by "a writer Robert Buchanan" as someone who "upheld the tyrannies of force and convention" (p. 165) needs to be put in context. I assume this was the same Robert Buchanan who had made a career out of attacking the doctrines of 'materialism', whether in the writings of 'fleshly poets' or scientists such as Tyndall and Huxley (see Dawson, "Intrinsic Earthliness: Science, Materialism and the Fleshly School of Poetry"(*Victorian Poetry*, 2003)). It was part, then, of the ultra-conservative criticism that Huxley's work had always evoked. White highlights Huxley's adoption in the late 1880s of a "new rhetoric of continuous and inevitable war" in place of his earlier emphasis on "self-help and improvement" (p. 151), although it is worth noting that his scathing opponent at the *Pall Mall Gazette*, W. T. Stead (p. 154), was to employ far more bellicose language than Huxley in his editorials in the *Review of Reviews* of the 1890s in his emphasis on international industrial competition.

White concludes his book with a re-interpretation of Huxley's final public address, *Evolution and Ethics*. He suggests that in separating ethical evolution from the "cosmic process" of ceaseless struggle, Huxley responded to his perceived loss of moral authority as a "man of science" by cutting "the links between nature, morality, and scientific practice" (p. 173), thus bringing into being "a new persona, the modern scientist" (p. 169). Huxley thus "surrendered the moral commitments that had always been fundamental to his vocation, and embraced what would become a standard distinction between 'fact' and 'value', between science and its social use" (p. 173). I find it hard to see the impassioned language of *Evolution and Ethics*, with its fierce onslaught on the culture of individualism, which goes back to his attack on Spencer in *Administrative Nihilism* of 1871, as a surrender of moral commitment. Huxley envisaged "no limit to the extent to which intelligence and will, guided by sound principles of investigation, and organized in common effort, may modify the conditions of existence" (*Evolution and Ethics*, London, 1893, p. 36), and surely he placed the "man of science" at the forefront of that effort?

Although I do not always agree with the conclusions reached, White's work opens up new ways of thinking about Huxley's career, particularly in its explorations of his cultural networks and some of the more neglected aspects of his social campaigns. *Thomas Huxley*:

Making the "Man of Science" shows there is always room for a new, well-written and researched book on this indefatigable and multi-faceted Victorian.

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BOOK NOTICES

Cong Cao, *China's Scientific Elite*. London and New York: Routledge Curzon, 2004. Pp. xv + 256. £60.00 HB.

The Chinese Academy of Sciences (CAS) is the flagship of China's scientific research. It is a government-controlled institution, similar in structure and purpose to the former Soviet Academy of Sciences. Comprised of dozens of research institutes, the CAS has a research staff of 45,000. Its primary function is to implement science policy and propel China's scientific enterprise. However, the CAS also functions as a society or academy of fellows, in the mode of the Royal Society or the National Academy of Sciences of the United States. Becoming a member (or academician) of the CAS is the highest honour and recognition that a Chinese scientist can receive in his or her profession. Over the past fifty years, only about 1000 Chinese scientists have been elected to be CAS members.

Cao's sociological study of CAS memberships is the most comprehensive analysis of China's scientific elite to date. Following primarily the tradition of Robert Merton's study of the scientific community, Cao analyses the memberships in relation to a set of well-chosen social factors, such as family background, education, institutional affiliation, geographic location, the role of elite mentors, personal relations, party membership, political background, and area of specialisation. Together, these analyses present a solid institutional history and sociological survey of China's scientific establishment. (FF)

Allen G. Debus (ed.), *Alchemy and Early Modern Chemistry: Papers from Ambix*. Huddersfield: Jeremy Mills publishing for the Society for the History of Alchemy and Chemistry, 2004. Pp. xv + 543. £33.00/US$60.00 PB.

As Allen Debus reminds us in his introductory essay, *Ambix*, established in 1937, is one of the pioneering journals in the history of

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science, being the organ of the Society for the History of Alchemy and Early Chemistry. Moving with the times, the Society was later to drop the word 'Alchemy' from its name, so that *Ambix* has for some time now been essentially a history of chemistry journal – but one that reveals its ancestry by its name and by some of the articles on the history of alchemy that it continues to publish. Among the small group of founders were such well-known scholars as E.J. Holmyard, D. McKie, J.R. Partington, and F. Sherwood Taylor, who between them made the history of chemistry the leading branch of the history of science studies in Britain. McKie headed the principal department in the country at University College London in the 1950s, from which a good many of its former students moved on to successful academic careers.

The present collection is evidently intended to show the wares of *Ambix* to the world; and I believe it does so most successfully. Readers will find conveniently within a single pair of covers twenty-eight papers by twenty-four authors of note (M. Baldwin, N.H. Clulee, A.G. Debus, B.J. Teeter Dobbs, N.R. Gelbart, D. Geoghegan, B. Heineke, J.P. Herschbell, C.H. Josten, B.T. Moran, W.R. Newman, D.R. Oldroyd, W. Pagel, J.R. Partington, J.C. Powers, L.M. Principe, P.M. Rattansi, G. Rees, J. Ruska, H.J. Sheppard, F. Sherwood Taylor, L. Thorndyke, M.T. Walton, and R.S. Westfall). When the writer of this notice was working in the field of alchemy (actually at its edge only), he found the works of Debus, Pagel, Teeter Dobbs, Partington, and Rattansi specially helpful to his understanding, but most of the above came in his sights. Within the present collection, some of the main themes are: the origins of alchemy; the social situation of alchemy, including its courtly patronage; mathematics and alchemy (as in John Dee) and the quantification of alchemy/chemistry; the borderland between alchemy, chemistry, the mechanical philosophy and mineralogy; Robert Fludd; Paracelsus, van Helmont and iatrochemistry, and the Helmontians; Walter Charleton; alchemy and Jesuits; the alchemical interests of Boyle and Newton; and the eventual transmogrification of alchemy into chemistry. Some contributions on Becher and Stahl, and the relation of alchemy to phlogistonism, would have been welcome, but that would have taken the collection somewhat beyond its chosen time-frame. (DRO)

Jonathan Gil Harris, *Sick Economies: Drama, Mercantilism, and Disease in Shakespeare's England*. Philadelphia: University of Pennsylvania Press, 2004. Pp. 263 US$49.95, £32.50 HB.

In what is first and foremost a contribution to literary criticism and has no pretensions to be a history of medicine, Harris looks at the relationship between (metaphors of) pathology and the early history of economic theory as found in Shakespeare's plays. He does not, however, limit himself to Shakespeare, dissecting other sixteenth and early-seventeenth century literature, as well as the economic writings of Gerard Malynes, Thomas Milles, Edward Misselden, and Thomas Mun.

Thus, Harris is able to elaborate a sophisticated argument concerning the *mise-en-scène* of mercantilism based on seemingly endless snippets from plays by Shakespeare and his near contemporaries. A typical argument that he presents in relation to Thomas Middleton and Thomas Dekker's *The Roaring Girl*, is that "early modern economic usages of "consumption" or "to consume" almost invariably carried a pathological freight" (p. 167). Thus, the weft of the early modern economy and the warp of the pathological body politic are woven together. Although interesting for those who want a modern take on sixteenth and seventeenth century English literature with respect to the use of pathological metaphors, it is not for the theoretically faint-hearted. In his exposition of pathology, plays and mercantilism, Harris mobilises such notable postcolonial theorists as Homi Bhabha and Arjun Appadurai, and is not afraid to deploy the vocabulary of 'globalisation'. Thus, his claim that "[i]f transnational economy in *The Merchant of Venice* has an explicitly romantic accent, it also has a more occulted pathological one" (p. 11) serves as an illustration of the general tenor of the book. (JS)

Nicholas Rescher, *On Leibniz*. Pittsburgh: University of Pittsburgh Press, 2003. Pp. x + 252. £26.95 HB.

Rescher's latest book on Leibniz reinforces his credentials as one of the most engaging and prolific of Leibniz scholars. He is also one of the longest serving and most philosophically astute of Leibniz's commentators. This collection of eleven essays, all previously published, ranges from detailed expositions of the theory of possible worlds and Leibniz's monads to biographical studies of the great philosopher's life. The essays were written from 1977 to 1999. The discussions of Leibniz's philosophy include Leibniz on possible worlds and the contingency of the world, a paper on the relations between monads, the plurality of space–time frameworks, inductive reasoning and Leibniz's concept of a system. Rescher is not immune from making sweeping claims, such as his statement that "Leibniz was the first thinker to describe himself as having a system of philosophy" (p. 106), but he writes with such enthusiasm for his subject that such generalisations are inoffensive. The historical studies at the end of the collection balance the analytical discussions that precede them and the result is a highly readable volume, suitable for students and scholars alike. (PA).